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CONTENTS

No. 1, March, 1937

No. 2, June, 1937

No. 3, September, 1937

No. 4, December, 1937

VOL. 12, NO. 1 March, 1937

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WAS ARISTOTLE AN EVOLUTIONIST?

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A CRITICAL re-examination of this question has been prompted by the large differences of opinion that have been found to exist among the historians and commentators on whom the reading public has been accustomed to rely for its own opinions of Aristotle. His extraordinary influence on the course of western civilization—an influence still very much alive, often conspicuous, in the intellectual life of today—gives his views on any subject peculiar importance. Doubly desirable is it to be clear as to his attitude toward so significant and pervasive an influence in the modern world as the evolution idea. This has provided the motive for the present attempt to find, in spite of current misunderstandings, whatever unity the facts themselves may warrant. To reach this goal, it has seemed profitable, first, to consider the senses in which the term evolution is commonly used; next, to summarize contemporary opinion; finally, to let Aristotle speak for himself.

I

Among the many meanings that have been given to the term evolution since its introduction into biology in 1762 by Charles Bonnet (24), there are at least four that may well be kept in mind as we pursue our present enterprise:

Evolution as the development of the individual. This was as Bonnet used it. But he conceived development, on philosophical grounds, as an unrolling or unfolding of a germ that contained, and had contained from the instant when germs were first created, the counterpart in miniature of every characteristic destined to appear in the adult. It was thus a mere extension of a pre-formed organization by a sort of interstitial swelling of pre-existing elements. The term evolution was chosen originally to fit this particular conception of individual development. With the rise of embryology, however, in the first half of the nineteenth century and the total eclipse of Bonnet's special views, it was transferred from the latter to the more general case, becoming easily identified with the "succession of gradual

changes" by which the egg "evolved" into the adult, whatever the underlying mechanism.

"These successive changes of growth," wrote Louis Agassiz (3), just before he died, "constitute evolution, as understood by embryologists, and within these limits all naturalists who know anything of Zoology may be said to be evolutionists. The law of evolution, however, so far as its working is understood, is a law controlling development and keeping types within appointed cycles of growth, which revolve forever upon themselves, returning at appointed intervals to the same starting-point and repeating through a succession of phases the same course. These cycles have never been known to oscillate or pass into each other."

Evolution as the development of the race. This was to Agassiz, writing in 1873, a "recent and novel application" of the term *evolution*. As early as 1852 Spencer (53) had used Bonnet's term in this new sense. While the last word in the first and every subsequent edition of the *Origin of Species* (27) is "evolved," it is not until the sixth edition, finished in 1872 and published ten years later, that Darwin extends this meager usage by mentioning "evolution" three times in one paragraph of the concluding chapter. It was Darwin's custom to speak of "descent with modification" when he wished to recall that races, like individuals, change as they develop, and that racial types are not of necessity permanent but show variations that are perpetuated, accumulated, modified, from generation to generation. His expressive phrase, however, with its explicit recognition of the genetic factor, was soon to give way to its less colorful and definitive successor. When Agassiz wrote, *evolution* had become familiar to English ears in its new meaning.

Evolution as the development of the cosmos. This application of the term *evolution* in its most inclusive and general form was also, in its beginnings, Spencer's doing (55, i, 201 ff). From boyhood, in his contemplation of nature, he had sought interpretations untinged with supernaturalism. When, therefore, in 1840, he first met Lamarck's views on the mutability of species in the pages of *The Principles of Geology* (38), he found in them a "congruity with the course of procedure throughout things at large" that gave them, in spite of Lyell's unfavorable comment, "an irresistable attraction." Two years later, in a series of letters to *The Non-Conformist* on *The Proper Sphere of Government*, he showed a recognition of "certain factors in the process of evolution at large." The reflections thus initiated led to *Social Statics* (1850), and this to *The Principles of Psychology* (1855). Then, early in 1858, in a mind gradually saturating with such studies, there suddenly crystallized an outline of the *System of Synthetic Philosophy*. This was only a few months before that memorable meeting of the Linnaean Society when Darwin (28) broke his long silence and *The Origin of Species* at last was born.

There is no need to pause here over the abstract terms of the well known definition of evolution which Spencer (54) now applied as a basic formula to Nature in all its manifestations—inorganic, organic, and, as he said, superorganic. It was essentially a philosophic summary, a least common denominator; reminiscent, too, of kindred speculations by the naturalists of ancient Miletos who first expressed the conception of a mechanical, as well as a changing, universe that made natural science possible. In so general a form it was intended to include but not to replace the more restricted conceptions of evolution which, as has already been indicated, were associated with various special aspects of the general problem.

Evolution as the development of human culture. Under this head fall naturally

the manifold results of human activities, such, for instance, as: social, political, religious institutions; languages; the arts and sciences, with their applications to architecture, industry, medicine, agriculture, domestic life. All these may be studied as to their development and evolution. But the words now take on new meanings, with the shift of interest from organic processes to their products, from man himself to that not inconsiderable portion of his environment which he has fashioned and in which he is forced to live. Change in this vast artefact is evolution of a sort, but without hint of organic reproduction. Close as may be resemblances between cathedrals, or airships, or philosophies, it is as the handiwork of man the artificer that they are related, not by anything remotely suggesting Darwinian descent with modification. Of this Spencer was fully aware when, in his Prospectus (54: xvi) distributed in 1860, he specifically characterized as evolution the development of governments, languages and ideas.

II

Was Aristotle an evolutionist? Since the convenient and accessible outline of the history of the evolution idea published forty years ago by H. F. Osborn (43), there have been numerous answers to this question, with a variety of interpretations amounting in extreme cases to diametrically different conclusions. These will be examined in the present section, as a preliminary summary of current opinion.

Osborn himself believed that Aristotle "was the first to conceive of a genetic series, and his conception of a single chain of evolution from the polyps to man was never fully replaced until the beginning of this century" (p. 44). Farther on, he arranged in five sections adaptations of several passages from Book 2 of the *Physics* as translated by T. Taylor (5). In the last of these, taken from Chapter 8, are the following words: "*Further still, it is necessary* (*i.e.*, *according to law*) *that germs should have been first produced, and not immediately animals; and that soft mass which first subsisted was the germ.* . . Nature produces those things which, being *continually moved by a certain principle contained in themselves*, arrive at a certain end" (p. 56). His conclusion, following immediately, is of especial significance: "These passages seem to contain absolute evidence that Aristotle had substantially the modern conception of the Evolution of life, from a primordial, soft mass of living matter to the most perfect forms, and that even in these he believed Evolution was incomplete, for they were progressing to higher forms" (p. 57). Osborn refers here to an "'internal perfecting tendency', driving organisms progressively forward into more perfect types" (p. 51).

Certain comments at once suggest themselves. In the first place, as has already been indicated, one may arrange objects in a series designed to emphasize their resemblances without thereby committing himself to a belief in their blood relationship. There could be, obviously, no blood relationship between golf balls, however minute their differences; or between locomotives, as Lewes (34:189) long ago remarked. Such an inference would be hardly less superfluous in the case of organisms, in the absence of a statement expressly to the contrary—which has thus far eluded us. Whether or not he ever gave it a thought is a question that will be considered later. For the present, it seems necessary only to say that if his series, whose terms are coexistent in time, is to be regarded as in any sense a genetic sequence, this conclusion must rest on evidence other than the mere existence of the series itself.

Similar reasoning is applicable to the assumption that Aristotle's well known belief in an 'internal perfecting tendency' in nature carries with it a belief in the transmutation of specific types.

Finally, it should be said that whatever may have been Aristotle's views on the "Evolution of life, from a primordial, soft mass of living matter," the text of Book 2 of the Physics does not reveal them. The words that Taylor rendered "that soft mass which first subsisted" are the words, not of Aristotle, but of Empedocles. In the Greek text translated by Wicksteed and Cornford (6), they appear in quotation marks: '*oulophues men prota*,' and their derivation is expressly indicated in this rendering by the introduction of the name of Empedocles, thus: "Empedocles' 'primal all-generative'" (199b, 9). They come from one of Empedocles' Fragments, to be found in Fairbanks (30) in both Greek and English as No. 262, and in Burnet (25) in English as No. 62. Fairbanks translates them: "In the first place . . . forms grown into one whole:" (p. 191) and Burnet: "Whole-natured forms first" (p. 249); and more recently, Hardie and Gaye (9), following Burnet: "whole natured first." None of these translations suggests the "soft mass" of Taylor, nor does the Greek text embody this or any other description of the physical character of the *sperma*. Furthermore, Aristotle's quotation of Empedocles' phrase is merely incidental in a chapter that is concerned with a discussion of *purpose in nature*, not at all with theories of the origin and evolution of life.

E. Clodd (26), who appears to have been indebted exclusively to Osborn for his account of Aristotle's "pioneer work in evolution," recurs to "the question of the origin of life forms" with the assertion that Aristotle "was nearest of all to its modern solution, setting forth the necessity 'that germs should have been first produced, and not immediately animals; and that soft mass which first subsisted was the germ'" (p. 18). There is no need to comment further on this misconception.

W. A. Locy (35), a decade later, writes as follows: "Professor Osborn in his interesting book, *From the Greeks to Darwin*, shows that Aristotle had thought out the essential features of evolution as a process in nature. He believed in a complete gradation from the lowest to the highest, and that man is the highest point of one long and continuous ascent" (p. 12).

The words: "thought out the essential features of evolution as a process" are Osborn's (p. 50). Osborn had just been discussing "Aristotle's conception of movement, as outlined in his Physics" (Bk. 3, Ch. 1). His account appears to be taken in part from Zeller (65). But neither in Zeller nor in the Greek text (6) nor in the translations of Taylor (5), Wicksteed and Cornford (6), and Hardie and Gaye (9), is there the suggestion of a primarily biological motive for the passage or a basis for the alternative meanings which he has supplied, in biological terms, for the four kinds of movement there characterized—with the possible exception of the first.

Further consideration of this reference to Aristotle's views will be found in the literature already cited. It should be said that Locy (36), during the seventeen years following the publication of his first book, found reasons for restating his view with much subdued conviction: "In the philosophical consideration of animal life, Aristotle possibly glimpsed the germ of the idea of evolution" (p. 32).

This was some years after Royce (48) had written his illuminating essay on Herbert Spencer in which he said: "Aristotle did indeed lay great stress upon the

evolution everywhere present in the sublunary region of 'genesis and corruption', but in this region it was each individual thing which grows and then passes away. The 'forms' which are responsible for the evolution of individuals are as eternal as the Platonic ideas. They therefore do not evolve" (p. 28). And again: "This conception of the *eternity of the forms of things* is, historically considered, by far the most significant opponent that the philosophical doctrine of evolution has ever had or ever can have" (p. 29).

Arthur Platt (14) makes an interesting note—here reproduced only in part—on the discussion by Aristotle of the possible 'earth-born' origin of men and quadrupeds: "I think it certain that the transmutation of species in any modern sense no more occurred to him than to Empedocles. He contemplates the possibility that man's ancestor was a scolex; he never thought that he might have been a monkey. Each species would have had a separate beginning by spontaneous generation; they would not be related by descent from a common ancestor" (762b, 29, note 2).

T. E. Lones (37), commenting on the statement of Aristotle (19) that "Some animals share the properties of man and the quadrupeds, as the ape, the monkey and the baboon" (502a, 17) says: "Neither in this nor, apparently in any other passage does Aristotle show that he had any idea of a development of higher forms of life from common ancestors, at all resembling the Darwinian idea of the origin of species" (p. 82).

Two years later, A. W. Benn (23) inclusively and crisply asserts:

"He did not, like the Ionian physiologists, anticipate in outline our theories of evolution. He held that the cosmos had always been, by the strictest necessity, arranged in the same manner; the starry revolutions never changing; the four elements preserving a constant balance; the earth always solid; land and water always distributed according to their present proportions; living species transmitting the same unalterable type through an infinite series of generations." (p. 272)

Charles Singer (51) does not believe it can be claimed that Aristotle "regarded the different kinds of living things as actually passing one into another but there can be no doubt that he fully realized that the different kinds can be arranged in a series in which the gradations are easy" (p. 29). However, he says in a later publication (52):

"It is probable that in ascribing to animals certain human qualities, Aristotle was influenced by his advance toward what would nowadays be called a belief in Evolution. . . . It cannot be said that he ever definitely attained to the 'evolutionary' point of view. But it is evident that he was moving in that direction, and perhaps if he had lived another ten years he might have reached it. But, whether we call him an Evolutionist or whether we deny him that title, it is yet quite easy to read an evolutionary meaning into some of his biological writings. To do this is to develop but not to force his meaning" (p. 39).

Less tentatively, *G. Sarton* (49) writes that Aristotle "outlined the theory of evolution (scala naturae) and a scientific classification of animals" (p. 128).

A. E. Taylor (57), however, believes that Aristotle "turned his back on evolutionary ways of thinking" (p. 144). Why, he asks, was Aristotle "content with applying the notion of biological development to the growth of the individual. Why did he not extend it to the kind?" (p. 446).

H. H. Newman (41) appears to have thought that he did. For in Newman's opinion, the

"evolution idea took a great step forward with Aristotle and reached a stage beyond which it did not go for many centuries . . . He . . . attributed all evolutionary changes to natural causes. . . . He 'had substantially the modern conception of the Evolution

of life, from a primordial soft mass of living matter'. . . . He had an idea of a linear phylogenetic series, beginning with plants, then plant-animals, such as sponges and sea anemones, then animals with sensibility, and thence by graded stages up to Man" (p. 13).

These passages, reminiscent as they are, call for no further comment at this time—unless it be the observation that after more than thirty years it is to Osborn and not to Aristotle himself that writers of widely read textbooks continue to acknowledge their indebtedness for materials that Aristotle alone could have known at first hand.

E. Nordenskiöld (42) is, with one exception (Singer, 52) the most recent of the authors who have helped us to determine Aristotle's position as an evolutionist in the current of informed opinion. Under the caption: "The first evolutionist," he writes:

"Here we find enunciated for the first time a really complete theory of evolution. . . . Aristotle saw a consistent evolution from lower to higher forms of being, and although it is based on purely metaphysical speculation, this idea has proved for all time a fertile one in the biological sphere, for the very reason that it is here in agreement with actual fact" (p. 37). Again: "The finest merit of this system of thought lies in the fact of its being based on an evolution subject to rigid laws and proceeding from the lower to the higher. But as this theory of evolution is, as has been shown above, primarily based on a predominant guiding intelligence, it acquires a dogmatic arbitrariness: the subjection to law is not an act of nature itself, but rather the product of divine wisdom, or, in other words, human speculation. . . . Thus it came about that . . . the man who was the first to introduce and logically to apply to the conception of the entire universe a theory of evolution from the lower to the higher appeared fifteen centuries later as the founder of a system of stagnation and obedience to authority." (p. 43)

Inadequate as these quotations are, they reveal once more an association of the evolution idea with a *scala naturae*. But they leave one in some doubt as to whether the theory of evolution which, according to Nordenskiöld, Aristotle applied logically to "the entire universe" may not be rather a principle of logical classification than a recognition of causal succession in time.

This ends our summary of current opinion.

What, now, does Aristotle say for himself?

III

Two centuries before Aristotle was born, the speculative foundations of science were laid by the hardy school of natural philosophers that Thales founded at ancient Miletos. It is not necessary here to try, out of the fragmentary records of that day, to give definite shape to individuals or the authority of personal reference to their views. What Thales, Anaximander, Anaximines and their successors throughout the Greek world contributed as a whole to cosmology during the sixth and fifth centuries before Christ will provide a general background for the views of Aristotle with which we are especially concerned (See esp. Fairbanks, Burnet, Bailey, Benn).

First of all, they conceived the universe as a self-contained mechanism, without design or purpose, ruled by necessity from within, not by a Divinity or other agency from without. So doing, they broke boldly from the past, initiating a profoundly significant revolution in human thought. They conceived also the universality of change, and a fundamental substance infinite in extent, out of which the multiform world of their experience had differentiated, out of which indeed countless worlds had arisen and decayed through infinite time, in cycles that carried throughout the universe the conception of recurrent change so familiar to their mundane experience.

Thus, in sweeping speculations, they

sketched a developing universe, made mechanically intelligible by the partition of the basic substance into ultimate particles, which varied in size and shape but were eternal and indestructible as the stuff of which they were composed. They were also inherently active, capable of moving in all directions in a structureless void. It was from the movements of these atoms, governed by necessity but without purpose or plan, that aggregates, infinite in number and variety, shaped themselves into earth and sun and moon and stars and all other objects whatsoever, great and small.

Aristotle was not unfamiliar with these evolutionary views. It was not from ignorance but from choice that, under radically different influences he devised a radically different cosmos. Long before his birth Miletos had ceased to exist as a center of philosophical culture. Educated as a boy at the Macedonian court in the medical tradition of his father Nicomachus, he was sent as a matter of course at seventeen to Athens to study philosophy at the Academy, where he began what proved to be an intimate association for twenty years with his master Plato. There, during the latter's absence in Sicily, he fell under the potent charm of the earlier Plato of the Dialogues. There for twenty years he breathed an atmosphere charged with Platonic forms, final causes and teleological interpretations. There he lived in a Platonic world of imperfections, the shadows and images of the eternal realities that existed, detached and perfect, in heaven alone.

His *cosmology* was a product of this early Platonic period, when he still wrote as a true disciple of the master, before an increasing preoccupation with the tangible problems of the sublunary world of experience determined the final focus of his interests. The time came when he criticized Plato; but whatever his criticisms, he remained, in comparison with his Ionian forbears, a Platonist to the last. For him as for Plato, the eternal heaven (*De Caelo*, 283b, 26; 287b, 25) was the abode of divine perfection (*De Part. An.*, 644b, 21). It encompassed a finite universe (*De Caelo*, 273a, 5; 276a, 18) in the form of a sphere (286b, 10) of all forms uniquely capable of eternal motion (*De Gen. et Corr.*, 337a, 1; 338a, 18ff) and with definiteness (*P. A.*, 641b, 23) demanded by a world in which "Nature always strives after the better" (*G. C.*, 336b, 27; *De Incessu*, 704b, 15; *De Juv. et Sen.*, 469a, 29). Embedded in this outermost sphere were spherical stars (*De Caelo*, 289b, 31ff; 290a, 8; 291a, 27; 291b, 11); and within it, other spheres for the other heavenly bodies; and between these still other spheres whose function it was to account for certain apparent motions of the bodies themselves (291a, 30ff; *Metaph.*, 1074a). All the spheres were of ether, the finest of the elements as it was also, from the earth, the most remote. All were in contact. And they revolved regularly and invariably through endless time around the earth, which was fixed and motionless at the center (*De Caelo*, 286a, 21; 286b, 9; 296b, 21). Thus Aristotle conceived a universe that was uncreated, changeless, eternal. Its material substrate was a continuum (*Phys.*, 216b, 20ff), in which the four (five) elements existed as fundamental substances, not ultimate atomic particles (*G. C.*, i, 8; *De Caelo*, 303a, 3ff). And motion, instead of emanating from the latter, was communicated to the outermost of the spheres by an unmoved Prime Mover, without substance or mechanical support (*Phys.*, 267b, 15; *Metaph.*, 1072a, 24; 1074a, 35) existing apparently only as a metaphysical device (*G. C.*, 337a). Thence, from sphere to sphere, motion proceeded to the sub-

lunary world, whose individuals exhibited the well-known changes of growth and decay with their attendant variations and material imperfections not observable in the distant and inaccessible heavens (*De Caelo*, 269b, 16). Form, however, was to Aristotle more significant than matter (*P. A.*, 640b, 29; *Ph.*, 193a, 30; 194b, 10; 200a, 32; *Metaph.*, 1029a, 5; 1041b, 8); formal and final causes were dominant factors in the cosmic mechanism. Sublunary individuals might change materially, but their forms—including their specific types—were fixed and without beginning or end (*De Gen. An.*, 731b, 35; *De An.*, 415b, 3).

It is clear that such a universe, rigid, uncreated and immutable, left no opportunity for the ceaseless differentiation of which the plastic universe of the Ionians was capable. Whatever Aristotle's understanding of the ways of terrestrial nature, it does not seem possible, by the farthest stretch of the imagination, to reckon him among the cosmic evolutionists.

In the sublunary world, indeed, he was more at home. It was tangible and could be investigated objectively by the observation of individuals, to which he turned more and more in the last great productive years of his life. In the *Meteorologica* (see especially i, 14) he discussed *changes in the earth's surface* as incidents in the development and decay of its parts analogous to similar cycles in the lives of organisms. Among these terrestrial changes, the phenomena of sedimentation and erosion especially interested him. He regarded the whole country of Egypt as "a deposit of the river Nile" (*Meteorol.*, 351b, 28). He called attention to the fact that

"in the time of the Trojan wars the Argive land was marshy and could only support a small population, whereas the land of Mycenae was in good condition (and for this reason Mycenae was the superior). But now the opposite is the case, for the reason we have mentioned: the land of Mycenae has become completely dry and barren, while the Argive land that was formerly barren owing to the water has now become fruitful. Now the same process that has taken place in this small district must be supposed to be going on over whole countries and on a large scale" (352a, 9–16). "But the whole vital process of the earth takes place so gradually and in periods of time which are so immense compared with the length of our life, that these changes are not observed, and before their course can be recorded from beginning to end whole nations perish and are destroyed" (351b, 8).

These passages have a modern sound. With all his interest in sedimentation, however, Aristotle seems never to have understood, as his predecessor Xenophanes did, the nature of fossils. And since he could not have known the records left by successive organic types and only recently brought to light by the combined efforts of many palaeontologists, his imagination remained quite untouched by the vast panorama of geologic history that is a portion of our present inheritance.

Of *gradual transformations in human institutions* through long periods of time, he was made aware by the intensive historical studies of his later years. He collected 158 constitutions of ancient states for empirical study, with the coöperation of members of his group, especially Theophrastus (*Eth. Nicom.*, 1181b, 7, 17; *Pol.*, iv-vi; *Ath. Resp.*). He became the historian of science and philosophy (*Metaph.*, i). He contributed fundamentally to the chronology of literature (*Didascaliae;* see Jaeger, 326), founded poetics and created philology (30:328). He believed that cultures grow and decay like organisms; that there are cycles in the discovery of truth; that the myths and proverbs of pre-literary times embody the discoveries of previous ages (*De Caelo*, 270b, 19; *Metaph.*, 1074b, 11; *Meteor.*, 339b, 28). His ethnological, antiquarian and mythological interests were reflected in the Barbarian Customs (30:328). He believed that man

had lifted himself out of barbarism; but among the abundant evidences of the cultural developments that attested it, he found no corresponding change in the human type. The primeval inhabitants may have been "ordinary or even foolish people," earth-born perhaps, but always men (*Pol.*, 1269a, 5).

Turning now to the ***problems of reproduction*** and the ***development of individual organisms*** (*Gen. Animal.*), in which Aristotle was especially interested, it might be said that as an embryologist he was an evolutionist—in the sense of Agassiz, however, not Darwin. A summary of his views in this connection will serve as final words of introduction to a consideration of his views on the development of races.

Aristotle recognized both sexual and non-sexual reproduction; and to this knowledge he added a straightforward belief in spontaneous generation. He knew nothing of cells, so he could have known nothing of the cellular nature of the sex elements. And he knew nothing of modern factual presumptions against abiogenesis. So it was easy for him to see marked resemblances between processes now known to be fundamentally distinct. It was easy for him, in fact, to contemplate sexual reproduction, non-sexual reproduction and spontaneous generation from a single point of view and to reach a unitary conception that presents insuperable difficulties to modern eyes.

Essential to this conception were, first, a material basis for the generated individual, and second, an agency capable of imparting to this material the ***motion*** displayed in development and the non-material ***formal cause*** without which none of the potentialities of the developing individual could be finally realized. The material basis was provided by the catamenia (in the human female), by analogous material in other organisms, or by the earth itself. The non-material activating agent was derived from the semen of the male, or from analogous material in other organisms (plants have it "mingled with the female principle within themselves", *G. A.*, 762b, 11), or from earth or air (762b, 14).

Sexual reproduction was characteristic of animals that move about (730b, 33), among whom the sexes were separate. This was in marked contrast with matters in the sessile plants, in which—with the possible exceptions of the fig-tree and the caprifig (715b, 17–25),—the sexes were 'mingled' (731a, 1). The female animal contributed a homogeneous secretion which was in fact a nutritive residue that closely resembled 'primitive matter' (729a, 32; *De Lon. et Brev. Vit.*, 466b, 8). But this she was unable to concoct into semen comparable in powers to that of the male (728a, 19; 765b, 15; 766b, 17), and was, accordingly, in this respect, the inferior sex. The semen of the male was a secretion also (725b, 26), but fully concocted and endowed with the activating agent mentioned in the previous paragraph. This activating agent, however, though usually associated with male semen, was not dependent exclusively on it as a medium. It might pass from the male through the air and be inspired by the female, as when the hen partridge conceives on standing to windward (*sic*) of and within scent of the male *Hist. An.*, 560b, 13). It might even exist, independent of any living organism, in the air or the soil (*G. A.*, 762b, 17). Always, however embodied, it was superior to the material principle supplied by the female. For the formal nature, Aristotle repeatedly insisted, was of greater importance than the material (*P. A.*, 639b-642a).

This was more than two thousand years before the historic experiments that led to the now familiar aphorism: ***Omne vivum***

ex vivo. There were no such crucial facts to compel Aristotle's attention. For him, the offspring was not of necessity an organized fragment of the parent. He knew very well that plants complete themselves from slips and cuttings. Such regenerative processes, however, appeared to him only to prolong the life of the individual from which the cutting came, just as buds do (*L. et B. V.*, 467a, 23–31). And he distinguished them for that reason from the reproductive processes typical of animals.

Non-sexual reproduction, however, was not limited to plants. Aristotle was convinced that certain sedentary animals, such as mussels, propagate much as onions do, by lateral budding; and that others emit a 'generative slime' (distinguished from true semen), from which, apparently by a process analogous to fragmentation, many individuals might spring (*G. A.*, 761b, 24ff).

Spontaneous generation produced both plants and animals, the latter as diverse as barnacles, insects, eels, possibly quadrupeds, and even man himself (762b). The organisms appeared in all cases in soil, water, and mixtures of the two, with or without accompanying evidences of putrefaction. The heat necessary for the adequate concoction of such a substrate (supplied in sexual reproduction by the animal itself) was the "heat of the warm season in the environment." Then "the portion of the psychic principle which is either included with it or separated off in the air makes an embryo and puts motion into it" (762b, 17; 762a, 18–26). This was a very different conception from the awakening of material germinal particles with life inherent latent from the beginning of creation—as Anaxagoras imagined, and Augustine, and the pre-formationist Charles Bonnet. Here again, Aristotle showed his relative indifference to the materials out of which the embryo was formed. Whatever their source—in catamenial nutritive residues or in festering soil—they were in every instance nonliving and of secondary importance. The non-material principle that gave motion and form to this inert substrate was the thing of prime concern. There was no organized material continuity, therefore, between parent and offspring in sexual reproduction. What continuity existed was established by imponderable principles; just as continuity was similarly established between a spontaneously generated organism and its inorganic substrate.

Regardless of its source, however, the generative substrate changed profoundly during its development. It began as a homogeneous secretion, perhaps; in any case without preformed organs. To that extent, development was epigenetic (734a, 17–735a). But development was also predetermined, proceeding always according to eternal designs, both individual and specific, toward an end or fulfillment that itself determined every stage of the process (*Phys.*, 202a, 12; *G. A.*, 741b, 7ff). Obviously, however, it did not follow an absolutely invariable course. First of all, the developing individual might be a male, or it might be a female. Then it might resemble one parent, or the other, or a grandparent, or still more remote ancestor. Or it might have no family resemblance at all, though otherwise normal and true to specific type. Or it might depart from the parental type, as when black grapes appear among the customary white (770b, 20). Or it might be defective to the point of frank monstrosity.

All these cases were common knowledge in Aristotle's day, as in ours. By way of explanation, he referred them to accidents of development depending on certain differences between the male and the fe-

male nature. When the former prevailed in reproduction, the offspring was male; when it did not prevail, female (767b, 22). The latter was looked upon as an imperfect male, unable to fulfill the exclusively male function of imparting motion and form to the embryo. Yet the material principle which she furnished reacted to some extent on the male principle much as something which is cut affects the cutting instrument (768b, 17).

Again, the offspring took after the father when the paternal principle in the semen prevailed, and after the mother when it did not. Similarly, resemblances to more remote ancestors were interpreted as results of the success or failure of still other principles pertaining to the male. And, finally, monsters and other defects were but extreme terms in the same series (767b, 10ff). In a sense, all departures from the norm established by the male might be regarded as monsters in greater or less degree. As we have seen, Aristotle did not hesitate to contemplate the female of the species from this frankly unflattering point of view (767b, 6ff).

It should be emphasized that all the variations enumerated above were looked upon by Aristotle as *developmental defects*. In the case of the grape already cited, he expressly denied the possibility of a "change into another nature" (770b, 24). It would appear that for him they were all due to perturbations of developmental patterns—patterns that were eternal in form but sufficiently plastic to accommodate themselves to various resistances in the material substrate without altering their true nature or relinquishing their essential control. This was a natural concession for him to make to the familiar facts of every day experience with which every naturalist is confronted. At the same time, he did not compromise his lifelong belief in the primacy of immutable forms that governed not only his views on reproduction and development but his more rigidly architectural conception of the celestial mechanism.

Up to this point in our consideration of Aristotle as a possible contributor to the evolution idea, he has appeared as a *cosmologist*, fashioning his universe according to fixed, eternal plan; as a *geographer* and *historian*, recognizing changes in the topography of the earth and in the conditions and cultures of men, but without changes in the nature of man himself; as an *embryologist*, describing the gradual differentiation of individuals from simple beginnings into fully developed adults, but silent on the possibility of similar differentiation from humble beginnings of the races to which they belong. This last phrase suggests the final focus of our attention. For notwithstanding the contrary presumption that has been accumulating in the foregoing paragraphs, did he after all somewhere express, as Osborn and those who followed him have thought, "the modern conception of the Evolution of life, from a primordial, soft mass of living matter to the most perfect forms?"

Aristotle "was attracted to natural history" says Osborn (43),

> "by his boyhood life upon the seashore, and the main parts of his ideas upon Evolution were evidently drawn from his own observations upon the gradations between marine plants and the lower and higher forms of marine animals. He was the first to conceive of a genetic series, and his conception of a single chain of evolution from the polyps to man was never fully replaced until the beginning of this century" (p. 44).

The last sentence is of especial importance and has already received some attention in the second section of this paper. It appears to be based on two passages from Aristotle. The first, from the *Historia Animalium*, (588b, 4ff) reads, in the translation of D'Arcy Thompson (19) as follows:

"Nature proceeds little by little from things lifeless to animal life in such a way that it is impossible to determine the exact line of demarcation, nor on which side thereof an intermediate form should lie. Thus, next after lifeless things in the upward scale comes the plant, and of plants one will differ from another as to its amount of apparent vitality; and, in a word, the whole genus of plants, whilst it is devoid of life as compared with an animal, is endowed with life as compared with other corporeal identities. Indeed, as we just remarked, there is observed in plants a continuous scale of ascent toward the animal. So, in the sea, there are certain objects concerning which one would be at a loss to determine whether they be animal or vegetable. For instance, certain of these objects are fairly rooted, and in several cases perish if detached; thus the pinna is rooted to a particular spot, and the solen (or razor shell) cannot survive withdrawal from its burrow. Indeed, the entire genus of testaceans have a resemblance to vegetables, if they be contrasted with such animals as are capable of progression.

"In regard to sensibility, some animals give no indication whatsoever of it, whilst others indicate it but indistinctly. Further, the substance of some of these intermediate creatures is fleshlike, as is the case with the so-called tethya (or ascidians) and the acalephae (or sea-anemones); but the sponge is in every respect like a vegetable. And so throughout the entire animal scale there is a graduated differentiation in amount of vitality and in capacity for motion."

The second passage is from *De Partibus Animalium* (681a, 10), here quoted in the translation of W. Ogle (13):

"The Ascidians differ but slightly from plants, and yet have more of an animal nature than the sponges, which are virtually plants and nothing more. For nature passes from lifeless objects to animals in such unbroken sequence, interposing between them beings which live and yet are not animals, that scarcely any difference seems to exist between two neighboring groups owing to their close proximity."

In these passages Aristotle paints with a broad brush a panorama of the contemporary nature with which he was personally acquainted. His thoughts are confined to the present. The transitions that he recognizes in nature as he knows it, with its less than six hundred kinds of animals, are very crude compared with the subtler transitions that Linnaeus and Cuvier and Agassiz recognized long afterward in the thousands of species that crowded their study tables. Cuvier, the founder of palaeontology, and Agassiz, who also gave careful attention to both past and present, constructed far more complete series of far more closely similar forms. Agassiz even based his distinctive classification of animals on his observation of striking resemblances between the adult stages of extinct species and embryonic stages of species now living. But the implication of these facts, so plain to Darwin, Agassiz expressly repudiated in the very last work that came from his pen. He died outside the fold of Darwinian evolutionists.

This being true of Agassiz, especial caution would seem to be indicated in interpreting the passages quoted above from Aristotle. For Aristotle was totally ignorant of palaeontology, and knew nothing of resemblances between extinct and living forms. Nature passes from lifeless objects to animals in unbroken sequence without the least suggestion of the past. No mention is made of the derivation of the organisms of which his scale is built—not a word of a genetic, or, as Newman prefers, a *phylogenetic* series. Positive evidence for the view that Aristotle was an evolutionist—in the Darwinian sense—appears, therefore, to be lacking. Which is not surprising, when it is remembered that to Aristotle species were immutable.

This crucial fact, however, has not always been remembered by commentators. It is not mentioned by Osborn, or Clodd, or Locy, or Newman, or Nordenskiöld, all of whom reckon Aristotle as a racial evolutionist, or Singer, who leans in the same direction. Royce, and Benn, and Mure (39), on the other hand, not only mention the fact, but clearly recognize its implications. Royce has already

been quoted as saying that the Aristotelian "'forms' which are responsible for the evolution of individuals are as eternal as the Platonic ideas. They therefore do not evolve." Benn speaks with equal conviction: "To him [Aristotle] the eternity not only of Matter but also what he called Form,—that is to say, the collection of attributes giving definiteness to natural aggregates, more especially those known as organic species—was an axiomatic certainty. Every type, capable of self-propagation, that could exist at all, had existed, and would continue to exist forever" (p. 292). And Mure: "the infima species—which Aristotle regards as immutable and not evolving—is a single identity. Difference between singular individual specimens serves no purpose; it springs from the irregularity of matter, and is no part of nature's plan" (p. 124).

Aristotle himself says, in *De Generatione Animalium* (14):

> "These, then, are the reasons for the generation of animals. For since it is impossible that such a class of things as animals should be of an eternal nature, therefore that which comes into being is eternal in the only way possible. Now it is impossible for it to be eternal as an individual (though of course the real essence of things is in the individual) —were it such it would be eternal—but it is possible for it as a species. This is why there is always a class of men and animals and plants." (731b, 32ff)

In *De Anima* (17) is a passage to the same effect (415b, 3ff). In *De Generatione et Corruptione* (10) there is a very significant passage which should be quoted in full. After discussing the circular and therefore eternal revolution of the heavens, Aristotle asks:

> "Then why do some things manifestly come-to-be in this cyclical fashion . . . while men and animals do not 'return upon themselves' so that the same individual comes-to-be a second time (for though your coming-to-be presupposes your father's, his coming-to-be does not presuppose yours)? Why, on the contrary, does this coming-to-be seem to constitute a rectilinear sequence?"
>
> "In discussing this new problem, we must begin by inquiring whether all things 'return upon themselves' in a uniform manner; or *numerically* the same, in other sequences it is the same *only in species*. In consequence of this distinction, it is evident that those things, whose 'substance'—that which is undergoing the process—is imperishable, will be numerically, as well as specifically, the same in their recurrence; for the character of the process is determined by the character of that which undergoes it. Those things, on the other hand, whose 'substance' is perishable (not imperishable) must 'return upon themselves' in the sense that what recurs, though specifically the same, is not the same numerically" (338b, 7ff).

These passages have their convincing commentary in the words of Agassiz (2) that follow. Reflecting on the regular succession of individuals in reproduction, he writes: "Whatever minor differences may exist between the products of this succession of generations are all *individual peculiarities*, in no way connected with the essential features of the species, and therefore as transient as the individuals; while the specific characters are forever fixed." In a footnote he adds: "All that is not individual peculiarity is unceasingly and integrally reproduced while all that constitutes individuality, as such, constantly disappears." (p. 11).

It is especially noteworthy that these sentences, expressing Aristotle's thought so clearly, were written to convey the personal convictions, not of Aristotle, but of Agassiz himself.

If, then, species were eternal, and specific characters forever fixed, what was the nature of *hybrids*? Aristotle's consideration of this question is, for our present purpose, quite disappointing. For it is given but incidental treatment, first in a discussion of the essential contributions of the sexes in reproduction (*G. A.*, 738b, 25ff) second, in a discussion of the problem of sterility (746a, 30ff). In the latter, a formal explanation of the sterility of the mule is suggested in which Aristotle

juggles with the idea of species as a logician rather than a biologist, and achieves on an insufficient basis of fact a confessedly empty and barren result (747b, 30–748a, 12). In the former he concludes that the hybrid tends in the course of generations to resemble the female in form, "just as foreign seeds produce plants varying in accordance with the country in which they are sown. For it is the soil that gives to the seeds the material and the body of the plant (738b, 35)." The female parent in the cross provides the soil by which the hybrid is nourished and thus is capable of modifying the direction of differentiation just as in reproduction within the species. There is thus no change in the specific type, apparently; merely a selection of type that is to give its characters to the embryo.

To the problem of *variation* Aristotle gave no consideration except as it concerned individual modifications that altered in no essential degree the specific type. These have already received their share of our attention.

In the field of *heredity*, it is interesting that he distinguished in so many words between congenital and acquired characters (721b, 30). He believed there was satisfactory evidence of the reappearance in the offspring of parental scars and mutilations. The mechanism, however, which Hippocrates (*Airs, Waters, Places*, xiv)—anticipating Darwin's *pangenesis*—had proposed for such cases, he flatly rejected. He opposed it with an array of arguments that it is not necessary to reproduce here, but that were thoughtful and on the whole impressive. He opposed it also as he had long opposed anything suggesting atomistic materialism. "If the parts of the future animal are separated in the semen," he asked, "how do they live?" (*G. A.*, 722b, 3). Instead of a collection of particles coming from all the body, he regarded the semen as a unitary secretion "whose nature is to go *to* all of it." (725a, 23). And though the male "does not give any material at all to the embryo" (764b, 12), it is the semen of the male with which the principle is associated that gives motion and form to the latter.

Thus, in place of the physiological mechanism of Hippocrates, there emerges a conception of inheritance in which non-material forms are the essential elements. This, as we have already seen, he used to elucidate the differentiation of the individual. Nowhere did he suggest for it a similar function in the differentiation of the race. Which again stirs suspicion that such a possibility had not yet occurred to him.

Did Aristotle believe in *xenogenesis*? Certain passages have led some commentators to that conclusion. In *De Generatione Animalium*, Book iii, Chapter 11, Aristotle wrote:

"and the so-called 'entrails of earth', in which comes into being the body of the eel, have the nature of a scolex.

"Hence one might suppose, in connection with the origin of men and quadrupeds, that if they were really 'earth-born' as some say, they came into being in one of two ways; that either it was by the formation of a scolex at first or else it was out of eggs. For either they must have had in themselves the nutriment for growth (and such a conception is a scolex). . . .

"It is plain then, that, if there really was any such beginning of the generation of all animals, it is reasonable to suppose it to have been one of these two, scolex or egg" (762b, 27–763a, 4).

The scolex is further characterized in an earlier passage: "For pretty much all creatures seem in a certain way to produce a scolex first, since the most imperfect embryo is of such a nature; and in all animals . . . the first embryo grows in size while still undifferentiated into parts; now such is the nature of the scolex" (758a, 32).

The following pertinent passage is from the *Historia Animalium:*

> "Eels are derived from the so-called 'earth's guts' that grow spontaneously in mud and humid ground; in fact, eels have at times been seen to emerge out of such earthworms, and on other occasions have been rendered visible when the earthworms were laid open by either scraping or cutting. Such earthworms are found both in the sea and in rivers, especially where there is decayed matter; in the sea in places where sea-weed abounds, and in rivers and marshes near to the edge; for it is near to the water's edge that sun-heat has its chief power and produces putrefaction." (570a, 15ff).

This is the evidence. How shall it be interpreted?

In two footnotes to the first passage quoted, Aristotle's translator, Arthur Platt, remarks:

> (1) "These 'entrails of earth' are earthworms almost certainly. A. thinks they are spontaneously generated, and develop into eels." (2) "This is the only passage from which we can gather anything about Aristotle's views on evolution; it appears to have strangely escaped the notice of modern writers on the subject. . . . It is clear that . . . he had no objection to the gradual development of man from some lowly organism, but also that he wisely maintained an attitude of absolute agnosticism on the question". . . .

According to Lones, commenting on the same pasage, Aristotle "seems to have admitted the possibility of generation of men and some quadrupeds from much lower forms of life, for he says that, if generation from the earth did happen, it must have been generation from worms or larvae, or from ova." (p. 82)

But *did* Aristotle actually consider even the possibility of 'the gradual development of man from some lowly organism, as Platt puts it, or the 'generation of men and some quadrupeds from much lower forms of life,' to use the words of Lones?

In our opinion, he did not, for the following reasons: He defined the scolex as an embryonic or larval form common to "pretty much all creatures;" not an adult form. He asserted that if man and quadrupeds were earthborn, they arose, not "perfect in every part, limbed and full grown" but as embryos, that is as scolices; that the 'entrails of earth' (*ges entera*) had the nature of a scolex; that they were spontaneously generated in mud, especially in marginal fresh and salt water; that eels had been seen to emerge from such earthworms and had been found within them. It is reasonable to infer that he associated these worms with caterpillars and other insect larvae from whose bodies in due course adult forms emerged. Possible xenogenesis thus becomes actual metamorphosis, individual, not racial. Thompson (60:149) calls attention to the fact that the larval eel (*Leptocephalus*) is well known to Sicilian fishermen as *Casentula*. This name he derives from *ges entera* through the Sicilian Doric *gas entera*. Could Aristotle himself have seen *Leptocephalus* and recognized its true nature?

The present inquiry has now reached its end. Our conclusion is that Aristotle was not, in fact, either a cosmic or a racial evolutionist. He was familiar with the development of individuals. Why, then, did he not "extend the notion of development to the kind?". Taylor (57:446), who asks this question, himself makes the following reply: "It is the simple fact that, so far as Aristotle or any of his contemporaries could know, there was no evidence for the mutability of organic types."

Is the inclusiveness of this assertion entirely justified? Was evidence perhaps available but for some reason not clearly apprehended? When Lamarck proclaimed transformism, it was not on the basis of evidence that could be said to be demonstrative. Such evidence was lacking when Erasmus Darwin wrote

Zoonomia and *The Temple of Nature;* when Spencer read Lyell; when Charles Darwin first caught the idea of descent with modification. It was similarly lacking for Huxley, and Wallace, and Gray. But to them all it was at least sufficiently impressive to be hospitably received. Agassiz repeatedly protested that the demonstration of specific transmutation had not been made; and Baer, even after Agassiz' death, continued similar criticism of the "new Evolution." It can hardly be said that these distinguished naturalists were basing their protests on ignorance. There were no facts available to the Harvard botanist Gray that were not equally accessible to the Harvard zoologist Agassiz. What separated them were not facts but preconceptions. Gray was ready to accept the idea of specific mutability. Agassiz, on the other hand, continued vigorously to reaffirm his old conviction that species were categories of the Creator's thought, divine and immutable.

Similarly, Aristotle had adopted early and retained throughout his life the conception of immutable specific types. Yet the evidence in favor of the fixity of specific types is surely no weightier than the evidence for their mutability. Neither Aristotle nor Agassiz could demonstrate their convictions. Nevertheless, they held them, and looked without enthusiasm on all suggestions to the contrary.

It is idle to inquire whether, had Aristotle held Agassiz' professorship at Harvard, he would have approved *The Origin of Species*. To do so, he would have been obliged to revolutionize his conception of species, accelerating the process by revolutionizing his conception of adaptation as well. For adaptations to Aristotle were manifestations of eternal ends. The significance of vestigial organs appears to have eluded him entirely (*P. A.*, 670b, 13; 689b, 5; *H. A.*, 502b, 24). Discovery, in fact, was but the recognition of such ends in terms of final causes. They were not causes at all in the modern scientific sense. On the contrary, they turned his eyes away from causal mechanisms and postponed for many long sterile centuries the prime discovery of the working hypothesis.

Against the consequences of these retarding influences, so serious for scientific progress under his successors, Aristotle's developing enthusiasm for the concrete particulars of sublunary experience made such advances that the last dozen years of his life in Athens have been more than once distinguished as his *scientific* period.

But was he thereby becoming an evolutionist? Singer may be right in believing that another decade would have removed all doubt of it. At the same time, the reflection obtrudes itself that what Aristotle failed, in his ancient world, to see, Agassiz, in modern days but in a similar universe of ideas, not only clearly saw but with the very last words he penned, expressly disavowed.

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A CRITIQUE OF PLANT SEROLOGY

PART I. THE NATURE AND UTILIZATION OF PHYTOSEROLOGICAL PROCEDURES

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[*Editorial Note:* Exigencies of space make it necessary to publish this review in three parts. Parts II and III will appear in the next two numbers following this. The extensive bibliography will follow Part III]

I. INTRODUCTION

WHILE the subject of the serology of plant products has claimed, to a greater or less degree, the attention of botanists and pharmacologists for about forty years, it is only within the last decade or two that studies in this field have conclusively demonstrated the usefulness of serology in plant research. Particularly through the efforts of Mez and his associates in Germany, of Osborne, Wells, and their colleagues in America, and of a number of independent workers in Europe, the subject of phytoserology has attracted widespread attention in recent years, as is attested by the accompanying bibliography. Up to the present no attempt has been made to collate and unify the rather extensive literature in the field in question. The writer has repeatedly felt the need for such a digest, particularly as the greater part of the literature involved is to be found only in European publications. The present paper is designed as an attempt to meet this need.

In delimiting the scope of such a review it has been felt advisable to restrict the discussion to the serology of plants and plant products exclusive of bacteria. The serology of the phytopathogenic bacteria commands the attention of the phytopathologist, but the growing literature concerning this phase of serology has been dealt with by Link (179). The literature on plant serology as delimited above embraces the techniques and applications of phytoserology in such problems as the following: the identification of plants and plant products; the study of the systematic relationships of plants; the investigation of protein metabolism in plants; the genetic constitution of plant hybrids; the nature and properties of the plant viruses; and the elucidation of the principles of general serology as exemplified in plant materials.

The concepts of serology rest upon the thesis that the introduction into the animal circulatory system of a foreign substance of biological origin, typically proteid (antigen), gives rise to the production in the blood of substances (antibodies) capable of affecting the introduced antigen in a variety of ways, which effects may be demonstrated by a number of types of serological reactions. From the point of view of plant serology these reactions may be briefly characterized as follows:

1. In the *precipitin reaction*, the combination of antigen (precipitinogen) and antibody (precipitin) results in a visible precipitation. "*Conglutination*", so-called, a reaction utilized to considerable extent

by Mez and his associates, may in the form used by Mez be looked upon as a modification of the precipitin reaction, differing in the addition to precipitin and antigen of a third substance, "conglutinin", present in normal cattle serum and believed to increase considerably the sensitivity of the precipitin reaction.

2. In the *anaphylaxis reaction*, the introduction of antigen (anaphylactogen) is followed by the production in the animal of an antibody (anaphylactin) which in combining with the anaphylactogen under appropriate conditions results in the production of more or less violent and spasmodic muscular contractions, of hyperactivity of various glands, of inflammation of tissues, and of other abnormal physiological effects, often resulting in death.

3. In *lysis*, the antibody (lysin) produces a dissolution of the antigen, which in this case is customarily particulate, as for example is true of such antigens as bacteria, blood corpuscles, and in plant serology, plastids, spores, pollen, unicellular algae, fungi, etc. As special modifications of the lytic phenomena may be mentioned the Abderhalden reaction and complement fixation. The *Abderhalden reaction* consists of a detection of the lysis of soluble, proteinaceous, non-particulate antigens, through a chemical analysis of the protein cleavage products formed on the interaction of antigen and lysin. *Complement fixation* rests upon the thesis that a hemolytic antibody requires for its action the presence of a non-specific, heat-labile constituent of normal blood (complement). When an antigen and antibody unite, complement is used up or fixed, and the extent of such fixation of complement, which varies with the extent of serological reactivity of antigen with antibody, is measured by observing whether or not there is sufficient complement unused to permit a second, visible reaction to take place. This second reaction is customarily the lysis of sheep blood-corpuscles by the serum of rabbits immunized with sheep corpuscles, the system being free of complement beyond that which may or may not be left unused by the primary reaction. Under appropriate conditions the lysis of the sheep corpuscles, the observed effect, takes place only in the absence of specific reactivity between the primary antigen and antibody.

4. *Agglutination* of a particulate antigen (agglutinogen) by an immune body (agglutinin) results in a clumping and precipitation of aggregates of antigenic bodies (bacteria, corpuscles, pollen, plastids, unicellular fungi, algae, etc.) which is probably a manifestation of the same antibody as participates in the precipitin reaction, the precipitin or the agglutinin reaction following according as the antigen is in solution or suspension.

5. *Acquired toleration* of toxins and the *neutralization* of toxins and viruses constitutes a fifth expression of immune reaction of animals to plant materials, toxins being rendered innocuous and viruses non-infective by the action of immune neutralizing antibodies.

6. *Phagocytosis*, the ingestion of foreign matter by amoeboid cells of the animal body, is dependent upon the presence in the blood of *opsonins*, which latter may be specifically increased in amount as a result of immunisation with plant products.

The reactions enumerated above have all been applied to the study of plant products, and it has been found that the immunisation of an animal with a plant antigen may result in the appearance of antibodies producing any one or several of the reactions in question, while in addition it has been found that frequently normal, non-immune animal sera will show analogous phenomena in the presence of plant antigens at high concentrations, and conversely that not rarely plant antigens, instead of being precipitated, agglutinated, or dissolved by normal animal serum, affect the normal blood in such a way as to exert analogous actions upon its various constituents.

It is not within the scope of the present paper to go into detail as regards the principles and theoretical aspects of the serological reactions in question, and this phase of the problem will be found adequately presented in Wells' *Chemical Aspects of Immunity*, and Zinsser's *Infection and Resistance*. The details of technique as applied to plant materials, however, as well as such results and theoretical matters as are of chief interest to the botanist, will be dealt with below.

II. PREPARATION OF PLANT ANTIGENS AND IMMUNE SERA

Preparation of antigens for immunisation. Many techniques for the preparation of plant antigens have been utilized, the chief requisites being that antigenic extracts be non-toxic, free from substances producing artefact reactions, and protein-

containing. In much of the work reported up to the present, solutions of the proteins of pulverized seeds have been used, the solvent most frequently being .85 per cent NaCl. Because of aspecific artefact reactions which may result from the non-protein constituents of seed powders, it has become customary to precede protein extraction by a thorough preëxtraction with ether, petrol-ether, benzol, or acetone, followed by a second pre-extraction with concentrated ethyl alcohol. More will be said subsequently of the value and hazard of the alcohol pre-extraction, since this is one of the controversial differences between the Königsberg and Berlin schools of plant sero-systematics, but it may be said that in general the alcohol preëxtraction is to be recommended provided that it is not continued so long as to result in a possible denaturation of the antigen. The plant proteins as a rule are more resistant to both heat and alcohol than are the animal proteins, and the danger of denaturation is not as great as is often believed (263). It should be mentioned, however, that certain types of plant protein, the prolamines (e.g. zein, gliadin, hordein), are soluble in strong alcohol. Such proteins are to be found in the seeds of nearly all grains, and doubtless elsewhere in the plant kingdom, and in the preëxtraction of plant tissues with alcohol, allowance must be made for the possible loss of such proteins.

Preëxtraction, besides removing substances which may cause artefact reactions, also has the advantage of removing many substances which are of no value in the serological work, and which are disadvantageous in their toxic effects on the experimental animals or in the fact that they may be largely responsible for difficulty in rendering plant solutions sufficiently clear for serological testing (e.g. gums and resins). Thus non-pre-extracted solutions are frequently slimy, thick, and opalescent, and this is often obviated by preëxtraction. The alkaloids of plants (e.g. nicotin, atropin, strychnin) are frequently responsible for the death of animals during immunisation, and these may be largely or entirely removed by alcohol preëxtraction. Mez recommends the addition of a small amount of tartaric acid to the alcohol used in pre-extraction (221, 390) as this mixture will dissolve certain toxic alkaloids, e.g. those of the Coniferae, which are not dissolved in pure alcohol. Preëxtraction is usually continued for several hours or even for one or more days, in an extraction apparatus. Acetone has been used very rarely as a preëxtractant (41) but might well be considered, as it is a solvent of resins and gums which frequently give trouble in antigen preparation.

Having considered preëxtraction one is next confronted by the problem of protein extraction. Instead of the more commonly used isotonic saline, either water or weak alkali may be used. Both the Königsberg and Berlin schools have turned to alkali in cases where protein tests show little chemically demonstrable protein in saline extracts. 0.1 to 1.0 per cent NaOH is the usual concentration. At first the Königsberg workers were of the opinion that there was no serological difference between the NaCl and NaOH extracts of the same plant materials (220, 278, 192, 304), but this has been contradicted by the Berlin workers (96, 7) and more recently by Guttmann at Königsberg (117). Subsequent neutralization with acetic acid is recommended if higher concentrations of alkali are used, but the precipitate resulting from neutralization should not be filtered but should be injected in suspension, since this precipitate contains considerable quantities of native protein. A

small amount of phenol may be added to the extractant to check decomposition (390, 292, and others) without affecting the serological results. Water as a solvent is not generally recommended. Phosphate buffers have been used by Gohlke (104, .85 per cent Na_2PO_4)and the writer (47, KH_2PO_4 + K_2HPO_4, M/10 at pH 6–8) with success. The time required for satisfactory extraction depends upon both material and temperature, most workers extracting from 1/4–2 hours at 37°C., or from 8–24 hours on ice. In certain types of experimentation strong salt solutions have been used as extractants (84), but such a procedure is not recommended as a customary technique.

The concentration of antigen for immunisation varies according to the material in hand, but most workers have used from 5–20 parts of dry tissue per 100 cc. of extractant. The antigens for immunisation should be as concentrated as possible when one is dealing with plant extracts, in which the concentration of soluble protein is often very low. Such antigens need not be clear, but it is desirable to filter or centrifuge them to avoid animal injury or even death by embolism if the venous route is used in immunisation. The materials to be extracted are rarely sterile, although a thorough preëxtraction serves to sterilize the tissue pulp. While many authors lay particular stress on the desirability of sterility of extracts, in the writer's experience this has seemed relatively unimportant provided due attention was paid to chemical cleanliness of apparatus. In spite of the fact that non-sterile extracts have been used in the immunisation of hundreds of laboratory animals, no case of animal loss has been traceable to lack of sterility of the immunisation antigen.

In the maceration of tissues, simple grinding in clean mortars is usually sufficient. The addition of pumice or other abrasive is not necessary except in cases where maceration is difficult, as with delicate fungi (221, 390, 180), and elsewhere the use of abrasives is to be avoided because of the danger that antigen may be adsorbed to the abrasive and thus prevented from becoming dissolved in the extractant.

The techniques described above apply typically to dried seed powders and all other dried plant parts. Alternative satisfactory procedures involve the use of fresh tissues. Expressed saps are in some cases more satisfactory vehicles of immunisation antigens than saline extracts of fresh tissues, and such saps may often be used without preëxtraction in the production of specific sera, and as test antigens. If artefact-producing substances are present, they can frequently be removed by dialysis. Such saps are usually of pH 5–6, but neutralization has not been found necessary. Expressed saps have been used successfully by Birkeland (29), Franz (96), Magnus and Friedenthal (188, 189, 190), and the writer (47, 50). Maschmeier in addition (196) used expressed sap dried in thin layers, pulverized, and later preëxtracted and extracted in saline.

Similarly fresh tissues may be macerated, preëxtracted or not, and then extracted. Ziegenspeck in Königsberg has recommended such a procedure for plant serology (390), and Bürger in Berlin also finds it suitable. The students of the serology of the plant viruses have in general used saline extracts of fresh vegetative tissues, and other workers report success with such a technique (196, 26, 84, 69). The choice of sap, extract of dried tissues, or extract of fresh tissues will depend on the material available, but all may be used with success.

When dealing with small, particulate

antigen-containing bodies, e.g. unicellular algae and fungi, pollen, plastids, etc., it is entirely feasible to suspend the unmacerated bodies in physiological saline or olive oil, and then inject. The latter procedure has recently been adopted by the Königsberg workers (169), and there have been numerous reports of success using for immunisation saline or oil suspensions of macerated vegetative tissues (225, 56, 105, 26, 180), of algae (300, 188), and of yeasts and fungus spores (167, 207, 178, 64, 323, 34, 198).

A common type of antigen in the serology of bacteria is the filtrate of the substrate on which the bacteria are grown. Such an antigen has little place in most phytoserology, but Matsumoto (197, 198) obtained good precipitin results using filtrates of *Aspergillus* cultures, when antigens prepared from dried mycelium and and spores were unsuitable.

Purified or altered protein preparations have been used as antigens by numerous workers in plant serology, the techniques of antigen preparation being too varied and specialized to discuss in detail, particularly as a separate section below will deal to a greater extent with this subject. Much of the work dates back to the classic studies on the purification of plant proteins by Osborne (263) and the subsequent immunological studies of such purified proteins by Osborne and Wells and their colleagues, the purification being accomplished by taking advantage of such differential properties of the proteins as their solubilities in water, salt solutions, acids, alkalis, and alcohol. Such proteins frequently but not invariably show an extremely high degree of antigenicity as judged by various tests. Modified plant proteins, such as iodized, diazotized, and peptized proteins, have also found a place as antigens, but a discussion of these specialized types of antigens is better taken up at the time when the results of plant-serological tests are considered.

Similarly, various non-protein plant substances have been used as "antigens" in attempted immunisation of animals. Chief among these are chlorophyll and xanthophyll, starches, glucosides, and other carbohydrates, plant lipoids, and alkaloids. The preparation of such "antigens" and the result of their serological study will be considered subsequently.

The question of whether preserved plant materials may be used as antigens is of practical importance. The Königsberg workers have found that, while plant materials preserved in H_2SO_4, $HgCl_2$, and formalin are unsuitable for serology, materials preserved in alcohol are entirely suitable, and dried herbarium specimens which have been so preserved for many decades still give tests comparable to those given by fresh material of the same species (217, 210, 211).

Whether, on the other hand, various tissues of the same plant may be used indiscriminately in sero-systematic work, is a question which has been debated by the phytoserologists.

The Königsberg school maintains that all tissues of a plant react similarly (217, 220, 211, 117, 390, 214, 380), a position which accords with Mez' view that the serologically active component of plant extracts is the nucleo-protein or ideoplasm which is presumably identical in all cells (213, 214). Likewise, Magnus and Friedenthal (190, 191) were unable to detect any significant serological difference between seed, pollen, and root antigens of rye, although from their tables this identity is not as well indicated as one might expect. The Berlin workers, on the other hand, find that leaf and seed extracts of the same plant behave quite differently serologically (209, 39, 38), and this finding is confirmed by Moritz and vom Berg in Kiel (235, 24), the Berlin work resting on the precipitin test, the Kiel work on the delicate uterine-strip (Schultz-Dale) anaphylactic test. Dunbar (69) has reported complement fixation tests in which rye pollen, rye grain, and rye leaf sera were each highly specific for its own tissue. Azuma (12) found serological (anaphylaxis) differences between seeds

and seedlings of grains. Kubeš (165) in complement fixation tests had no trouble in differentiating seed, leaf, and root of the poppy. Wodehouse (382) believed that pollen and seed proteins of the same plant are probably anaphylactically distinct. Finally, Lieske (176) reports that chlorophyll-containing and chlorophyll-free cultures of the same species of green alga are to be distinguished serologically (agglutination).

On the whole, the evidence seems to indicate that various tissues of the same plant may show serological differences. This is particularly in line with the finding of the investigators of purified seed proteins that such storage proteins give definite chemically-specific but not species-specific serological tests, and since such storage proteins are lacking in other parts of the plant than storage tissues, it it follows that the storage tissues will presumably react serologically differently from the vegetative tissues. This possibility must accordingly be borne in mind in the selection of plant tissues for the preparation of antigens. Wartenberg (359) has distinguished between species-specific and organ-specific proteins. The latter, exemplified in animals by eye-lens protein, are relatively uncommon in plants, the majority of the organs of a plant being fairly uniform serologically, but the possibility of the occurrence of organ-specific proteins makes it advisable to limit single experiments to a single type of organ. The problem of antigenic variation in a given individual at different stages of development has never been adequately explored. Possibly there may occur a recapitulation of protein structure during ontogeny, comparable to morphological recapitulation.

For animal injection the toxicity of plant tissues must be considered. Many plant tissues are very highly toxic to animals, e.g., those of the Coniferae, especially the Taxaceae and Gnetaceae, *Rhododendron*, *Phytolacca*, *Melandryum*, *Pisum*, *Aesculus*, *Dipsacus*, *Allium*, *Iris*, *Canna*, *Thea*, etc. (221). The toxins of such plants may render successful immunisation very difficult. Proper preëxtraction will aid in the removal of toxic substances. In addition, or as an alternative, dialysis through collodion (Du Pont "cellophane") rapidly removes the highly toxic alkaloids of such plants as tobacco, potato, and tomato, leaving the proteins behind. By dialyzing against a buffered salt solution, one may at the same time stabilize the salt concentration and the pH of the antigenic solutions. Frequently when plant serologists have faced the difficulty of toxic extracts, the solution has been to select another related but toxin-free species, and in a study of plant relationships such a practice might sometimes suffice.

Nearly all workers in plant serology have emphasized the desirability of some method of measurement of the protein in antigenic extracts. Of the numerous techniques which have been used, those chiefly concerned are the Esbach test, Kjeldahl determinations, and the hot nitric acid test or "Kochprobe," although the trichloracetic acid test, the biuret reaction, precipitation with ammonium sulphate, the Millon test, the nitro-prussic reaction, and the sulphosalicylic acid test have all been mentioned in connection with the preparation of plant antigens. In the case of the Esbach test, Esbach reagent (picric acid:citric acid:H_2O = 1:2:100) is added to the extract, and the protein precipitate resulting is either estimated as to amount or measured in an albumimeter tube. The Esbach test has been more frequently employed in the Königsberg school, the nitric acid test in Berlin. Both schools recognize the inadequacies of such tests, however. The difficulty lies in the fact that not only is there no theoretical reason for

expecting that immunologically active protein and chemically demonstrable protein should be correlated, but actually numerous investigators (217, 211, 228, 333, 390, 245, 38, 105) have found that good serological reactions may be obtained with extracts giving poor or negative protein tests (usually Esbach), and that some chemically active proteins are poorly reactive serologically. Furthermore none of these protein tests is specific for protein in the sense that it reacts with all protein and only with protein. In spite of these facts, many investigators have not only felt that it is necessary to keep a record of such tests, but that in addition all extracts of an experimental series should be diluted to an extent where all give the same test with the protein indicator used (129, 211, 17, 98, 385, 8, 196, 36). If there are motives for so doing, there are motives that appear at least equally strong for holding constant not the protein test but the ratio of plant tissue to amount of solvent, and it may very well be that in some cases serological results have been badly distorted by the adjustment of all extracts to the same Esbach test.

Finally, a word should be said as to the preservation of antigenic extracts. When plant powders are used, a most satisfactory procedure is to seal the powders in brown bottles which are kept in the cold, solutions being made whenever required. Such powders keep indefinitely without deterioration. Extracts and saps may be preserved on ice with or without the addition of phenol (1 per cent) for considerable lengths of time (a few weeks to a year or more), and in some cases the preservation of antigenic extracts and saps frozen solid may be continued a year or more without appreciable loss of reactivity. The drying of extracts on paper with subsequent re-solution in saline has been suggested as a means for antigen preservation, but the method has never come into general use, and the theoretical advantage of accuracy in measurement seems to be outweighed by a number of practical disadvantages.

Immunisation of animals and preparation of sera. For the preparation of sera immune to plant antigens, any of a variety of animals may be used, although in the greater part of the work thus far reported rabbits have been employed. Dogs have been used successfully for the production of immune sera by Cao (42), Corpaci (61), and de Angelis (5), goats by Dunbar (68) and Ehrlich (73), horses by Dunbar (68) and Müller (243), and guinea pigs by Corpaci (61) and others. For anaphylactic sensitization the guinea pig is almost universally used. In preparing immune sera, rabbits weighing from 1500 to 2000 gm. are preferable, and a period of quarantine before inoculation is desirable in order to minimize the danger of loss from coccidiosis.

The question of the feeding of animals for immunisation is an important one theoretically, because of the possibility of the production of immune bodies due to immunisation *per os*. It is well known that animals can be immunized by feeding certain plant toxins to a point where many normal lethal doses may be eaten with impunity. Thus various animals can be immunized to ricin (243, 72, 135, 226) to a point where they may resist as much as 5000 normal lethal doses (62), and rabbits can similarly be immunized to the *Amanita* toxin, phallin (86), yet while it is comparatively easy to produce specific precipitins by inoculating animals with ricin, no specific precipitins are to be found in the blood of animals which have been fed large doses of ricin (226). Similarly rabbits and guinea pigs fed exclusively on rape, radish, lettuce, or cab-

bage yielded no precipitins in their sera for extracts of these plants, although *cutaneous immunisation produced such* precipitins (128). *Again* Wendelstadt and Fellmer found that exclusive feeding of potato, pea, or corn produced no precipitating, complement fixing, or anaphylactic antibodies for proteins of these plants (370). Worseck also had similar results (383). Hiki (125) claimed that the feeding of guinea pigs and dogs with bean protein and egg white gave rise to sera which contained precipitins and anaphylactic antibodies for these proteins, but the titers were low and the work has not been confirmed. It would thus seem evident that there is comparatively little danger in immunisation due to the appearance of antibodies produced by ingested food, although in the serological testing of articles involved in the diet of experimental animals it would be well to keep such a possibility in mind. One need not feel compelled to use carnivorous animals in the serological study of plant proteins.

As regards route of immunisation, intraperitoneal and intravenous inoculations are in most general use, particularly the former. Intravenous inoculations have been claimed to produce high-titer sera rapidly, but in the experience of the majority of workers the intraperitoneal inoculations are fully as satisfactory and avoid certain difficulties and dangers of intravenous inoculation (e.g. animal loss through embolism, anaphylactic shock, or extract toxicity). Extracts for peritoneal injection need not be free of particles, and sterility is of less concern with this type of inoculation. A combination of one or two intravenous inoculations followed by several intraperitoneal inoculations is frequently used to advantage. Intracutaneous inoculations have been frequently used, particularly in the earlier work on plant serology, but while it is often possible to produce immune sera by *this route, it is not recommended for* general use as immunisation is slow, the titers are not as high as by the venous and peritoneal routes, and in plant extracts there are frequently present substances which irritate to such an extent as to bring about the formation of large abscesses at the site of cutaneous inoculation. Immunisation *per os* may be used for the production of an immune state to many plant toxins, but, as has been seen above, this is not a satisfactory route for the production of immune sera or an anaphylactic condition.

The schedule of inoculation, i.e. the amount of extract inoculated, and the frequency and number of inoculations, will vary with the concentration of the inoculum. Since antigenic plant extracts are for the most part relatively poor in protein, one usually uses a greater amount of inoculum and longer period of inoculation than is customary with the more highly concentrated animal antigens. Using extracts of whole plant tissues it has been the practice to inoculate rabbits with from 25–100 cc. of extract in 6–10 injections at intervals of 3–4 days. Rabbits show great individual differences in susceptibility to immunisation, and with some individuals it is extremely difficult or impossible to obtain high titer immune sera. Over-injection has been avoided by the Königsberg school because of a possible danger of loss in titer from too long continued inoculation. Longer intervals than 4–5 days are to be avoided because of the danger of anaphylactic shock after a week or more following injection. From 5–10 cc. of inoculum may be injected peritoneally and from 2–5 cc. intravenously, as a general rule, without undue ill effect on the experimental animal.

In the writer's experience the following technique has produced excellent precipitating and neutralizing sera *with little animal loss* (50): *non-preëxtracted* expressed sap, *inoculated intraperitoneally in* 5 cc. doses except for the first dose of 2 cc. with 5–8 injections at 3–4 day intervals, followed by a rest period of 9–10 days before bleeding. The animals thus inoculated in general showed a slight loss in weight after the first injection, which was soon regained, and at the time of the last injection most animals showed a gain in weight over the immunisation period of 100 to 500 gm. Using purified protein solutions the dosage and length of the period of immunisation may be considerably decreased, and if chemically pure plant protein is used a single injection of a few milligrams may suffice to produce high-titered precipitating serum 10–20 days after inoculation.

It is quite customary among phytoserologists to make periodic blood probes during the course of immunisation, as for example after the 3rd, 6th, and 9th inoculations. The blood probe is made 4–6 days after the injection, usually from a sample of about 5–10 cc. of blood drawn from the marginal ear vein or heart of the rabbit. This blood is allowed to coagulate, the serum is centrifuged clear, and serological tests are performed to determine the titer of the serum. Blood probes are desirable when one is in considerable doubt as to the length of time necessary for adequate immunisation, or when, as in some types of sero-systematic work, a low-titer serum is desired.

Preliminary to the first inoculation it is desirable to take a sample of 10–15 cc. of blood from each rabbit, which will serve in later work as a normal serum control for the serological tests with the immune serum from the same animal. Such a procedure is preferable to the rather common practice of taking a larger quantity of normal serum from one or several animals which are not used later for immunisation. The normal serum should be preserved in the same manner as the immune serum later obtained, and to insure comparable normal and immune sera, the immune sera may be preserved a few days before using.

After the last inoculation it is customary to wait a week or 10 days before bleeding in order to insure that there is no longer free antigen in the blood. For 24–36 hours before bleeding, the animal should be given no food, as feeding just before bleeding often results in sera which show a tendency to gelatinize or which have a more or less marked opalescence that may render difficult the reading of precipitin or agglutination tests. A similar period without food should precede the taking of normal serum. If the sera are opalescent, heating to 56° for an hour will often eliminate this without destroying specific reactivity.

In bleeding it is preferable as a rule to take all the blood possible from an animal rather than to take sub-lethal samples at intervals after immunisation. This insures the production of 30–40 cc. of comparable immune serum from a rabbit, and there is less danger of change in titer from preservation *in vitro* than from preservation *in vivo*. Sterile blood may be obtained either by bleeding from the carotid artery or by dissecting open the breast cavity, cutting the aorta, and pipetting out the blood. Both techniques are entirely satisfactory. The experimental animals are anaesthetized, as with ether, chloroform, etc., preliminary to bleeding. The drawn blood is permitted to coagulate for an hour or two at room temperature or over night on ice, the clot is loosened from the sides of the tube, and the serum is centrifuged twice.

Sera may be preserved in a variety of ways, as by the use of glycerine, phenol, thymol, toluol, or chloroform, or by storing at refrigerator temperatures, freezing solid, drying on paper or drying to powder with subsequent re-solution in physiological saline, or by asepsis or bacterial filtration, or a combination of these measures. All the techniques mentioned have been used in phytoserology, and all are useful with the exception of the method of drying which has not given entirely satisfactory results.

In preparing animals, chiefly guinea pigs, for anaphylactic tests it is customary to give a single intraperitoneal sensitizing dose followed by the test for anaphylaxis approximately 18–24 days later. If, instead of inoculating a plant antigen solution for sensitization, an intraperitoneal injection be made of 2–3 cc. of anti-plant precipitating serum, the animal becomes passively sensitized and will react specifically to the antigen used in preparing the serum. The test for anaphylaxis in this case may be made 24 hours after the intraperitoneal inoculation of the immune serum or 8 hours after if the inoculation of serum is intravenous.

A certain amount of animal loss usually accompanies immunisation with plant antigens. With rabbits, coccidiosis is a frequent cause of death and can best be guarded against by quarantine before immunisation and subsequent cleanliness in the cages. Much of the animal loss experienced by plant serologists may be avoided by care in handling of animals and use of extracts. The frequent men-

tion of animal loss in the literature is largely due to embolism as a result of air bubbles or solid particles in antigenic solutions injected directly into the circulatory system, failure to permit animals to recover from previous inoculation before reinoculation due to too heavy dosages or too short intervals of inoculation, and toxicity of plant antigens. Anaphylactic shock is rarely responsible for animal loss in immunisation. It is advisable to keep weight records of animals undergoing inoculation, regulating the dosage according to the gain or loss in weight. In case of serious loss of weight after an inoculation it may be necessary to resort to an emergency bleeding even though the period of immunisation is not complete, or better to inject a very small quantity of antigen, sufficient to desensitize enough to prevent anaphylactic manifestations in the subsequent inoculation, but not enough to cause a serious toxic reaction in the animal. In the case of suspected toxicity of the plant material, it is advisable to prëextract thoroughly or to dialyze the extracts before inoculation.

A section on the preparation of sera for plant serology would be incomplete without mention of the artificial sera or "Künstsera" devised by Mez (221, 390, 216, 225, 214) and used with apparent success by a number of his associates in the preparation of the Königsberg serosystematic genealogical tree of plants. The preparation of Künstsera rests upon Mez' assumption that "antitoxins are no more than de-toxified toxins" (Büchner), and that antibodies in general consist of the antigenic molecules modified by ferments present in normal blood serum. If this be true, then it might be possible to prepare antibodies by judiciously mixing and incubating antigens and normal serum. Mez' technique for the artificial preparation of immune sera is as follows (390):

> Three cc. of plant extract (1:200 solution of tissue powder) is mixed with 1 cc. fresh beef serum and allowed to stand 8 days at 35°C. with daily agitation. This mixture is then diluted with 20 cc. of phenolized (.5 *per cent) physiological saline solution and centrifuged clear.* Three cc. of fresh beef serum (complement) is added and enough NaCl solution to make a total of 30 cc. Such artificial sera are then tested for precipitin titer against the antigens employed in preparation and have been found to have titers of 1:3200 or 1:6400, and are claimed to be perfectly comparable to "natural" sera, or even more specific (221). To increase the titers of such sera one substitutes for the beef serum the serum of a rabbit which has been injected with a foreign, heterologous protein (e.g. milk) 12 hours previous to bleeding.

The Künstsera are held to be preferable to natural sera in that one avoids animal loss through toxicity of antigens, and that the same beef serum may be used to prepare many sera, a step toward homogeneity in serological materials. A patent has been applied for. A number of observed phenomena in serological (precipitin) routine have been explained by Mez in terms of the Künstsera hypothesis, such as the clouding of mixtures of normal serum and plant antigens after long-continued incubation, and the fact that in the preparation of Künstsera there first appears a precipitate in the incubated tubes (the action of the first, low-concentration artificial antibodies on the plant protein) which is later redissolved. A year previous to the publication of the first accounts of the Künstsera by Mez, Jena in Germany (139) obtained a patent for a comparable process for the preparation of "substances similar to immune bodies" (e.g. diphtheria toxin + pancreatin + inactive blood serum + 24 hrs. at 37° → antitoxin). Even as early as 1915 Ostromyslensky in Russia (265) had published enthusiastic accounts of the preparation of antitoxins *in vitro* by mixing serum globulin with toxin (botulism,

diphtheria, staphylotoxin), incubating, and then breaking down the new compound into antitoxin and free toxin by means of HCl. These antitoxins were claimed by Ostromyslensky to be indistinguishable *in neutralizing* and therapeutic effects from natural antitoxins. Kostoff (156) has mentioned, without reference, studies purporting to have obtained similar results in the production of antibodies *in vitro* by Kabilek (1927), Sdrawosmislow (1927), Baschkirtzev (1929), and Krishanovsky (1929).

The great success reported by Mez with artificial sera led to a number of attempts to confirm the work in other laboratories. Of the phytoserologists, Moritz (232), 230), Boom (36), Grijns (116), Nahmmacher (245), and Sasse (308), and among the animal serologists von Eisler (75) have all attempted to repeat Mez' work following his techniques in detail and with variations, and none has had much success in obtaining specific sera by this method. Sasse obtained 8 artificial sera in 26 trials, but the titers were low and the sera showed little safety for the study of plant relationships. Nahmmacher's results were similar. The sera obtained by Boom, Grijns, and von Eisler were entirely worthless serologically. Even Ankermann in Mez' laboratory at Königsberg, when speaking of using artificial sera in connection with serological work on the Monocotyledons, states that a number of the artificial sera were unsatisfactory. These attempts at confirmation appear on the whole to be both thorough and impartial. In particular this may be said of the work of Boom, who substantiates Mez in most particulars apart from the matter of Künstsera. Such being the case, it is extremely difficult to understand why this work is claimed to have such success in Königsberg. Perhaps, as Boom suggests, Mez and Ziegenspeck have not described the method adequately. Perhaps there is an element of truth underlying the conflicting reports, obscured thus far by inadequacies in technique. At least, however, one does not feel in a position at the present moment to recommend the use of Künstsera in plant serology as a technique for general application.

Preparation of antigens for testing. In general the same remarks as apply to the preparation of antigens for immunisation apply to the preparation of test antigens. Certain additional precautions must be taken, however, such as clarification, exclusion of artefact reactions, and adjustment to appropriate concentrations.

In the anaphylaxis and neutralization reactions it is not necessary to have clear antigens. In the other serological reactions with plant antigens, and above all in the case of the precipitin reaction, it is necessary to have extracts which are crystal-clear and which contain as little coloring matter as possible. This, with plant antigens is often a requirement difficult of fulfillment, and numerous techniques have been employed for clarification of extracts. Filtration through paper, particularly the hard papers, will sometimes suffice, and will often serve as a preliminary step. Where extracts are very turbid this paper filtration may be preceded by filtration through a coarser paper or gauze. With slimy extracts it is sometimes desirable to prepare a large excess of extract and to save and re-filter only the first portion passing through the filter. In addition to the paper, in refractory cases, use may be made of filters of asbestos, kieselguhr, or finely divided glass, using suction or pressure to force the liquid through. At times centrifuging will suffice for clarification, at other times this may be employed as a useful adjunct to other methods. It has been

suggested that in difficult cases one may adsorb the colloidal matter from cloudy extracts by means of animal charcoal or other finely divided adsorbant, but this is not recommended as a general practice because of the loss of protein necessarily involved. In fact, with any type of filtration one must count on a certain protein loss. It may be repeated here that thorough preëxtraction will aid in later clarification. An excellent technique for some proteins is the practice of alternately freezing and thawing several several times. The precipitate resulting from this procedure is easily thrown down by centrifuging and there is often no serious loss in concentration or volume of antigen. Another practice consists in discarding refractory extracts and the choice of closely related plant species for testing. Extracts of dried tissue powders as a rule are much more easily cleared than extracts or saps of fresh tissues.

. In immunisation it is customary to use antigens in as concentrated form as possible. In testing, on the other hand, it is better to dilute considerably. The students of plant serodiagnosis ordinarily use a concentration of 1:200 of plant tissue powder in physiological saline solution. Expressed sap, properly cleared, may be used without further dilution, and usually represents a concentration of about 1:50 to 1:100 in terms of dry weight. Saline or aqueous extracts of fresh tissues are proportionately weaker. Since with tissue powders concentrations are expressed in terms of dry weight, while with saps these are expressed in terms of the volume of the total sap, a titer of 1:100 with expressed sap is of the same order as 1:10,000 with powder extract or 1:100,000 with purified protein, since the protein constitutes only a small fraction of the dry weight. Since in plant serology the titers customarily observed with powder extracts are of the order of 1:10,000 to 1:50,000, and with saps of 1:100 to 1:500, these titers show an immunological sensitivity of animals toward plant antigens of the same magnitude as the titers usually obtained in serology with bacterial or animal antigens.

Normal serum frequently precipitates in the presence of plant antigens when both are at relatively high concentrations. Although this phenomenon has been attributed to so-called "normal precipitins," this term is better avoided because there is usually little or no evidence that the precipitations are qualitatively of the same type as those caused by immune precipitins, and in a number of cases there is definite evidence that such reactions are due to relatively simple chemical relationships between serum and extract, having nothing to do with antigen-antibody reactions in the accepted sense of the term. Thus, many plant proteins have isoelectric points definitely in the acid range, while serum proteins have alkaline isoelectric points. Under some conditions, the mixture of such will result in precipitation which bears no relationship whatever to specific serum reactions. Such normal serum reactions are also involved in other immunological techniques than the precipitin reaction, and frequently they are sufficiently strong to distort the results of specific reactions and must either be eliminated or taken into consideration in evaluating results.

With respect to the precipitin reaction three techniques have proven successful in eliminating this source of error.

In the first place, and simplest of all, one may dilute antigen or serum or both until no precipitate is observed on mixing and incubating with normal serum. Since the titer of specific serum is necessarily higher and usually much higher than the "normal precipitin" titer, the specific reactions are thus freed of any effect due to normal serum without affecting the utility of the specific reaction. Instead of pre-

liminary tests with all extracts against normal serum, it is customary in much of the phytoserological work to select an arbitrary dilution of antigen (e.g. 1:200 with tissue powder), and when this dilution does not eliminate all normal serum reactions all results are discarded using antigens more concentrated than the least concentration giving a positive test with normal serum. This is the customary technique in the Königsberg school.

A second technique has been utilized by Kōketsu (154, 153) and Kojima (152) in Japan, and consists in the treatment of each antigen with an equal volume of normal serum prior to testing. This antigen-serum mixture is incubated, then centrifuged, and the supernatant fluid, which no longer reacts with normal serum, is used in testing against immune sera.

A third technique as worked out by Hannig and Slatmann at Münster (118, 330, 119) and later used with apparent success by Bürger (39) and Bry (38) at Berlin, consists in the addition of phosphate buffers to each extract. Either a KH_2PO_4-K_2HPO_4 mixture or a solution of $H_3PO_4 + KOH$ is used, preferably with a pH of about 6.5 (e.g. 5 cc. molar $H_3PO_4 + 6.8$ cc. 1.5 molar KOH). In every case tested this eliminated the precipitation with normal serum, while numerous immune serum reactions remained. There is no doubt from the works of these various investigators that normal serum precipitates with plant antigens may be eliminated by the use of such phosphate buffers. On the other hand, Hannig and Slatmann report numerous cases where immune serum was negative with a given antigen until phosphate was added, when a strong reaction appeared. These investigators have reported numerous serological reactions which are not in accordance with the accepted systematics of the plants studied and rather curiously such non-specific reactions involve for the most part combinations of serum and antigen in which the combinations were negative until phosphate was added. Perhaps the phosphate, in addition to eliminating the normal serum precipitates, causes some sort of artefact precipitation, and if so the method is certainly to be avoided until more is known of the effect of phosphate.

Hiki in a study of the serological reactions of bean extracts (125) found that the normal serum reactions customary with ordinary techniques were completely eliminated if the extracts were exactly neutral, while specific immune serum reactions were unaffected. It remains to be seen whether this technique will be of general use, but the evidence of Hiki shows that in one case at least the normal serum reaction has nothing to do with precipitins in the usual sense of the word, since these are relatively independent of pH within a broad range on both sides of neutrality.

Altogether the methods of dilution or absorption with normal serum seem preferable to other methods for the elimination of normal serum reactions. Mez objects to the absorption method on theoretical grounds, namely that the addition to plant extracts of normal serum is likely to result in the production of artificial antibodies *in vitro*, but it would seem that the evidence for such an objection is weak, particularly as an hour or two in the incubating oven is sufficient for the absorption of the normal serum reaction.

III. PRECIPITATION REACTIONS

The precipitin reaction in various forms has been used more extensively than any other serological test in the study of plant antigens. Briefly, the test consists in the addition to plant extracts of immune serum, the two constituents being either overlayered one upon the other (ring test), or mixed (flocculation test), a period of incubation, and the reading of the precipitate formed. An immune serum may be permitted to act fully and consecutively against two or more antigens (absorption test), the precipitate being removed by centrifuging after each admixture. Such a technique is useful in determining whether two reactions of the same serum are identical or whether the antibodies involved in the precipitation of an heterologous antigen are qualitatively the same as those involved in the homologous test. Ideally the amount of precipitate is greatest when a serum is added to its homologous antigen, less with closely related antigens, and negative with more distantly related ones. The variations of

technique in different laboratories are considerable.

The flocculation test. The older, flocculation test has been used to greatest extent in the Königsberg school. As performed in Königsberg the procedure is as follows (210, 211, 390): Crystal-clear antigen solution (1:200) is further diluted with physiological saline 1:400, 1:800, . . . 1:51,200. To 1 cc. of each antigen dilution in a test tube of 10 mm. internal diameter is added .1 cc. of undiluted and untreated immune serum. The tubes are shaken and placed in an incubator at 37°. Readings of the amount of precipitate are taken after 12 hours of such incubation, at which time the flocculation has settled toward the bottoms of the tubes. The antigenic solution may be diluted as above, the serum being held constant, or the serum may be progressively diluted, the antigenic concentration being held constant. As the reaction is independent of complement, the serum used need not be fresh, and it may be inactivated at 57° for 1/2–1 hour without detrimental effect on the reaction.

The Königsberg school uses the following system of controls:

1. 1 cc. of each antigen dilution (or often only the dilution 1:1000) is tested with .1 cc. of normal serum.

2. .1 cc. of each serum is tested with 1 cc. of antigen diluent (saline solution), and 1 cc. of each antigen dilution with .1 cc. of the same.

3. All readings are made "blind", i.e. the observer deals only with numbered tubes the contents of which are unknown to him.

4. All reactions for use in the study of plant relationships are further safeguarded by the following requisites (214): (a) Relationship reactions between two plant antigens must be reciprocally confirmed. (b) The results of precipitin testing must be confirmed by those of the "conglutinin" reaction performed simultaneously on the same material. (c) Plants of undoubted close affinity must show serological affinity and plants undoubtedly not closely related must not show affinity serologically. (d) Plants which are known definitely to be intermediate between others must be intermediate serologically. (e) The summation of all serological relationship reactions must form a logical system. (f) The systematic position of a group must be confirmed by numerous serological reactions proceeding from various reaction centers.

According to the Königsberg requirements, controls 1 and 2 must be negative, 3 must be complied with before an experiment is considered significant, and the qualifications of 4 must be fulfilled before a serological system of plant relationships may be considered valid.

A similar technique, with the important exception that the time of incubation may be greatly shortened with advantage, has been used in many other laboratories. One of the chief differences in technique among different workers involves the method of evaluation of results. In Königsberg it is most common to consider the last tube of a dilution series of antigen which shows a settled flocculation (not a uniform turbidity) as representing the titer of that antigen with the serum used, and the titer is the criterion of strength of reaction. A number of investigators, feeling that the titer alone does not give an adequate measure of the strength of reaction, have laid emphasis on the time required for the reaction to appear or to reach given strength (104, 4, 196, 8, 385, 291, 35, 225), and this alone, or in combination with the titer, is taken as the measure of reactivity of a given antigen-serum combination. Others have felt that neither the time nor the titer are so important as the volume of the precipitate formed at constant concentrations of serum and antigen (383, 260, 46, 347), and this may be fairly accurately observed by centrifuging the mixtures for a constant length of time and at a constant speed in narrow tubes of even diameter, and measuring the height of the precipitate column thus produced (260, 46, 347). Serologists in England frequently prefer to use a method of titration in which the tube of a titration series which shows the strongest

reaction is taken as an index of titer, rather than taking as a measure the most dilute mixture showing a reaction. This method of "optimum concentration" has an advantage when there is danger of a reaction being inhibited by excess of antigen (zone phenomenon), but appears to have no great superiority to the ordinary methods of titration with plant proteins, since the latter are rarely found in excessive concentration. Another method of determination of the strength of reaction is to use simultaneously sera of various titers prepared against the same antigen, for it has been observed (187, 104) that a short period of immunisation produces sera which react only with closely related species, while a longer period of immunisation enlarges the reaction range. Kōketsu (153) used an interesting method of correction of observed results. The amount of protein in the antigens was determined by Esbach reagent, and 1 per cent taken as a standard. The observed titer was then doubled if the protein test was half as great as the standard, and halved if twice as great, etc. Secondly, this corrected value was again corrected by a factor which would make all sera show the same titer against their homologous antigens. Meyer (209) used a simpler but similar type of calculation.

That so many methods have been devised for measurement of strength of precipitin reaction is indicative of the recognized inadequacy of any one. If a reaction is positive, many factors will determine the intensity of precipitation, and it may be that much of the precipitate consists of non-specific substances apart from antigen and antibody. In our present state of comparative ignorance of the mechanism of the precipitin reaction it seems rather futile to argue as to the relative value of the various techniques outlined. If, for example, any two reactions are so nearly alike that the results obtained by using time of appearance as a criterion differ from those using titer or amount of precipitate as a criterion, then one would not be justified in considering them significantly different. Particularly when dealing with such complex and unknown solutions as extracts of whole plant tissues one has little justification for reporting as significant any results which are not so well marked that significant differences are shown by all three criteria, time, titer, and amount of precipitate. As to the method of correction of titers used by Kōketsu, this must be considered as highly liable to error. We have already seen that one of his factors of correction depends on the Esbach test which may bear little or no relation to specific serological reactivity, while the second factor of correction, homologous titer, would perhaps better be treated by dilution of the sera to comparable reactivities.

The ring test. In performance of the ring test, tubes of relatively narrow bore (usually 3–4 mm.) are employed. A small amount of the heavier liquid is pipetted into the tube, and the lighter liquid pipetted carefully along the side of the tube so as to avoid mixing the two as far as possible. Ordinarily the amounts of serum and antigen-dilution are not measured accurately. Using the ring test, the antigen, which is customarily the lighter, is better diluted, while the heavier serum is kept constant in concentration. Gilg and Schürhoff in Berlin have called this the "capillary method", but the expression is misleading and should be abandoned because the tubes used (101) are 2–3 mm. in diameter and, thus, far from being capillary. The ring test is read as a rule at 20-minute intervals for 1–1½ hours at room temperature.

The Berlin school of investigators have found much fault with the flocculation

test as used in Königsberg, their chief criticisms being that the readings are too subjective as compared with those of the ring test, and that using Mez' prescription the normal serum controls very frequently show cloudings such that the experiment must be rejected. Later investigators are divided on this question, but an analysis of these points of difference brings out a lack of fundamental disagreement.

In the first place, Mez uses a length of time of incubation of serum-antigen mixtures far in excess of that usually employed by animal serologists. Using the flocculation test, an hour or at the most two hours in an incubator at 37° is sufficient to bring out fully any reaction which might occur. With 12 hours' incubation it is not suprising that artefact, normal serum reactions are common. Even serum and antigen alone are likely to auto-precipitate with such incubation. But while 1–2 hours at 37° is sufficient to permit the precipitate to form, it is not enough to allow it to settle sufficiently for an objective reading, and the only advantage of the last 10 or 11 hours of incubation is to allow the precipitate to settle. This last is accomplished to far greater advantage in a cold room, at a few degrees above freezing. Even the Berlin workers observed no normal serum artefacts before 6–7 hours at 37°, and using the combination of 1–2 hours at 37° + 12 hours on ice, such normal serum artefacts are not usually observed.

As to subjectivity, the chief factor involved is the cloudiness of unsettled, nonspecific precipitates in experimental or control tubes according to Mez' technique. Using the combination method described above, such cloudings occur but rarely, and the observed result is in the writer's opinion actually more objective than with the ring test, where the slightest irregularity in pipetting will alter the appearance of the rings.

It is convenient, although not commonly in practice, to perform the precipitin test in tubes about 5 x 50 mm. in size; .2 cc. each of serum and antigen dilution, accurately measured, are convenient amounts to use. It is a good practice to overlayer the two components in such tubes, read the ring test after 1 hour at room temperature, then mix and incubate 1 or 2 hours at 37°C., and then 12 hours in the cold, after which the flocculation reading is made. Although the ring test gives a useful preliminary reading, the final flocculation reading appears to be more decisive. Mixing of the contents of such small tubes may be accomplished with an apparatus made by removing from an electric door-bell the bell and hammer, and attaching a rubber stopper to the hammer-arm. The tubes are held momentarily against the vibrating stopper.

Mez' "conglutination" reaction. The "conglutination" reaction, so-called, has come to occupy a very important place in the Königsberg school of serodiagnosis and a word should be said regarding its performance and significance. The term "conglutination" has been used with three very different meanings in serology, and an effort should be made to restrict it. "Conglutination" was first applied to the normal agglutination of blood corpuscles by such substances as ricin, abrin, and crotin (e.g. 227, 28), which at a relatively high concentration cause a clumping of the corpuscles of many species of animals. Later Bordet, Gay, and Streng (Bordet and Gay, *Ann. Inst. Past.* 20, 1906; Bordet and Streng, *Ztbl. Bakt.* I, 49:260, 1909, *reprinted in* Bordet, J., Studies in immunity, Transl. F. P. Gay, *New York* (Wiley) 1909) described a reaction which they designated as "conglutination". Normal horse serum contains a weak agglutinin for corpuscles of various species. The addition of horse serum, corpuscles, and complement, however, produces at best

only a very weak reaction. To the system is added heated (56°) bovine serum, containing a substance called "bovine colloidal substance", or later, "conglutinin", which stimulates the complement to function sufficiently to induce powerful agglutination ("conglutination") often followed by hemolysis. The "conglutinin" is satisfactorily shown to be neither antibody nor complement, but merely accessory to complement, and is peculiar to bovine blood.

Although Mez has taken over the term "conglutination", his reaction bears no relation to either of the above (cf. discussion in 194). The Mez reaction is performed as follows (210, 211):

To tubes containing 1 cc. of plant extract are added respectively .08, .02, .01, .005, and .000 cc. of immune serum. The tubes are shaken and incubated 2 hours at 37° during which time the mixture is said to become "sensitized". A precipitin reaction does not take place because of the small amount of serum used. Then .4 cc. of fresh beef serum is added to each tube and the tubes are returned to the incubator. A precipitate soon appears, and is read at 20, 40, 60, 90, 120, and 150 min. from the time of the second mixing. The usual types of controls are employed, and the experiment is discarded if any show turbidity. Ziegenspeck (390) later suggested using as serum series: .1, .05, .025, .015, .0625, and .0000 of serum in place of the older series used by Mez. The beef serum must come from a healthy animal because otherwise protective ferments cause a clouding of the controls. Reading is difficult and special illumination of the tubes is recommended.

Mez looks upon this reaction only as a technical variation of the precipitin reaction ("Sie liefert aber die gleichen Ergebnisse und wird sich wahrscheinlich nur als methodologische Variation herausstellen." . . . 211). And indeed this is true as Mez performs the reaction, the beef serum yielding some substance which increases the reaction without apparently changing its specificity, perhaps only adding colloidal material which suffices to make a subvisible precipitate visible. But to look on this reaction of Mez' as homologous with the reaction of Bordet, Gay, and Streng is an error. Manteufel (194) suggests that this be called the "Mez reaction", reserving the term "conglutination" for the Bordet-Gay-Streng reaction, and this seems justifiable especially as the original use of the term for the agglutination of corpuscles by toxalbumins has never come into general use.

With but rare exceptions, Mez' reaction in Königsberg has yielded results entirely comparable with those by the flocculation technique. The rare cases of disagreement are usually traceable to an error in procedure and are discarded. Mez insists that both reactions be performed on all materials for relationship study, and that the two reactions must agree. In Berlin the method has been criticized by Helwig (123), Franz (96), Eisenträger (74), Wermund (371), and Zarnack (387), all on the grounds either that it was impossible to obtain error-free results because of clouding of control tubes, or that if well-controlled results were obtained they did not agree with the results of the ring test. This failure in confirmation may in part at least be due to the fact that the Berlin techniques did not follow those of Mez strictly with particular reference to preëxtraction of antigens, and hence the work may not be comparable. Sauli working independently in Helsingfors, previous to the work of the Königsberg school (309) but using a similar technique found the method to be entirely satisfactory and more delicate than the precipitin technique. The writer has had occasion to use Mez' reaction in parallel with the ring and flocculation tests of plant virus material (50). The technique of Mez was followed closely. The results were entirely comparable in all three tests, and there was no trouble with turbid controls, but the Mez reaction was no more

sensitive than the other two, and that being so it seemed preferable to avoid the additional chance of error when with Mez' technique another variable is added (beef serum) to an already complex system.

IV. ANAPHYLACTIC REACTIONS

The anaphylactic reactions have been used frequently in plant serology, the various forms of the reaction all being dependent upon the fact that if the inoculation of an animal with a small amount of protein is followed 2–3 weeks later by a second inoculation of the same protein, more or less violent symptoms result, frequently terminating in death. In simplest form the anaphylaxis reaction consists in two such inoculations of the guinea pig, the reaction observed being severe shock following within a few minutes after the second inoculation (gross-allergy test). The sensitizing inoculation may be by various routes, the subcutaneous and intraperitoneal routes being frequently employed, while the second, test inocu lation is more frequently by the intravenous or intracardial routes. The amounts of protein required are exceedingly small, and the reaction is highly specific for the protein originally employed.

The gross-allergy test. Much of the immunological work on the purified proteins of plants by Wells, Osborne, and their colleagues was performed by making use of the gross-allergy test (361, 365, 366, 364, 374, 367, 368, 369, 382, 140, 175, 263). The amounts of protein required for sensitization of guinea pigs were of the order of .001 gm. to .0001 gm. injected subcutaneously, and after 18 days severe specific reactions usually resulted from the subcutaneous injection of .1 gm. Some of the plant proteins used were so highly active that but .000,000,5 gm. (squash-seed globulin, 364), or even .000,000,1 gm. (edestin, 364) was necessary for cutaneous sensitization. Certain proteins such as gliadin, on the other hand, were much less active, presumably because of their poor solubility in the body fluids (361). Tomcsik and Kurotchkin (348) have shown that anaphylactic shock may even be obtained with a purified carbohydrate from yeast, provided the animal is sensitized passively with serum prepared from whole yeast extract.

Many other investigators have used the gross-allergy test with the more complex extracts of plants, such as the grains and legumes (287, 298, 12, 349, 125, 132), and the fungi (60, 84). The reaction has been used to determine the presence of contaminants in fodder (315, 314), and for the identification of oils through the type of their protein contaminants (194, 351). On the whole, the reaction, while entirely useful, is not considered quite as efficient and delicate as the uterine-strip anaphylactic reaction or the complement fixation test, the chief disadvantages being that an animal can be used but once, and that the observation of the reaction may be obscured by other physiological phenomena, e.g. the direct toxic effect of the foreign substance.

Temperature, local skin, and bronchospasm reactions. In order partly to avoid this last difficulty, it has sometimes been the practice to keep an accurate temperature record of the animal at the time of the second inoculation, the temperature fluctuations being indicative of specific reaction. This technique has been used to some advantage in connection with plant antigens by Kôketsu (153), although other techniques were preferred because of the primary toxicity of many plant antigens. Coons and Strong (60), on the other hand, had no success with temperature as an index of shock.

Link and his associates (180) have reported the use of local skin tests for a study of allergic reactions to fungous extracts, and as is well known this type of test is one of the leading techniques used in the detection of specific sensitivities of hay-fever patients. Given an anaphylactic hypersensitivity to a plant protein, the rubbing of that protein into the skin results in a local inflammation; the reaction is highly specific. Thus the nature of hay-fever sensitivity is determined by making skin tests of possible sensitizers, until the specific one is found. The conjunctiva of the eye and the mucus membranes also show a high degree of hypersensitivity and may be used in the detection of specific sensitizers (68, 382, 57, 69). Workers who handle satin-wood (*Fagara flava* of the Rutaceae) show a similar type of local reaction to products of this wood (360). The use of skin tests in detection of the cause of hay fever, the nature of dermatomycoses, etc., will be discussed in a subsequent section.

A third form of anaphylactic reaction is the bronchospasm test, used by Wells and his associates in the study of purified vegetable proteins. The reaction is based on the fact that the second administration of antigen to a sensitized animal gives rise to acute, involuntary spasmodic contractions of the smooth muscle of the respiratory tract. These are recorded on a kymograph drum.

Passive anaphylaxis. Passive anaphylactic sensitization is attained a few hours after the intraperitoneal or intravenous inoculation of a guinea pig with serum immune to a plant protein or tissue extract. The method has been used with success by a number of workers in plant serology, viz. Wendelstadt and Fellmer with legumes (370), Karasawa with legumes and grains (143), White and Avery with edestin (374), Lake and others with hordein and gliadin (168), Fellmer with fungus extracts (84), Dunbar with hay-fever serum (69), and Moritz with legumes (234). The usual dosage is approximately .5 to 3.0 cc. of immune serum followed 24 hours later by .05 to .1 gm. of purified protein or 1–2 cc. of plant extract at moderate dilution. White and Avery (374) find the method even more sensitive than active anaphylaxis.

Schultz-Dale technique. The most sensitive and satisfactory form of anaphylactic test is that devised by Schultz and Dale. Adequate descriptions of the rather exacting technique will be found in Dale, H. H., *Jour. Pharmacol.* 4 (1912), Dale, H. H., *A System of Bacteriology (Med. Res. Counc.)*, 9:229–235, and with respect to plant antigens, Moritz, O., *Planta* 7 (1929): 759–814. Briefly the reaction is performed as follows:

Virgin female guinea pigs are sensitized with plant protein, actively or passively, in the usual manner. For active sensitization the pigs should not weigh more than 125–150 gm., for passive, 250–300 gm. After the customary incubation period (3 weeks active, 24 hours passive) the animal is stunned by a sharp blow at the base of the skull, the throat is cut, and the blood drained for a few minutes. The abdomen is then opened, the viscera laid aside, and the two horns of the uterus dissected out. To each end of each horn is attached a thread. Each horn is then placed in a Ringer solution bath at 37°, the lower end of the muscle fixed rigid, the upper end attached by a lever to a kymograph needle. The muscle is continuously aerated by a stream of oxygen or air. After about half an hour of rest, a quantity of non-toxic liquid antigen is introduced into the bath. A positive anaphylactic reaction is manifested by a rapid contraction of the muscle, which slowly relaxes again. As the same muscle will react successively several times to different antigens, the reaction may be used as an absorption test.

A word is necessary as to the use of plant antigens with such a technique. Many plant antigens are more or less toxic to muscle and will induce non-specific

contractions of the muscle even when the antigens are at low concentrations. The writer found this particularly true of Solanaceous extracts, which in dilutions of 1:1000 or more caused such non-specific reactions as to render the technique useless without their elimination (53). A satisfactory technique consisted in a few hours' dialysis of such extracts against Ringer solution, in collodion ("cellophane") bags with continuous agitation. Such dialysis serves the several purposes of removing crystalloid toxic material, of replacing for the unknown and variable salt constitution of the antigenic extract the balanced salt constitution of Ringer solution in which the muscle is bathed, and of establishing a constant pH in muscle bath and extract. With the added precaution of warming the extracts to 37° before adding to the bath, it is possible to use an antigen concentration (in terms of whole plant sap) of 1:10 without production of artefact reactions.

The Schultz-Dale technique has been used most extensively with respect to plant antigens by Moritz (230, 232, 233, 234, 235, 24, 236, 237, 238), particularly for the purposes of studying the genetic constitutions of plant hybrids, the movement of proteins in plants, and the systematic relationships of plants. It has also been found to be of service in plant serology by Elmore in studies of algal diagnosis (81) and by Wells and his associates (175) in their immunological studies of purified plant proteins. It has been particularly useful in connection with the purification of plant viruses, since the viruses themselves show no anaphylactic reaction, while the ordinary contaminants of purified virus preparations are highly anaphylactic. Recrystallized tobacco mosaic virus protein has thus been shown to contain non-virus proteins of the healthy tobacco plant (53).

V. LYTIC REACTIONS

Normal hemolysis. Plant extracts frequently show the ability to hemolyze the washed red corpuscles of various types of blood. This is true of many of the so-called "phytotoxalbumins" (see discussion and literature references in Part II) and of extracts of numerous plants such as *Datura* (76), *Salpiglossis* (76), *Sapindus* (148), *Agrostemma* (148), *Herniaria* (148). *Yucca* (148), and tomato (50). Even methyl alcohol extracts of numerous species yield normal hemolysins, e.g. *Salix*, *Larix*, *Eucalyptus*, *Ginkgo*, *Paulonia*, *Podocarpus*, *Evonymus*, and *Brassica* (246), the active principle being attributed to chlorophyll and xanthophyll. On the other hand, hemolysins have been searched for but not found in numerous other species of plants.

The hemolytic titers of these normal plant hemolysins are often exceedingly high. Thus cyclamin lyses corpuscles completely at 1:100,000, and partially at 1:285,000 (350). Other plant substances which have titers of 1:100,000 or higher are quillaja acid (350), solanin (271), phallin (148), crotin (79), and digitonein (148). Such titers are customarily determined by adding 1 cc. of plant extract dilution to 1 cc. of washed corpuscles of various species, the corpuscles being at a concentration of 5 per cent (or better 2 per cent) in isotonic saline suspension. Hemolysis is observed after one or more hours at 37°.

Anti-hemolysins. If an animal be carefully immunized with the plant hemolysins, antibodies are frequently produced which are capable of inhibiting normal hemolysis. Such anti-hemolysins, for example, follow immunisation with ricin and crotin (134, 135) and with extracts of the fleshy fungi (97, 86, 93). The anti-hemolytic titers reach 1:1000 or more.

Even normal blood serum has some anti-hemolytic power as regards the normal hemolysis of solanin, cyclamin, saponin, and digitalin (18, 20). The immunisation and testing for anti-hemolysins are performed according to the customary techniques, the only qualification necessary being that because of the toxicity of such antigens one must begin with very small dosages, increasing as the animal develops resistance.

Acquired lysis. As a result of immunisation with particulate plant antigens it might be thought possible to obtain sera which are lytic to such antigens. However, the various attempts which have been made in this direction have not succeeded. Elmore found that a chlorolysis of *Euglena* and other plant cells occurred in the presence of normal human and rabbit serum, but this was not increased on immunisation (80, 308b). The action was associated with the lipoids of the serum and appeared to require lipoids in the cells. Similar results have been obtained with pathogenic and non-pathogenic fungi (56, 192, 34). In the cases mentioned, however, a production of acquired agglutinins or of protective substances did occur. Cao (42) reported an unusual type of lytic effect of starch grains following immunisation with starch, the lytic effect being determined by titration with Fehling's solution. He found specific combinations more lytic than heterologous combinations, but the work requires confirmation, as the starches in general are not considered to be antigenic. Sherwood (326) found that while neither normal serum nor extracts of plantain leaves suffice to liberate pigment from washed chloroplasts of legumes, the combination of normal serum and plantain extract does produce such a lytic effect. This peculiar result has not been explained.

Abderhalden reaction. In the Abderhalden reaction, immune serum and antigen are mixed and placed inside dialyzing membranes, and the appearance of protein cleavage products in the diffusate, as determined by some delicate indicator such as "Ninhydrin" (tri-keto-hydrindene hydrate), is an index of the extent to which the serum has broken the antigen into simpler cleavage products. The reaction is delicate and difficult to perform. Controls are very important, the dialyzing membranes must be carefully tested, and according to most authorities the technique is not generally practicable.

The Abderhalden reaction has been used to detect acquired lysins to plant antigens in very few cases. Elsesser (82) found that in general sera immune to purified vegetable proteins reacted more strongly by this technique with the specific protein used in immunisation than with heterologous proteins, and if closely related antigens were used a relationship reaction was seen. However, exceptions to this situation occurred, and the technique seemed inferior to other serological techniques applied to similar material. Nitzescu (256), attributing pellagra to a maize intoxication, found protective lysins for zein (the alcohol-soluble protein of maize) in the bloods of 56 pellagra cases, while 12 healthy bloods failed to give the reaction, but Herzfeld (124) showed that many normal bloods also give positive Abderhalden tests with zein. Ishiwara (133) performed a number of Abderhalden experiments using denatured plant proteins as antigens. Although he succeeded in differentiating some of these, the results showed very poor and erratic specificity. On the whole, the results with the use of the Abderhalden technique on plant proteins are indecisive and the technique is not recommended for use in plant serology without further preliminary study.

Complement fixation. The technique of

complement fixation has frequently been used, and with success, in the study of plant serology. The arrangement is as follows:

1. Hemolytic system: Anti-sheep-corpuscle serum + washed sheep corpuscles → lysis of corpuscles.

2. Fixation system: Inactive anti-plant serum + plant antigen → union of antigen and antibody.

3. Complement: Fresh guinea pig serum.

Reaction 1, which is the observed effect, can occur only in the presence of complement, which is present in fresh serum but not present in the hemolytic system above, because complement is destroyed at 57°C. and the hemolytic serum has been heated to that temperature (inactivated). Complement has been similarly removed from the fixation system.

In performing the test, the materials for reaction 2 are mixed and incubated in the presence of a minimal amount of complement. If reaction 2 is positive, i.e. if one is dealing with an homologous serum and antigen, the complement is fixed or bound, and none is available as free complement. When the hemolytic system is then added, the sheep cells are not hemolyzed. If, on the other hand, the immune serum and plant are not homologous, they do not combine, complement is not utilized, and remains free permitting the lysis of the corpuscles of system 1. The following details of technique have been found practical in complement fixation with plant antigens.

Anti-sheep serum can be prepared by inoculating rabbits intravenously with daily inoculations of 1, 2, 2, 1, 2, and 2 cc. of 50 per cent washed sheep corpuscles in physiological saline suspension, a 5-day rest period elapsing between the 3rd and 4th inoculations. The animals are bled several days after the 6th inoculation. The serum is inactivated at 57° for 1/2 hour, diluted to 1/2 with glycerine, and stored on ice. Sheep corpuscles are prepared by defibrinating fresh sheep blood, washing and centrifuging with an excess of saline 4–5 times, and storing on ice where they keep satisfactorily for several days. The titer of the serum is determined by titrating with sheep cells + an excess of complement. The unit of anti-sheep serum is the minimal amount which will completely hemolyze a 2 per cent solution of sheep corpuscles, and 2 units are used in routine tests. Using 2 units of serum, complement is next titrated, the unit value established, and twice this amount used in subsequent tests. Complement must be freshly prepared and retitrated frequently, but the titer of the serum is unlikely to change with storage.

The plant antigen and the plant immune serum respectively are next added to sheep cells + complement, as well as to sheep cells + hemolytic serum + complement, respectively, to determine the highest concentrations at which these reagents will neither fix complement by themselves nor hemolyze corpuscles. In all future tests the concentrations of plant serum and plant antigen must be below these concentrations. Finally, in the test itself, plant immune serum and plant antigen are added to complement, incubated, and then is added a mixture of hemolytic serum and sheep cells. In all dilutions physiological saline is used as diluent; distilled water is hemolytic in itself. An extensive system of controls is necessary, since any one of the various components used may in itself either be hemolytic or inhibit hemolysis. Customarily the test is performed in Wassermann tubes which accommodate 4–5 cc. of fluid. A convenient arrangement is as follows: Inactive plant immune serum (.3 cc. at 1:10) + complement (.2 cc. fresh guinea pig serum at 1:15) + antigen (1 cc. at various dilutions), incubation for 1–2 hours at 37°, + .5 cc. sensitized sheep cells (1 part hemolytic serum dilution + 1 part 4 per cent washed sheep cells, incubated 1/2 hour at 37°), further incubation for 1 hour at 37°. A more extensive account of the details of the test is given in Taliaferro, W. H., *The Immunology of the Parasitic Infections*, New York, 1929.

Complement fixation is the most sensitive of all immunological techniques, but as yet it has not come into general use with respect to plant antigens, partly because of its complexity and unfamiliarity to plant workers, partly because the narrow specificity exhibited is not always suitable to plant relationship study. Yet wherever it has been used with plant materials the results obtained have justified the added labor and difficulties involved. This is particularly true of its uses in the study of fungi pathogenic to man and other animals, of the plant viruses, and of purified plant proteins, and the success thus far obtained justifies a more extensive usage in this field. To

be sure, the plant antigens present difficulties which are of greater importance with plant materials than with animal and bacteriological antigens. As has been noted in the preceding section, many plant antigens are so highly hemolytic in themselves as to preclude their use without further purification. Others in themselves are anti-hemolytic. Since a number of these disturbing principles are non-proteinaceous, a technique of dialysis through collodion might sometimes serve as a useful preliminary to complement fixation with plant antigens exhibiting such artefact reactions, and in the case of hemolytic plant proteins titration to favorable hydrogen-ion concentrations, such as proved successful in the case of the tomato hemolysin (page 89), might offer a solution to this difficulty.

VI. AGGLUTINATION REACTIONS

Normal hemagglutination. Red blood corpuscles are not only hemolyzed by many plant extracts, but are also frequently agglutinated by such extracts. This applies in particular to the phytotoxalbumins (see discussion in Part II), to the extracts of *Amanita* (88, 91, 93) and other fleshy fungi (94, 95, 97), to purified plant proteins such as edestin (374), and to the non-toxic extracts of many species of plants, (cf. discussion in Part II). Not only are corpuscles agglutinated by abrin, crotin, and ricin, but also pus cells and isolated cells of various animal tissues (173, 223). The titers of these normal plant hemagglutinins are usually much lower than those of the normal plant hemolysins, but have been occasionally ascertained to reach as high as 1:10,000 (edestin, 374) or 1:32,000 (bean extracts, 170). Use has been made of the normal agglutination by ricin in the detection of ricin contaminations in animal fodders, since the ordinary constituents of fodders do not show such an agglutinative action (227). The customary procedure (288) consists in the preparation of a .85 per cent saline extract of the dried plant seed, to which is added a dilute suspension (2 per cent) of washed blood corpuscles. Agglutination is observed either microscopically or with the unaided eye after an hour or more at room temperature.

Normal serum agglutination of plant antigens. Conversely, normal serum may cause an agglutination of particulate plant antigens, although the titers are usually rather low. Thus fungus spores and yeast cells are agglutinated by normal serum (41, 197, 240) and by peritoneal fluid (56), chloroplasts are occasionally weakly agglutinated by normal serum (326), and suspensions of gum arabic and plant oils show a reaction resembling agglutination in the presence of normal serum (352). Weak agglutination of *Euglena* cells (titer 1:50) by normal serum has been observed by Elmore (80), and the agglutinative principle was removed by heating the serum to 56°, but Steinecke (333) in a study of acquired agglutinins toward algae was not troubled by an agglutination due to normal serum.

Anti-hemagglutinins. By immunizing animals with the normal plant hemagglutinins, sera are often produced which inhibit the normal hemagglutination. This has been shown for the hemagglutinins of ricin (134, 135), abrin (287), edestin (374), and bean extract (287), although attempts to produce anti agglutinative sera against the *Datura* (76) and *Amanita* (93) hemagglutinins did not succeed. But in the case of the latter at least, the agglutinin is non-protein and of a rather low titer, hence the phenomena may not be comparable. The titers of such anti-agglutinative sera may reach 1:10,000 (edestin, 374). In acquired immunisation the washed corpuscles show

no resistance to agglutination by the specific antigen used, but such resistance as appears is confined to antibodies in the serum (227, 183).

Acquired agglutination. The inoculation of animals with corpuscular plant antigens, e.g. unicellular algae and fungi, usually gives rise to the production of agglutinins. In contrast to the other serological reactions with plant antigens, however, the agglutination reaction is usually weak and not highly specific.

Acquired agglutination of algae has been reported by Dunbar (in 300), Rosenblat-Lichtenstein (300), Lieske (176), and Steinecke (333). The highest titers of the immune sera have been reported at 800 (300), 4000 (176), and 12,800 (333). Complement is held to be unnecessary to the reaction (300). Green and colorless cultures of the same alga show limited (176) or no (300) cross reactivity, the reaction hence not appearing to be bound up with the species-specific elements of the protoplast. Either living or dead (56°) algae may be used and give identical results (176). The method is held to be unsatisfactory with large thick-walled algae.

As regards the fungi, acquired agglutinins have been demonstrated in the cases *Aspergillus* and *Mucor* spores (197, 41, 307), pathogenic and non-pathogenic yeasts (321, 323, 240, 64, 30, 193, 270, 344, 178), and species of *Monilia* (145, 178, 83, 126, 177, 207, 224). In general, such agglutinins have been produced by the artificial injection of laboratory animals, but they have also been detected in the blood of patients suffering from diseases due to pathogenic fungi (224, 83). The titers are often very low, of the order of 1:20 or 1:40 (41), 1:100 (240), 1:50 (307), 1:80 (64), and 1:90 (192), or less frequently up to 1:500 (145) or 1:5120 (207). Normal serum, as has been seen above, will not infrequently agglutinate such plant antigens at titers but little below these. Complement has been found to be unnecessary to the reaction (323). Either living or dead fungi may be used for immunisation (240), and yeasts may be heated even to 115° without interfering with the reaction (64, 193). The yeasts are not agglutinated in general by the non-specific substances which agglutinate bacteria, such as formalin, $HgCl_2$, and dilute acid and alkali, but acetic acid does agglutinate them (193). Minute amounts of calcium have been found to be essential to the reaction with yeasts (30).

It is very probable that in the agglutination of the algae and fungi the capsular substances are most important, the protoplast itself having little part in the process. This is in keeping with the facts that the yeasts may be strongly heated without altering their antigenicity, and that empty plasmolyzed and washed yeast capsules produce sera giving agglutination reactions identical with those prepared from whole yeast cells. An interesting mechanism of agglutination is seen in *Euglena gracilis*. When Sauer (308a) immunized rabbits with this alga she found in the serum a specific cytotoxin which caused the *Euglena* cells to discard their flagella and settle to the bottom.

The specificity of the agglutination test with these plant antigens is by no means as sharp as that with tests such as the precipitin and complement fixation reactions. Thus little or no differentiation has been obtained between species of *Monilia* (207) or between *Monilia psilosis* and the yeasts of sprue and blastomycosis and other yeasts (126). An interesting case of lack of species-specificity is that reported by Sugg and Neill (344), who obtained cross-agglutination and -protection tests between anti-yeast serum and pneumococcus Type II. This lack of species-speci-

ficity of the agglutination reaction with plant antigens may well be due to the same factors as have led to the view expressed above that the reaction in this case is one involving the relatively non-specific capsular substances and not the proteins of the protoplasts.

Uhlenhuth and Jung (352) have described a phenomenon resembling acquired agglutination occurring when immune serum was added to a suspension of olive oil and gum arabic or rubber sap. The reaction did not occur in the presence of normal serum, and it was not specific for the olive oil, other oils reacting with equal facility in the presence of anti-olive-oil serum. It is very likely that in this case the pure oils in themselves were not antigenic, in the sense of immunizing, but that in the presence of protein admixtures or contaminants immune sera were produced which reacted with the oils employed (haptene or "Schlepper" theory). Citron (56), while unable to demonstrate acquired agglutinins to a species of *Trichophyton*, observed what he considered to be an agglutination of the fungus *in vivo* in the peritoneal fluid.

The technique of agglutination testing consists in the preparation of immune sera preferably by intraperitoneal injection to avoid the danger of embolism following intravenous injection, and the addition of a small amount of serum dilution to a fairly dilute saline suspension of antigenic material, the observation being made of the sedimentation in the test tubes, or perhaps more favorably of the clumping as observed through the microscope on a hollow-ground slide.

VII. ACQUIRED RESISTANCE TO PLANT PATHOGENS AND TOXINS

Besides the demonstration *in vitro* of immune reactions in animals toward plant antigens, there is a considerable body of evidence showing that there may be acquired a resistance to toxic plant products or toxic antigens from pathogenic plants, demonstrable *in vivo*, and that such acquired resistance may be accompanied by or due to the production in the animal of immune substances which inactivate or neutralize the toxins, or kill the pathogens involved.

Acquired resistance to plant toxins. The injection into animals of progressively increasing sub-lethal doses of plant toxins frequently produces an immune state such that doses of the toxin far in excess of the maximum sublethal dose for a normal animal may be borne with impunity. This has been demonstrated for the residual toxins in cultures of pathogenic yeasts (305) and *Actinomyces* (252), for the phytotoxalbumins, ricin, abrin, crotin, and robin (cf. Part II), and for the toxins of *Amanita* (86, 88). The toxins mentioned above, with the possible exception of one component of that from *Amanita*, are proteinaceous. On the other hand, animals which have recovered from sublethal doses of the highly poisonous crystalline glucoside of the "Tutu" plant (*Coriaria thymifolia* and *C. ruscifolia* of New Zealand) show no subsequent resistance (92).

While an immunity to the toxins of *Actinomyces* is held to be accompanied by an immunity to the fungus itself (252), this is denied for pathogenic yeasts (305). Immunity to plant toxins may be acquired either by injection or by feeding of the toxin, and the degree of immunity is frequently fairly high. Thus it has been shown that rabbits immunized with ricin either *per os* or by subcutaneous injection may later resist as much as 400 to 600 minimal lethal doses (134, 135) and the same has been shown for abrin (72). With abrin (57) it has been observed that the immunisation of the conjunctiva of the

eye is followed by a local immunity confined to the immunized eye, which later develops into a more general immunity toward this toxin. Acquired resistance to ricin may last as much as 7½ months after immunisation (72).

Protective, neutralizing, and lethal substances in the blood of animals immunized with plant toxins or pathogens. Neutralizing antibodies specific for the toxins used in immunisation have been detected in the blood of animals immunized with a number of the phytotoxalbumins (cf. Part II). Furthermore, the immunisation of animals with certain plant pollens results in the production of sera which are antitoxic to the hay-fever toxins contained in such pollens (68, 69). The titers of such sera are reasonably high, e.g. neutralization of 100 normal minimal lethal doses of ricin by immune rabbit serum (183). The antitoxin may persist in the blood for some time after immunisation (72). The neutralizing antibodies for ricin and the *Amanita*-toxin are present in the blood stream but are not demonstrable in the serum-free red corpuscles (183, 134, 86).

It is questionable whether the sera of animals prepared by immunisation with yeasts have a lethal or inhibitory effect on the yeast cells (193, 30). This is not unexpected, since, as was seen above, the antibodies produced by the injection of yeast cells are directed at the wall substance of the cells and not at the protoplasts themselves.

Sera prepared against pathogenic species of *Trichophyton* (56), *Saccharomyces* (305), *Aspergillus* (61), and *Monilia* (177) in no case protected other animals against single minimal infective doses of these pathogens, and in the case of *Actinomyces gypseides* only an incomplete protection has been claimed (252).

Acquired neutralization of the plant viruses. In the earlier literature on the serology of the plant viruses there have been several preliminary reports of a neutralization of tobacco mosaic virus by immune rabbit serum (244, 279, 280, 200, 328, 106). Normal rabbit serum, however, also was seen to have a strong inhibitory effect on virus infectivity, and accordingly it was not clear whether the effect of immune serum was actually qualitatively different from that of normal serum. It has recently been shown (47) that normal serum inhibits virus infectivity not principally through an effect on the virus but through a decreasing of the susceptibility of the host plant, while virus-immune serum in addition has a neutralizing effect on the virus. This neutralization is entirely specific as between the viruses of tobacco mosaic, tobacco ring spot, and cucumber mosaic, and the reaction follows the law of multiple proportions. Time is required *in vitro* for its demonstration. The serum titers are relatively high, 1:2,500 or more. Stanley has recently shown (332b) that virus protein which has been inactivated so that it no longer possesses infectivity will still elicit neutralizing antibodies in rabbits. If neutralized mixtures of tobacco mosaic virus and its specific antibody are digested with pepsin, the antibody is destroyed and the virus antigen recovered, while if similar mixtures of latent mosaic virus and its antibody are acidified, the converse is true, the virus being destroyed and the antibody recovered. Thus in these cases of neutralization the virus and antibody, respectively, are not destroyed but are held in a latent condition from which they may be freed by suitable treatments (55).

VIII. OTHER SEROLOGICAL TESTS WITH PLANT ANTIGENS

A number of other less usual serological techniques with plant antigens have been attempted, in some cases with positive

results, and these are here summarized briefly.

Phagocytosis and opsonic index. Skchiwan (329) has noted that the injection of yeasts into the peritoneum of rabbits is followed by an attempted ingestion of the yeast cells by leucocytes. The yeast cell responds by secreting a thick protective membrane, and retains its viability for several days. In intravenous injection, however, the yeast cells are quickly killed by phagocytes. Similarly, Citron (56) found that leucocytes protect normally against injected *Trichophyton*. The inoculation of pathogenic yeasts, according to Peckham (270) is followed by an increase in phagocytic activity as measured by the opsonic index, and this acquired reaction is demonstrable before the complement fixation and agglutination reactions appear. Porges (275) claimed that active homologous serum increases the opsonic effect in the phagocytosis of starch, but that this was not increased by immunisation with the starch. Inactive sera, on the other hand, inhibited such phagocytosis. These last results are difficult to interpret with no more than the scanty data offered regarding this apparently complicated condition. The increase in opsonic index with immunisation by bacteria is a regular concomitant of the acquired serological reactions, and although this reaction would have no general applicability with the plant antigens, it would be instructive to have more evidence to add to the limited findings noted above, with particular regard to the yeasts and yeast-like fungi, the unicellular algae, and possibly the pollens of plants.

Effect of immune serum on the germination of seeds and the growth of plants. Raubitschek (287) has reported that sera immune to the lentil and bean proteins often check the germination or growth of the homologous seeds or seedlings in a specific fashion. However, many of the sera obtained by him failed to exert such an action, and furthermore Kubeš (165) found that immune serum was no more effective than normal serum in preventing the germination and growth of poppy, clover, bean, and other plants. The scanty data thus far adduced accordingly do not furnish any reliable evidence that immune sera are able to act in the manner in question.

Decoloration of plant dyes by dye-immune sera. De Angelis (5) has claimed that the injection of animals with the plant dye hematoxylin (as well as with methylene blue) results in the production of sera which not only precipitate solutions of this dye but also decolor the solutions. The work has been repeated by Takemura, however (345), and no evidence was found that such a decoloration occurs on the addition of hematoxylin-immune serum to solutions of the dye, nor indeed were any precipitins demonstrated.

Pfeiffer's test. Lieske (176) in a serological study of the algae attempted the application of the Pfeiffer test to his material. Briefly, the test consists of a lytic demonstration in which organisms and immune sera are injected into the peritoneum of guinea pigs and the peritoneal exudate is later studied microscopically. He was unable, however, to obtain useable results by this method, although his sera were satisfactory as shown by other types of serological tests.

Anti-diastatic power of yeast-immune sera. Malvos (193) observed that sera from animals immunized against yeasts had no power of restricting or inhibiting the diastatic activity of the yeasts.

Coagulating effect of plant toxalbumins on normal blood and other animal proteins. In addition to the hemagglutinating, hemoprecipitating, and hemolytic property of the phytotoxalbumins it has been found that ricin, abrin, and crotin also stimulate

the coagulation of normal blood (122, 173). Cytisin does not exert such an effect (285). Likewise abrin coagulates normal cerebrospinal fluid (122), and crotin, ricin, and abrin curdle milk, although they have no such effect on such other animal proteins as myosin, meat-broth, and ovarial mucin (173).

(To be continued)

SURVIVAL OF THE ORDINARY

By W. L. McATEE

INTRODUCTION

NATURAL selection theory has been so frequently criticized that it seems well nigh impossible to bring forward an entirely new argument. Perhaps by presenting matters in a fresh light, however, and even by iteration alone, the attention of open minds may be focussed on some of the fatal weaknesses of the theory.

The thesis of the present paper is not original but it was independently reached and has been the subject of reflection for years. I find it fairly well stated in a letter written in 1917 from which the following quotation is taken.

> "I am unable to put much faith in natural selection however, and none in that phase of it expressed by the phrase 'survival of the fittest.' It seems Quixotic to assert, that among the very large numbers of offspring, produced by most animals, only the fittest survive. Chance enters into the equation so largely that the fittest stands a proportional chance of being the first eliminated. It seems to me that the survivors will almost invariably come from the great median group of ordinary specimens, and not from either the small proportion of subnormal or of supernormal individuals. In other words, natural selection will usually leave typical specimens to reproduce a species, and is a conservative rather than a progressive process."

About the only change I would make in that statement today would be to quote "natural selection" as well as "survival of the fittest," as both of these terms are are mere slogans used almost invariably without the slightest analytical perception. A fair interpretation of typical assertions might read: "Mortality occurs; there are survivors; natural selection therefore has been effective, and the survivors are the fittest." As has been pointed out on various occasions this is quite irrational, and means nothing but that survivors survive.

"SURVIVAL OF THE FITTEST"

Darwin uses the phrase "survival of the fittest," or equivalent words, less than half the times he defines the expression "natural selection," yet on those occasions he does it so deliberately and definitely that we must assume the synonymy was satisfactory to him. For instance near the beginning of Chapter III of the "Origin of Species" he says: "I have called this principle by which each slight variation, if useful is preserved, by the term natural selection in order to mark its relation to man's power of selection. But the expression often used by Mr. Herbert Spencer, of the Survival of the Fittest, is more accurate, and is sometimes equally convenient." (1912?, 55).

In this statement we have the implication that a slight useful variation puts its possessors in the ranks of the fittest, while as a matter of fact it would certainly place them in the median or average class to which considerable attention is given later. It would take more than a slight favorable variation among omnipresent slight neutral variations to move an organism from near the median state of its kind to the superior status implied by the word "fittest."

For the present, however, let us contemplate the meaning of the full phrase "survival of the fittest." *Fit-test* is a superlative; it means the best, the very best. As a prime consideration we must not forget that organisms are prolific,

many of them prodigally so, yet no matter how large the number of offspring, only enough survive on the average in each generation to replace their parents. Suppose this number is two as it is in so many cases, then "survival of the fittest" means that the very best two out of a usually large, often enormous, number of offspring are "selected" to perpetuate the race. What the probabilities of this outcome are may be judged to some extent from the mere numbers involved.

FECUNDITY OF ORGANISMS

Statements of the numbers of seeds, eggs, or young are available in many places but it will do no harm to cite examples. Prof. O. A. Stevens studied the seed production of numerous weeds and reports (1932) the number of seeds produced by an average individual plant to be 1000 or less in the case of 23 species, "1000 to 10,000, 86 species; 10,000 to 50,000, 56 species; 50,000 to 100,000, 9 species; above 100,000, 9 species." The highest number revealed by this study was 1,075, 000 for *Artemisia biennis*. A single *Paulownia* tree has been estimated to produce 21,000,000 seeds, while the numbers of reproductive bodies borne by orchids and ferns reach much more enormous numbers as 74,000,000 seeds for a plant of the orchid genus *Acropera*, and spores in the following numbers for ferns of the genera named: *Dryopteris* 50,000,000 to 100,000,000 (from the whole plant in a year); *Aratia* 2,800,000,000 and *Angiopteris* 4,000,000,000 (in each case the produce of a single leaf!).

Turning to the animal kingdom, the egg complement in our common frogs of the genus *Rana* ranges from 2,000 to 20,000; some of the stone-flies deposit from 5,000 to 6,000 eggs; dragonflies 5,000 to 11,000; and ticks 16,000 to 17,000. Presumably all of these are seasonal records; as an upper record for an insect the queen of an African species of termite is reported (Savage, 1850) to lay 80,000 or more eggs daily.

The lobster at its most productive period is said to bear 160,000 eggs at a time. Fishes are noted for their fecundity, and a good many kinds attain or surpass the million mark in eggs; angler 1,000,000 to 3,000,000; cod 2,000,000 to 9,000,000; coalfish 5,000,000 to 8,000,000; turbot 5,000,000 to 30,000,000; and the ling 12,000,000 to 60,000,000. The egg urchin may produce 20,000,000 eggs in a season and some kinds of starfishes twice as many. According to Weismann "Leuwenhoek calculated the fertility of a threadworm at sixty million eggs, and a tapeworm produces hardly less than 100 millions." (1904, 47).

HIGH RATE OF MORTALITY

However great the number of offspring, there survives on the average only one to replace each parent. The mortality rate, therefore, high in almost any case, is stupendous where such large numbers as those here cited are concerned. The average gross mortality in each generation is the means through which "natural selection" is conceived to operate. Apparently selectionists have assumed that high mortality automatically involves high selectivity. That this is merely an assumption is readily proved by analysis of the process of elimination.

A principle, almost universally evident, is that mortality varies directly with the immaturity of the forms involved. Thus there are, as a rule, heavier losses among ovules and pollen, among unfertilized eggs and sperm, than among any of the products of union of the gametes and there is higher mortality among seedlings and larvae or similar early stages than among adults. In fact until the approach of

senility, maturity brings a proportionate degree of security. The life-histories of almost any organisms illustrate this principle but those of highly prolific forms do so most clearly and emphatically. To those who see in this process only survival of the fittest we suggest the thought that if immaturity is to be interpreted as unfitness then the survival of the fittest evidently is not the ruling principle of evolution as adults regularly give way to young, and in innumerable cases (e.g. insects in general) completely disappear leaving the fate of the species entirely with the immature. Moreover it is not a comforting thought for selectionists that while a very great share of "selection" affects the immature forms, in proportion to the degree that this occurs, there will be just that much less selection of adults upon which continuation of species depends.

THE PROCESS OF ELIMINATION IN PLANTS

Oaks. Beginning again with plants consider the case of the oaks. The first "selection" (an abominably misused term) in the reproductive process each year (or two years) is among the ovules and pollen grains. Do the fittest of these necessarily participate in the formation of the acorn crop? The ovary has three cells, each with 2 ovules, and in maturity 2 of the 3 compartments and 5 of the 6 ovules become obliterated (Lindley and Moore, 1876, II, 948). Here is an elimination of 5 out of 6; in addition the pistillate flowers must share in the casualties to twigs, mentioned later, so that the proportion of survivors among the ovules is small, and their survival a process in which chance plays a far greater part than degree of fitness.

From examinations kindly made for me by A. C. Martin in April 1933, anther sacs of a specimen oak in the Mall, Washington, D. C., averaged somewhat more than 1800 pollen grains each, and each stamen therefore, produced in excess of 3600 grains of pollen. The flowers had an average of 7 stamens and consequently over 25,200 pollen grains, while the aments averaged 33 flowers producing all together in excess of 800,000 grains. This many from a single catkin, and the latter are produced in such profusion that their dried and shrivelled remains after they have shed their pollen and fallen to the ground can be raked up literally by bushels. On a single small twig used in the study referred to, there were 62 aments, the yield of which in pollen grains must have been more than 50,000,000. With that number from one twig, what an enormous number—millions of millions—of pollen grains must be produced annually by a mature oak.

These pollen grains have no direct means of reaching the stigmas but depend on wind, gravity, or perhaps chance disturbances, to scatter them. That the best of them, the actual "fittest," if indeed there are any "fittest," from such a vast number, and dependent on accident for the opportunity to fertilize an ovule, do in fact achieve that fertilization is entirely beyond the bounds of belief.

Even if selection of the "fittest" in the fertilization process is manifestly impossible, let us look into the fate of the fertilized ovules. Some will not mature due to a variety of causes including the attacks of insects (either direct or on the twigs bearing acorns), that can have little relation to unfitness. Squirrels eat off many twigs apparently wantonly, and cause the loss of whatever immature acorns there may be on them without regard to fitness; these and other acorn-eating mammals and birds cannot wait for the nuts to ripen and pick off, and consume in part, or in whole, many a

green fruit. Infestation by nut weevils (*Balaninus*) is common and sometimes reaches a high percentage. These weevils do not select the unfit, they are seeking a safe place and assured nourishment for the development of their own young and on the average, no doubt, oviposit in just as good acorns as there are available.

After all casualties, the formative and growing acorns must survive, in doing which it is apparent that fitness plays little or no part, thousands of acorns are, nevertheless, ripened by every large tree. These drop off during a more or less protracted season and suffer a great variety of fates. Some may fall in holes, or rock crevices where they will never germinate though they be of the best; some may roll into, or be washed into, rivulets and be borne to larger waters where they may perish or perchance lodge in a place where they will germinate. If they have been carried to a place too different from the habitat of their kind they are again subject to elimination, but through no fault of their own, through no lack of fitness. Most of the acorns in normal course continue to lie directly beneath the parent tree, where animals may trample them or devour them. Some may lodge in piles where the uppermost will dry out with no chance of germination. They must perforce remain on the surface over the winter, giving opportunity for many destructive factors to operate, few if any of which are guided by the fitness or unfitness of the acorns.

The destruction of acorns by animals is so great as to be an object of concern to foresters, one of whom, Clarence F. Korstian, has published studies (1927) upon the subject, made in North Carolina, that may be abstracted as follows: The proportions of acorns of four different species destroyed by insects were 9.8 to 27.69 per cent and by birds and mammals 56.51 to 74.78 per cent; these percentages combined showed destruction in the various cases of from 80.6 to 94.62 per cent of the total crop. Additional toll was taken of germinated acorns varying from 1.75 to 4.52 per cent. The proportion of sound acorns and live seedlings together surviving animal feeding varied from 0.37 to 6.65 per cent. The animals concerned were deer, bears, cattle, hogs, rabbits, squirrels, chipmunks, mice, turkeys, crows, jays, blackbirds, nut weevils, moth larvae, and cynipids.

We can be sure that all of these creatures in search of nourishment will eat of the best; in fact it is traditional that squirrels and jays get the cream of the crop. What of the comparatively few that do finally succeed in germinating and taking root? Rabbits, squirrels, or mice may bite them off, or they may be browsed fatally or trampled by deer, accidents they can in no way avert by "fitness." Any surviving these dangers as a rule die through shading out by the parent and other trees. We have not mentioned every hazard affecting the fertilization, the growth and ripening, and the germination of acorns, nor the persistence of seedlings; no man knows enough to do that. We have shown, however, that with respect to the high degree of mortality among ovules, fitness cannot enter into the equation to more than a slight extent. Certainly the "fittest" have no assurance of survival. Fertilization is ruled by chance and from the very numbers involved—millions of pollen grains for every pistillate flower—it is certain that there can be no "survival of the fittest." The elimination of growing, and again of mature acorns, is mostly due to the feeding of animals and they seek the best. These two stages of elimination, both with the very opposite effect to what the theory of "natural selection" demands, amount

to perhaps 50 per cent of the green, and to from 80 per cent to 95 per cent of the ripe acorns. Later comes elimination of seedlings also partly due to agents that allow no play for fitness. Willis refers to disease as a factor of this kind and says that in experimental work with plants "it is disease which kills the bulk of the losers, and that these are not necessarily the weakest but often well-grown plants" (1915, p. 326).

The process of reproduction in the oak, prodigally wasteful of just as good material in the way of pollen, ovules, and fruits as the tree can produce, goes on year after year for possibly two, three or more centuries and in the end (speaking in average terms) there will be left to replace the old tree only a single one of the almost infinite number of progeny it has produced. Can anyone believe that the survival of that one oakling out of untold millions is due to inherent superiority—that it is the "fittest" of all?

Orchids. Another group of plants in which the wastage of immature stages is enormous is the orchids. Despite elaborate flower structure dependent on the visits of insects to insure fertilization, many flowers are not fertilized, and the ripening of seed capsules is the exception rather than the rule. The prevalence of vegetative methods of reproduction in the group shows that the fertilization mechanism, the most specialized, on the whole, in the vegetable kingdom (and claimed to have been developed by "natural selection"), is not depended upon as a safe means of reproduction.

It seems therefore that pollen and ovules no matter how numerously produced are as a rule wasted. When seeds are ripened, however, they are characteristically very numerous. One plant of *Maxillaria* has been estimated to produce 10,500,000 and one of *Acropera* 74,000,000 seeds. With such fecundity, if the seeds were in any considerable degree successful in making new plants, the earth would soon be covered with orchids, yet in fact they are as a rule local and rare. The explanation is of course that the seeds do not germinate and grow. They are minute and in many species lose their vitality in a few months. They depend on so erratic an agent of dissemination as the wind, yet to be successful they must reach a spot where not only proper conditions of warmth and moisture prevail, but where there must be also a growth of a more or less specific strain of symbiotic fungus.

The chances against completion of this chain of events are so great that Oakes Ames, a great orchid specialist, was constrained to remark, "For every dozen seeds that fall where endotrophic fungi of the proper type are present, millions must drift to sterile ground and suffer extinction" (1922, 46). Under such circumstances it is evident that fitness can play little part. It is clear that we have instead mass elimination by chance of both fit and unfit in whatever proportions these may exist among the seeds.

Western yellow pine.—Conditions favorable to reproduction of the western yellow pine (*Pinus ponderosa*) are frankly stated to be due to chance by G. A. Pearson of the United States Forest Service from whom we quote the following paragraphs:

"Studies aiming at a quantitative determination of the amount of seed required to secure a satisfactory stand of seedlings have not succeeded in fixing absolute standards, because of the extreme variations encountered on different sites and in different seasons. The greatest variable is rainfall. In unfavorable years no amount of seed will bring results. In seasons when growing conditions are normal or above normal, germination is good if seed is present, and usually there is some survival.

"One of the most direct and obvious factors in seed supply is the consumption of seed by rodents. The effective seed supply is that which is preserved

until moisture and temperature conditions are such that germination can take place. In years of light seed production chipmunks and mice probably destroy almost the entire crop, but in good seed years a considerable amount remains unconsumed. The surplus is what counts in natural reproduction. (1923, 10)

"In attempting to fix a minimum limit of seed requirement, it must be borne in mind that in average years from 90 to 99 per cent of the seeds will be lost either through failure to germinate or through infant mortality. It is estimated that on the plots under observation near the Fort Valley Experiment Station during the nine years prior to the heavy seed crop of 1918, not more than one out of every thousand germinable pine seeds produced a seedling which lived beyond the age of 5 years. Present indications are that the 1918 seed crop, because of the extraordinary moisture conditions in 1919, will yield much higher returns, possibly as high as 5 per cent and probably not less than 1 per cent. This should result in complete restocking on areas which were well seeded. Heavy crops may fail to give results, because they are not followed by suitable climatic conditions, and likewise a period of favorable climatic conditions may be wholly or partly ineffective, because it was not preceded by a good seed crop. Although the right combination, such as was experienced in 1918 and 1919, appears to be purely a matter of chance, we should aim to take advantage of such opportunities whenever they come." (*ibid.* 10–11)

Mr. Pearson's remarks not only set forth the usual high mortality among seeds of the pine, but demonstrate that survival as he says is "purely a matter of chance." Fitness evidently is of no avail when the entire seed crop is wiped out year after year by unfavorable climatic factors.

THE PROCESS OF ELIMINATION IN ANIMALS

Let us now examine instances of great fecundity and devastating elimination in the animal kingdom.

The cod. This species "is one of the most prolific of fishes, so much so that a female 39 or 40 inches long may be expected to produce about 3,000,000 eggs and one of 41 inches at least 4,000,000. Earll estimated the number of a 52½ inch fish weighing 51 pounds at 8,989,094, with 9,100,000 in a 75-pounder" (Bigelow and Welsh, 1924, 428).

The eggs are minute, transparent, and buoyant; they are liberated at random in the sea as is also the sperm so that there must be a great waste of sex cells. For the eggs that are fertilized, a primary hazard is the specific gravity of the water, and this factor which determines whether the eggs float and develop, or sink and die, must be correct within a few thousandths of one per cent. While adult cod prefer a temperature of from 35° to 42°, and spawn in late winter or early spring, mortality among the eggs is great in the case of what would seem to be almost a normal happening at that season, namely, for the temperature of the water to be as low as 32°. Untold numbers of the eggs may be killed also by sudden changes in temperature, or may be driven ashore by adverse winds and currents.

Even in favorable seasons the eggs float about at the mercy of wind and wave for about two weeks. They are of course perfectly inert and are unresistingly devoured by spawn eaters of numerous kinds. When first hatched the young cod also are pelagic and drift helplessly, a condition in which they may persist for about two months; during this period it is evident that there is no fitness that will fend off enemies.

To summarize, the immature stages of cod under the best conditions are exposed for a period of 10 weeks to elimination by agencies which undoubtedly act upon them with utter indiscrimination. So complete in fact is the elimination that it is a matter of record that in some years there is a practically complete failure to produce young over vast areas though every one of the millions of female cod there spawns its millions of eggs.

The Atlantic oyster. The oysters of the east American coast, according to J. H. Orton "shed their sexual products directly into the sea where haphazard fertilization occurs. A good-sized individual may shed at one spawning upwards of 50 to 60 million eggs." (1929, 1004). The immature forms grow rapidly but it is about two weeks before they settle and cement themselves to some clean-surfaced hard object where they develop adult organs and pass the remainder of their lives. Ordinary sea bottom, and objects in the water covered with the almost omnipresent slime, algae, or other marine organisms are unsuitable as places for attachment, or cultch, as they are technically called, and in oyster culture it is very necessary to supply clean, hard, material to enable the oysters to make any gains. "The settling of larvae on objects in the sea is called a spatfall." The eggs may be spawned at a temperature of 3 or 4 degrees lower than is necessary for a good spatfall. Hence if the temperature of the water does not rise that amount during the larval period in seasons when early spawning has occurred, there will be more or less of a failure in spatting. Even in favorable years the larvae are at the mercy of tides and currents and it is by chance alone that some of them find the necessary firm anchorages. The proportion that do thus succeed is normally very small, and it is certain that their success is due more to luck than fitness.

"The oyster's natural enemies [also] are more numerous in early than late stages of life. The larva is eaten by small fishes, jelly-fishes, other bivalves, sea-squirts (ascidians), worms, anemones, and small crustaceans, all of which are usually common on oyster beds. Spat, young, and old oysters are attacked by borers which bore a hole through the shell and eat the oyster through the hole. . . . Fifty per cent of a spatfall may be destroyed where borers are abundant." (*ibid.*, 1005).

There is no fitness involved in relation to the attacks of predators as the oyster bed is for them simply a pasture upon any part of which they feed at random. Not only is indiscriminate destruction exemplified in the case of the oyster, but also very clearly the principle of intensity of elimination in proportion to the immaturity of the stock, as well as that of survival by age classes, intermediate classes being largely wiped out by unfavorable local conditions.

The human tapeworm.—The sheer incredibility of the proposition that the fittest among millions survive is well illustrated by the life history of many parasitic organisms. All that needs to be said about a typical example, the human tapeworm, can be quoted from the writings of an ultra-selectionist, Weismann, who informs us that this parasite produces hardly less than 100,000,000 eggs.

"There exists, . . ." he says "a constant relation between fertility and the ratio of elimination; the higher the latter is, the greater must the former be, if the species is to survive at all. The example of the tapeworm makes this very obvious, for here we can readily understand why the fertility must be so enormous, as we are aware of the long chain of chances on which the successful development of this animal depends. The common tape-worm of Man, *Taenia solium*, does not lay its eggs, they remain enclosed within one of the liberated joints or 'proglottides.' Only if this liberated joint or one of the embryos within it happens to be fortuitously eaten by a pig or other mammal can there be successful development, and even then under difficulties and possible failures, and not right away into adult animals, but first into microscopically minute larvae which may bore their way into the walls of the intestine, or, if they are fortunate enough, may get into the blood-stream and be carried by it to a remote part of the body. There they develop into 'measles,' the so-called bladder-worms, within which the head of the tape-worm arises. But in order that this may become a complete and reproductive adult worm the pig must die, and the next step necessary is that a

piece of the flesh of the infected first host must happen to be swallowed raw by a man or other mammal! Only then does the fortunate bladder-worm—swallowed with the flesh—attain the goal of its life, that is, a suitable place to mature in, the food-canal of a human being. It is obvious that countless eggs must be lost for one that succeeds in getting through the whole course of a development depending so greatly on chance. Hence the necessity for such enormous productivity of eggs." (1904, 47).

CHANCE ELIMINATION

In describing the elimination of the hundred million less one, and the survival or "selection" of the one, it is not surprising that Weismann has to use such terms as the "long chain of chances," "happens to be fortuitously eaten," "if they are fortunate enough" and "happen to be swallowed" in describing a "course of development depending so greatly on chance." The choice of language appropriate to the nature of the process, as I have said, is not surprising. What certainly is amazing, however, is that selectionists can record such matters, and it is to be assumed, reflect upon them, yet cling to the doctrine of "survival of the fittest."

A priori that doctrine is incredible anyway when the theoretical survival of only one or two, on the average, out of exceedingly large numbers is concerned. Common sense tells us that survival literally of the fittest can not happen, except by rare chance, but judging from the evidence alone, which as loyal scientists we must do, there is nothing in this world more certain than that the "fittest" do not survive in the cases here detailed of the oak, the orchid, and pine, the cod, the oyster, and tapeworm. Their life histories, moreover, are not unique but may be taken as fair illustrations of those of large groups of organisms with respect to which we must draw similar conclusions.

Recognition of the indiscriminacy of elimination in still another group is given in the following popular science item: Dr. Albert F. Blakeslee estimated the number of spores in a puffball of about 12 x 10 x 9 inches in dimensions to be in the neighborhood of six trillions—enough to allow one spore for every square rod of the entire land surface of the earth. "Theoretically, every one of these spores is capable of giving rise to a new plant of its own kind. Actually, nearly all of them fall into unsuitable places, and so perish." (*Science-Supplement*, 81(2104), April 26, 1935, p. 9.)

SURVIVAL OF THE ORDINARY

When elimination exceeds 99.99 per cent it must take the fit along with the unfit. Especially is this result certain as in the bulk of the population the margin of superiority of one individual over another is slight, both theoretically and actually. The impossibility of any process, natural or artificial, selecting the fittest from a large number of exceedingly similar individuals makes it certain that in general the survivors will be random samples, or in other words commonplace representatives of their kind.

The great checks, climate and disease, are indiscriminate with reference to many special characteristics of organisms. In fact every check that can be imagined is indiscriminate with respect to some adaptations of a race. Some individuals may be cold resistant, for instance, but not very alert; others may endure heavy parasitism but be relatively poor foragers, etc., etc. "Selection" for one good point may be impossible without simultaneous "selection" of one or more weakness, and "selection" effective under one set of circumstances will not be under others. With checks acting in this manner is it not inevitable that specimens from the common run will be the final residuum?

The force of the argument as to survival of the ordinary can be appreciated by scanning a normal curve of frequency (Fig. 1) which assumably applies as well to fitness as to any other character. The area inclosed by the curve may be taken as a measure of the individuals of the group subject to elimination or "selection." It is conspicuously apparent that the great mass of the group do not vary widely from the mean, and that those which do differ greatly (both the most and the least fit) are relatively few in numbers. The fittest and the least fit are represented by the extreme left and right ends of the figure. Quarters of the population lie to the left and right, respectively, of the quartile ordinates (q, q). The small proportion represented in a less than one per cent survival can readily be envisioned. It is a proportion that can easily be reduced to the vanishing point by any indiscriminate mortality, and as demonstrated in earlier pages, mortality reducing populations at all numerous is indiscriminate. Contemplating the matter with the aid of the graph, it is perfectly evident that a random sample of one or two individuals from the whole population will in the vast majority of cases come from the great median group of rather closely similar individuals. The dotted curve in the graph marks the bounds of the population after experiencing a 50 per cent indiscriminate reduction. In the remainder it is clear, as before, that average and near-average individuals greatly predominate in number over the more aberrant (unfit or fittest).

R. A. Fisher, a modern defender of natural selection theory, may be quoted in agreement with this conclusion, as after remarking that, regardless of genetic constitution, great likeness among members of the same species is necessary even for approximately normal adaptation, he adds, "Since any differences which may exist . . . are certainly extremely minute we have here a clear indication of the closeness with which any tolerably successful individual must approach the specific type." (1930, p. 68).

The totally unfit as well as the most fit do not form very large blocks of the population, so that if all of the former are eliminated and final "selection" made from the average plus the fittest groups, still in all probability survivors are likely to come from the median, and vastly more numerous, group. This type of "selection" is admitted by the ultra-selectionist, A. R. Wallace, in the following passage:

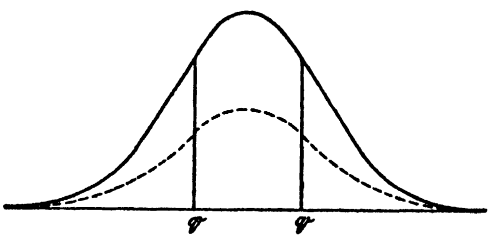

Fig. 1. Frequency Curve to Show (Solid Line) the Normal Distribution of a Population with Respect to Fitness, and (Broken Line) the Distribution after an Indiscriminate 50 Percent Reduction. The Portions Laterad of Ordinates q, q Are Each 25 Percent of the Total

"If now we consider the population of a species with regard to any particular character or combination of characters, we may divide it into three groups—a central group in which the mean or average development prevails with little variation, one in which the character is excessively, and one in which it is little developed. These groups would not be of equal extent, the central portion—that in which the mean characteristics prevailed—being, in accordance with the law of averages, much more numerous than the extremes; perhaps twice or even three times as great as either of them, and forming such a series as the following:—Maximum development 10, mean 30, minimum 10. These figures, whatever their exact proportions, would probably be pretty constant, for we have no reason to believe that the mean characters, or the amount of variation of a species, change materially from year to year or from century to

century; and we may therefore look upon the central and most numerous group as presenting the typical form of the species, being that which is best adapted to the conditions in which it has actually to exist, while the extremes, being less perfectly adapted, are continually weeded out by natural selection." (1880, pp. 100–101.)

Wallace probably did not realize that selection of the type he postulates could be no moving force in evolution. The weeding out of variants and the preservation of individuals near the mean is anything but Darwinian natural selection. Nevertheless, it apparently must be recognized as the ordinary occurrence.

In their work on "The relative value of the processes causing evolution" (1921), the Hagedoorns repeatedly refer to the normal occurrence of reduction in variation and state that "minorities have no chance." Their remarks are summarized by Robson and Richards, "The survival of only a small number to carry on the species must mean an enormous reduction in variation each year, probably enough to account for the observed constancy of most species. The chance that any variant represented by only a few individuals will form a part of the next year's initial population is very low, the magnitude of the chance depending (apart from survival value) on the ratio between the numbers of the variant and the total number of individuals in the species." (1936, pp. 171–172.)

Extending the argument, it seems clear that aside from the slight chance, mathematically speaking, of variants in small numbers surviving to propagate their kind, the observed constancy of species proves that they do not, in fact, survive. (The natural selection theory here faces another of its numerous dilemmas; if selection does not restrict variability it cannot direct evolution; if it does restrict variability it destroys the material essential to its own effectiveness.) The type of "selection" (if it may be so called) here referred to is the "periodic selection" of Karl Pearson, defined as reducing variation in each generation without altering the type (1900, p. 413). To put the matter in another way, if the race continues unchanged as in these concepts of Wallace, Pearson, and others, variation must be reduced in each generation and when variation is reduced survivors must necessarily come from the median class.

There are, moreover, unmistakable signs that there is a tendency in nature for the elimination of all aberrant forms, which applies to the "fittest," if these are significantly aberrant, as well as to the unfit. Mutations, as a rule, admittedly are backward steps; usually in fact they are lethal. Is not almost any decided variation likely to be harmful? In this regard W. D. Matthew says, "So with any wide and conspicuous variation from the normal. It is of relatively rare occurrence, and it is a handicap, not an aid to survival." (1930, p. 193). Is not conservatism the rule in nature? We clearly see that it is in behavior, as in the case of lone bulls and rogue elephants; it takes the varying one out of the stream of descent. Despite the universal occurrence of minor fluctuating variations, species nevertheless conform to type. They are so alike as to be recognizable at a glance. Conforming is a universal law (herd law, law of the pack, gang law). It is exactly innovators and innovations that are suppressed, and it seems clear that from Nature's point of view the commonplace are the "fittest."

Out of thousands or millions of ordinary specimens there are hordes of nearly the same orders of abundance that are equally capable of reproducing the race. What is true of large, tends to be true of smaller, numbers, and it is likely that the vast majority of organisms are prolific enough

to invite elimination of much the same character as that affecting their more numerous contemporaries, and that the net result in their case also is survival of fairly typical specimens.

Parents of the new generation coming from any part of the great mass of average or near-average individuals would give rise to offspring which, considering regression and the propensity to vary about a mean, would differ little if any from the preceding generation. Galton from his studies of stature in man "pointed out the remarkable way in which each generation was succeeded by another that proved to be its statistical counterpart." (1886, p. 256.)

There is no evidence that the so-called process of "natural selection" does away with this characteristic of species as the ordinary fluctuating variations appear generation after generation more or less within the same limits and about the same mean. (Lindsey, 1929, p. 268.) This can only mean either that extreme variants ("fittest") are not "selected" or that if selected they do not transmit their divergent characters to their descendants.

There is evidence in specific cases, furthermore, that in nature the range of fluctuating variations is maintained. We quote from Dr. Vernon L. Kellogg, as follows:

"But aside from the part that what we may call fortune of position plays in determining life or death among individuals, what of the actual rigour of the struggle in those cases where death does not come to thousands at a moment;—in the whale's mouth, by catastrophe of flood or drouth, or by the elephant's tread on the ant-hill? To this question of the rigour of intra-specific struggle I have given some personal attention in insect life, and while to detail observations here would be impossible, I may say baldly that no such rigour of individual selection based on variation in colour, in pattern, in venation and other wing characters, in hairs and in numerous other structural characters, as demanded by the needs of the selection theory, is to be detected. I find just as much variation represented in series of mature individuals collected miscellaneously after having lived for more or less time a free life exposed to all the dangers of this life, exposed, that is, to the rigour of the individual struggle for existence, as among series of similar extent of individuals of the same species collected just at the time of reaching maturity but before enjoying any opportunity to be weeded out (on a basis of disadvantageous variation) by the rigour of the life-struggle. Just as many varying individuals, with variations of just as much extent and variety, were found in series exposed to the struggle, in which these variations are presumably capable of saving or losing life, as among series not yet exposed; in other words, just as much variation exists after enduring the selective rigour of the struggle as existed on the day when the insects are first exposed to it." (1908, pp. 82–83.)

This account refers to coccinellids, and Dr. Kellogg records an instance of similar import relative to *Diabrotica soror* in which about the same range of fluctuating variation was maintained in the same locality over a period of 15 years (1910).

It is apparent from these and other exact records, as well as from ordinary experience, that the population of organic species always shows a more or less characteristic range of variation. This condition in itself is all the proof that is required that extreme individuals ("fittest") are not the chosen propagators of their kind, or that if they are selected, the average character of the species is not thereby affected. It is certain, therefore, that species are maintained along the line of averages (see also Galton, 1886). Variation about a mean, a universal phenomenon, always leaves the bulk of a species at or near the mean. From this mass of closely similar individuals, and not from extreme variants, reproduction proceeds. This is not only a common sense conclusion but is supported by experience and evidence. The burden of proof for any other claim certainly rests upon its makers.

If "selection" of superlative character-

istics were the rule, there would be visibly rapid evolution and a preponderance of bizarre species. The obviously very gradual process of evolution and the equally apparent conservative make-up of species, of themselves, indicate that propagation is from the median class of average individuals and not from aberrant extremes.

It is of interest to quote Dr. E. D. Cope in this connection: "In general, then, it has been the 'golden mean' of character which has presented the most favorable conditions of survival, in the long run." (1896, p. 174.)

In the course of the foregoing discussion of natural elimination and reproduction, two phases of the general subject were mentioned which could not then be more fully considered without interrupting continuity. We will now proceed to these, namely, the disproportionate mortality of the young, and survival by age classes.

DISPROPORTIONATE MORTALITY OF THE YOUNG

A cause of much mortality in the very young is constitutional defectiveness. The individuals affected can scarcely be said ever to have entered the struggle for existence. They are, so to speak, born outside of it and the cause of their being so born, misfunctioning of the hereditary mechanism, is not a process of "natural selection."

It is evident not only from the examples previously cited but from general biological experience that reduction in organic populations is most drastic in the immature stages. A few further instances may be cited to illustrate the principle. Dr. John D. Harshberger presents (1898) data on the numbers of abortive fruits of various plants from which the following percentages have been computed: *Pimpinella integerrima*, 22.65 per cent; *Hibiscus moscheutos*, 25.68 per cent; *Cornus florida*, 74.62 per cent; *Arisaema triphyllum*, 78.85 per cent; and *Azalea nudiflora*, 95.74 per cent. These, bear in mind, are losses to a more immature stage than the perfect seeds or fruits and later the seedlings, which themselves are subject to wholesale destruction.

Losses in the seedling stage have recently been emphasized by Professor E. J. Salisbury. He cites reduction in the number of seedlings of *Silene conica* from 175 to 9 per square decimeter in the period from September to January, and comments: "There is not only a very high mortality amounting to nearly 95 per cent, but also the important fact is that this mortality was entirely confined to the seedling stages. All the mortality occurred prior to the formation of the second pair of foliage leaves, and all the nine survivors will clearly attain the flowering condition."

Relating to *Verbascum thapsus*, he says: Of "several hundred thousand seedlings" all but 108 died during the first six months. "All the survivors which attained the rosette stage flowered and produced seeds."

Similar occurrences were noted also for *Ranunculus parviflorus*, *Helleborus viridis*, *Cochlearia danica*, *Dianthus prolifer*, and *Fagus sylvatica*. The author therefore justly concludes that "in these plants, at least, the mortality and therefore the operation of natural selection is almost entirely confined to the juvenile stages of development." (1930).

Thus elimination in the immature stages which we have shown to be highly indiscriminate, was in the cases Salisbury describes, the total elimination. All survivors of that random selection matured or clearly promised to do so. In other words all were fit, a result not at all compatible with the alleged "survival of the fittest."

The same type of mortality prevails

among animals and its relation to "survival of the fittest" theory is the same. The entomologist, F. Muir, in writing of biological control says, "My own experience covering such work has demonstrated to me that in the majority of instances the greater part of the mortality falls upon the eggs and younger stages, so that only a very small proportion comes under the influence of selection, so far as the adult characters are concerned." (1931, pp. 3-4.)

In combating Darwin's hypothesis of sexual selection, Wallace used the argument that selection in the adult stage, alone, would be ineffective, and if the argument is valid in that case, it holds good in the entire field of natural selection. Wallace's remarks are: "In butterflies the weeding out by natural selection takes place to an enormous extent in the egg, larva, and pupa states; and perhaps not more than one in a hundred of the eggs laid produces a perfect insect which lives to breed. Here, then, the impotence of female selection, if it exist, must be complete; for, unless the most brilliantly coloured males are those which produce the best protected eggs, larvae, and pupae, and unless the particular eggs, larvae, and pupae, which are able to survive, are those which produce the most brilliantly coloured butterflies, any choice the female might make must be completely swamped." (1923, p. 296.)

If natural selection is to operate in the manner usually claimed, it must be upon individuals that have not completed their reproductive cycle, in most cases therefore upon the immature. But the immature in general do not have the characters that are said to insure the survival of the species, hence "selection" among them cannot directly control the perpetuation of those characters; this point has been commented on also by Henslow (1893, p. 333), Salisbury (*op. cit.*), and Willis (1907, pp. 17, 18 and 1922, p. 210).

The heaviest losses are suffered by the immature stages, but this is not only because the young are relatively helpless but also because they are most numerous and under the principle of proportional predation (McAtee, 1932, p. 144) must bear the brunt of assaults by natural enemies. There are more of them, they must serve, just as grass is eaten by more terrestrial organisms than are other plants.

The trend of the examples more fully discussed in earlier pages of the present paper certainly is to the same effect as that of the Salisbury cases, namely, that mortality is so heavy in the immature stages that there is very little scope for additional "selection" in cutting down the number of adults to the average limit of the breeding population. The survivors are a uniform or standardized lot of average type and there being no superlatives among them there is no opportunity for "selection" of the "fittest." There may be those who believe that the requirements of evolutionary theory will be met by a small degree of "selection" among the residue that have survived the hazards of immaturity, but they cling to a faint hope and have no excuse for longer indulging in that full-blown speculation, termed "Survival of the Fittest."

We scarcely need repeat that the evidence is preponderantly to the effect that mortality among the immature stages is almost entirely ruled by chance. The survivors, therefore, will be a random lot, containing all degrees of fitness normal to the species. Since numerical reduction of the species already has about run its course most of these survivors will reproduce. Even if there were selective mortality in the immature stages, it could have little more than a general bearing on adult fitness because of the differences in the

characters involved. Willis remarks concerning plants, "while the characters that distinguish species and genera are largely characters of the floral organs, the struggle for existence is almost entirely among the seedlings and young plants, in which these organs are not yet present." (1907, p. 17.) As a special case consider the numerous metamorphosing animals in which immature stages are utterly different from, and in many cases even inhabit a different medium from, the adults. Moreover the period of adult life in such organisms often is a relatively brief one allowing little time for "selection" to act. Yet if "natural selection" is to explain evolution of species, it must be through "selection" of adults, the breeders of future generations. In many cases, it is certain that mortality among the immature is so sweeping that it leaves opportunity for little reduction in numbers and hence for "selection" among adults.

To recapitulate the few preceding paragraphs: Mortality among the immature stages is both sweeping and indiscriminate; practically all of the survivors, a normally variable, not a selected, lot reach maturity and may propagate; and although evolution must go on through the medium of adults, only a small proportion of the total elimination (or "natural selection") occurs in that stage. The chances seem poor indeed that there is any such thing as "survival of the fittest."

SURVIVAL BY AGE CLASSES

Survival by age classes was incidentally mentioned in the accounts previousy given of the western yellow pine, the codfish, and the Atlantic oyster. To emphasize the significance of these cases the following expanded discussion is given.

Mr. G. A. Pearson, the author quoted with regard to the pine, not only recorded the usual high mortality to seeds, but also the phenomenon of "seed years," one of considerable interest for its bearing on selection theories.

"It is believed," he says, "that the long period between good seed crops may be due to circumstances not connected with the fruiting habits of the tree. Repeated observations have shown that enormous numbers of cones die during the first season. . . . It is possible that late frosts have more to do with these conditions than dry winds. Whatever may be the cause it is probable that many of the light seed crops would be good crops but for the arrested development of cones in the early stages. Much damage is done by weevils as the cones approach maturity, but serious infestations are usually of a local character." (1923, 21–22).

"During the period covered by the rainfall graphs, good seed crops occurred in 1908, 1909, 1913, and 1918. Since the seed does not germinate until the year after it is borne, the right coincidence between seed production and rainfall occurs only when a seed year immediately precedes a period of ample moisture. In 1908 and 1909 seed crops both failed to give results because they were not followed by sufficiently long periods of immunity from severe drought. Similarly, the favorable moisture periods of 1911–12 and 1918–19 were unavailing because they were not preceded by good seed crops. The seed crop of 1913 was effective because no really serious drought occurred until the foresummer of 1916. It would have been still more effective if there had been more precipitation during the summer and fall of 1915 or if the seed had been available a year earlier, thus taking advantage of the entire rainy period, which extended from July, 1913, to August, 1915. The grand climax came with the heavy seed crop of 1918 and the extraordinary precipitation of 1919, followed by reasonably favorable moisture conditions in 1920. This period promises to give the best reproduction experienced on the Coconino in 40 years. Success would have been still more certain, however, if the seed which matured in 1918 had been available a year earlier, thus profiting by the two consecutive wet summers of 1918 and 1919. During the entire period of 12 years, two good seedling crops of general distribution have originated on the Coconino and Tusayan Forests, and only one of these, that of 1919, was adequate to give full stocking over extensive areas." (*ibid.* 28–29).

Commenting further on this matter, E. N. Munns, Chief, Division of Silvics, U. S. Forest Service, writes (letter of March 27, 1933),

"Foresters are coming to believe that factors other than periodicity account for seed production and that the forest tree fruits are susceptible to damage from early fall frosts while the buds are being formed, spring frosts as the flower buds start to develop, fog or rain at the time the pollen is ready to be liberated, excessive drying in the early spring, and other such climatic troubles.

"There has been no real crop of Douglas fir seed in the Pacific Northwest for a period of 10 years. Investigations disclosed that flowers were borne practically every year; some one or several of the climatic factors indicated have been responsible for the failure of these flowers to develop.

"In the Southwest the seed periodicity of *ponderosa* pine is quite marked. However, the seed does not catch and reproduction does not get started unless there is a coincidence of seed year with favorable climatic conditions. This has given rise to even-aged groups of trees extending in some places over considerable areas. Investigations have shown that the coincidence of good seed years and climatic years favorable for reproduction occur at intervals of from 15 to 40 years."

The survival of even-aged groups of trees is not a phenomenon confined to conifers nor to the western part of the country. In the vicinity of Washington, D. C., it is evident that the Virginia pine "catches" by seed years, and the beech which very rarely fruits here is as common as it is in some regions where it matures nuts regularly.

In the case of the codfish we are informed by a letter from the U. S. Bureau of Fisheries (March 17, 1933) that "there is almost a total failure of the natural hatch during certain years, while in other years the species is very successful in establishing an abundant generation. We thus find that in the commercial catch . . . certain year classes greatly predominate." In other words "fitness" in the case of the cod means being hatched in the right year. In one year few or none are fit to survive, while in another a high proportion are. Does this not put the theory of survival of the fittest in a sufficiently ridiculous light? Can it with any semblance of reason be claimed that the fittest of the cod eggs survive? Certainly not; in the favorable years a high proportion mature, in the unfavorable seasons, from unquestionably just as good foundation stock, few or none survive. Not fitness of the cod but that of the environment rules.

The same survival by year classes is known in the case of mackerel, herring, salmon, and other fishes, and it may be taken as axiomatic that species producing large numbers of eggs are subject to powerful devastating influences, the action of which is little affected by fitness. In the case of the extraordinarily prolific Atlantic oyster, we are informed that "very good spatting seasons occur sporadically. . . . In the same locality bad and moderate seasons have occurred on the average about twice as frequently as good or very good seasons. Similar fluctuations occur in all parts of the world. . . . In times of scarcity when the spatfall has failed for a longer succession of years than usual, and when the adults on the beds have been reduced beyond a certain minimum, particular beds have gradually died out." (Orton, 1929, 1004).

Bad years twice as numerous as good years, spatfall failing for a succession of years—the nature of the case is the same as in those of the pine and the cod. Sometimes complete failure, sometimes great success, but at all times upon the same basis of potentially uniform reproduction from which "nature" is supposed to, but obviously does not, "select" the "fittest."

According to the natural selection theory only two on the average out of any number of offspring of bisexual species are fit enough to survive. Survival of large numbers by age classes completely demolishes this pretense; a high proportion prove fit. The converse of this proposition, namely, that in the years of failure

due to environmental conditions, none are fit, although reproductive effort is just as strong, and the quality of the progeny just as high, is, of course completely absurd. The sweeping failures demonstrate that all the eliminated are not unfit, while successful mass reproduction shows that it is not only the "fittest" that survive. Mass survival must result in a preponderance of ordinary or typical, not aberrant ("fittest") representatives of species.

SURVIVAL AMONG CYCLIC OR FLUCTUATING SPECIES

Similar conclusions must be drawn from the ups and downs of cyclic species. At the height of their abundance they are swiftly cut down to minimum numbers; according to the "natural selection" test nearly 100 per cent are unfit. Then begins an upswing in numbers, during which the rate of survival is so high that the species soon regains its abundance; i.e., a high proportion are fit. The impossibility of explaining these results by the theory of "survival of the fittest" is manifest. Aside from the great improbability that it is the "fittest" that survive the "drops," consider what they do to prove their "fitness." They propagate a population that in a few years again is swept away in exactly the same fashion as their own generation. Better proof could not be asked that the "selected" breeds are merely ordinary members of their race.

The cycles of the snowshoe hare, for instance, are known to have been going on for a century, and the great probability that they have an underlying climatic cause indicates that they may have occurred for an indefinite period in the past. In these cycles at the period of the "drop," "crash," or "die-off," the "selection" is the most drastic imaginable, apparently just short of extermination. If "survival of the fittest" had prevailed through the ages, the cyclic races should have developed some sort of resistance to the forces that destroy them. The fact that they remain subject to extreme fluctuations is sufficient evidence that there is no "survival of the fittest" or race improvement; if there were the cycles would end. Similar conclusions apply in the case of any species that undergoes great fluctuations in numbers, and the indications are that a very high percentage of all existing species are so affected. For various reasons, some of which at times we think we can descry, a species becomes relatively rare; its "fittest" are few indeed. Then its ranks are rather rapidly refilled by the survival of a much higher than usual proportion of young; its "fittest" for the time being are numerous. The same remarks as to the average character of survivors, are in order, as in the case of cyclic species. "Natural selection," the "survival of the fittest," do not explain such phenomena and they are practically universal.

CONCLUSION

Among large numbers of organisms, the "fittest" (two out of millions for instance) are an exceedingly small proportion. It is wholly incredible that any process could be devised even by man, much less brought about by the forces of nature, that could insure "survival of the fittest."

It is evident, moreover, from the instances discussed in this paper that elimination, which bears most heavily on the immature stages, tends to be highly indiscriminate. For this reason also it is very probable that, as a rule, not "fittest," but commonplace specimens survive to propagate the race.

Elimination among the immature being

practically indiscriminate, the survivors will be a random or ordinary lot. Since a high proportion of the total elimination occurs among the young, there is opportunity for comparatively little "selection" later among the adults, and most of them (necessarily of average make-up) will breed.

Many species have a tendency toward an "all or none" type of survival. It is very strongly developed in those exhibiting age-class phenomena, and is only slightly less evident in cyclic species and others subject to considerable fluctuation in numbers. The mass elimination these species experience (sometimes total for certain periods or areas) certainly takes the "fittest" along with the rest, while their characteristic mass survival obviously is not limited to the "fittest" (a group which according to selection theories, as we have already noted) is very small. There is no conclusion possible, therefore, but that the continuity of these forms (which include probably a great majority of all existing species) is through ordinary representatives. On all these counts (elaborated in previous pages), therefore, the unavoidable deduction is that average representatives of species, not the "fittest," survive.

What do these considerations mean in relation to the theory of "natural selection?" The preservation of the commonplace or fit is something very different from the "survival of the fittest." The latter, though incredible, if it existed, could produce evolution; the former, and it is certain, what actually occurs, by itself cannot.

"The survival of the fit," "the elimination of the unfit," favorite succedanea of neo-Darwinians (of themselves) will not do the business, will provide no motor for the evolutionary machine. Even "the better equipped survive, the worse equipped die," idea is scarcely an improvement, because of the reversion that must always occur to account for the observed maintenance of fluctuating variations. From better or "fitter" to fit or ordinary is a gradation so small that it is easily within the scope of the usual range of variation. It is certain that descendants of organisms so characterized would, on account of merely normal regression and variation, be indistinguishable from the average of their kind.

The evidence marshalled in this paper is to the effect that reproduction of species, on the whole, is carried on by ordinary individuals. They come from the great median mass of the population, and maintain the normal range of fluctuating variations. In the absence of other effective forces, propagation in this manner cannot change the character of the race nor produce evolution.

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THE CYCLIC CHARACTER OF HIBERNATION IN FROGS

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INTRODUCTION

HIBERNATION is usually thought of as a resting condition in which an animal exists in a state of complete or partial torpidity. This state is associated with low temperature, but, a similar condition, called aestivation is found at high temperature and low humidity.

Many causes have been suggested to account for this torpidity, but, whatever the cause may be, the result seems to be that the animal is able to live through unfavorable weather conditions with no lasting injury to itself. This period of inactivity in a protected location offers the opportunity for carrying through the physiological changes in the life-cycle.

Six different modes of expression of the tendency to become quiescent occur in the animal kingdom. The extreme specialization of the reaction to unfavorable conditions is to be found in some protozoans, rotifers, and copepods, where the animal loses its normal shape, and many of its physiological characteristics are modified so that, in a new form, the cyst, it is able to withstand great extremes of temperature and moisture.

Insects may be used as examples of the second type, for the animals become more or less dormant regardless of environmental factors. The codling moth prepares for hibernation by heavy feeding, which is followed by the elimination of all waste, then burrows into the ground and hibernates. If they cannot burrow, they remain active and cease to feed for several weeks (Tower, 1906). Shelford (1908) found that tiger beetles become dormant in August and continue so all the winter with no apparent environmental factor responsible for bringing on quiesence.

A third type comprises invertebrates that develop accessory protective devices against unfavorable weather. For example, in a dry soil annelids may be found knotted together in a cyst-like ball and covered with mucus while awaiting increased moisture. Another example is found in the terrestrial snails which with the approach of unfavorable weather withdraw into their shells secreting successive mucus diaphragms across the apertures. With this group may also be classed the terrestrial reptiles and insects that seek shelter from unfavorable conditions without passing through any evident physiological change.

A fourth type represents true dormancy and may be found in homothermic mammals, such as the marmot, which assumes a state of poikilothermism as an adaptation against unfavorable environment. The fifth type is found in the bear which observers assert, merely "dens up" to maintain a high temperature when food becomes scarce. The sixth type embraces certain frogs and other poikilotherms which enter lethargy without the necessity for a change in the body temperature regulation.

There is a fairly large body of literature dealing with the nature and causes of hibernation, but little of it deals with

the amphibia. The conditions in insects as well as in mammals have received attention, and there are several extensive summaries of the literature on these groups with very complete bibliographies. Dubois in his "Physiologie Comparé de la Marmotte" reviewed the literature to 1896, and Polimanti (1912–14) and Rasmussen (1916) discussed the literature to 1916. Pocock summarized the work from the standpoint of general biology, while Gorer (1931) studied the subject from the angle of comparative physiology. Particular mention should be made of the work of Johnson (1931) and also an excellent compilation contained in the monograph of the German writers von Ferdmann and Feinschmidt (1932).

It is the purpose of this paper to summarize the literature for the amphibia, and show that hibernation is a phase of the annual cycle of the common frog. This cycle is evident in the changes of weight and of longevity under unfavorable conditions, and in seasonal differences in the glandular portion of the hypophysis, gonads, adrenals, pancreas, thymus, thyroid, spleen, chromatophores, muscles, heart, and blood.

It will be seen that this study together with that of Johnson (this journal 1931) brings the knowledge of hibernating vertebrates up to a late date.

There is so little literature on the factors concerned with the hibernation in amphibia that the author has been compelled, for the purpose of comparison, to include some of the more important contributions on the hibernation of other forms. The recognition of the nature of the differing behaviour of the animals is based upon the interpretation of physiological and other studies. Much of the older work on the frog has been presented without due consideration of the time of the year during which the observations were made; this has led to a multiplicity of inaccurate and contradictory statements.

SUGGESTED CAUSES FOR HIBERNATION

(*A*) *Temperature*. The cause of lethargy has been commonly ascribed to the factor of environmental temperature. Lethargy was held to be the result of stiffening of muscles as a reaction to the cold (Spallanzani, 1792), while in 1926, Adler held that cold was a factor in causing hibernation. The heat produced by the animal was not sufficient to cope with the environment, the blood became cold and the animal torpid (Buffon & Lacepede, 1920). On the contrary, studies of marmots have not shown that cold is the cause (Mangili, 1807, Mares, 1892, Cleghorn, 1910, Rasmussen, 1916). Mangili even claimed that severe cold arouses hibernating animals. Nor do the studies on insects point conclusively to temperature as a factor.

Since hibernation in amphibia has been studied but little, it was thought that a study of *Rana pipiens*, subjected to zero degree centigrade at each season of the year would be instructive, and accordingly during each month, throughout a period of two years, groups of *Rana pipiens* were put into an electric refrigerator and kept at zero centigrade temperature and saturated humidity. Only those frogs which were placed in the refrigerator from October to April became completely torpid. They buried themselves in sand in a position characteristic of the hibernating animal, i.e. with arms folded under the chin, the hind leg drawn close to the body and head resting upon the front legs. The eyesockets were drawn into the head. In the summer these frogs became sluggish, but at no time did the cold cause complete torpidity. These results lead to the conclusion that external

temperature is of itself not sufficient to cause hibernation in *Rana pipiens*. However it was shown in these investigations that external temperature is a necessary factor in the development of physiological conditions attending dormancy as was demonstrated in the increase of weight during the months of winter.

(*B*) *Food*. A lack of food is also an environmental factor in hibernation, and has been believed to make the animals more susceptible to cold (Hall, 1832, Mangili, 1908). Others have observed that marmots will hibernate with food available. Accumulation of fat was proposed as a cause of lethargy by Claparède (1905). In nature frogs bury themselves in the mud in October and remain without food until March (Barthelemy, 1926). No food was available to these frogs in the refrigerator at any time of the year, yet in the months of winter the frogs became lethargic and in the summer grew merely sluggish in response to the same environment.

It is of interest here, to note that the "control-frogs" which lived in atmospheric temperature did not attempt to snap at insects placed in the cages during the winter months. Autopsy showed no insects in the contents of the stomach, from October to February, although beetles and worms were placed in their cages.

(*C*) *Moisture*. An environmental factor suggested as a cause of hibernation, is a lack of water. Desiccation of vegetables has been thought to cause lethargy in marmots (Kasharov, 1907, Shaw, 1925, Kalaboukhov, 1920). Contact with water is said to be one of the controlling factors in the emergence of certain Orthoptera (Bodine, 1921, Fink, 1925). Likewise, emergence of toads and salamanders has been attributed to rain (Townsend, 1921, Blanchard, 1930).

In this work on *Rana pipiens*, moisture could not be considered a factor in determining emergence since, throughout the entire period of study, the humidity in the refrigerator was constantly saturated. The melting ice on the shelf above the frogs, kept the sand in which the animals hibernated, constantly moist, and still, without any change in the distribution or content of the moisture in the refrigerator, the hibernating frogs became active and tried to climb out of the box when the door was opened. During hibernation there was a hydration of the animals, but upon emergence hydration decreased.

THE INTRINSIC FACTORS

(*A*) *Nervous System*. An essential characteristic of the hibernating animal is its ability to change from a homothermic to a poikilothermic type at a rather definite period. In the hibernating mammal the heat center apparently fails to function and the animal reverts to a condition of the animal with a more primitive type of nervous system (Barkow, 1845, Pembrey, 1895, Noe, 1903, Merzbacher, 1904). If the caloric center is known a satisfactory answer is still lacking as to how it is possible in so short a time for hibernants to cause this center to become active or inactive. What are the stimuli for increased or decreased activity? Certain animals roll themselves into a ball while hibernating, owing to tonic stimulation of adductor and flexor muscles. The center for the clinging reflex shown by bats in hibernation is thought to be in the medulla.

Diminution of Nissl's granules and a general basophile reaction of the cytoplasm, have been observed in the hibernating mammal (Legge, 1899). In the present experiments it was found that the hibernating frog assumes a position in which the outside curvature of the body

is a segment of a circle. When a dormant frog is electrically stimulated in any manner, the circle tightens, but when stimulation is increased, a series of random jumping movements occur.

A structural relationship between hibernation and the nervous system in frogs has been found also (Donaldson, 1911). Levi (1898) found seasonal alterations in the staining reactions of the cord of a frog. The author found a seasonal difference in the staining reactions of both cord and brain, as well as of other tissues. The nuclei and the cytoplasm of the frogs were less basophilic during the winter. In the hibernating frogs the cytoplasm loses its basophilic character entirely. Donaldson (1911) found that the relative weight of the central nervous system in the leopard frog was low at the time of hibernation.

Seasonal changes were observed in the functioning of the nerves of *Rana pipiens*. Stimulation of the vagus failed to stop the beat of the heart in hibernating frogs, while the same stimulation inhibited the heart of active frogs of the summer. An alteration in nervous control is indicated by a change in the absorptive power of the skin of dormant frogs. Adolph (1921) found that the nervous system is responsible for resistence to osmosis. As will be discussed later, the frog absorbs water during dormancy. Undoubtedly the nervous system plays an important part in the development, duration and awakening from hibernation, but apparently as a phase of the seasonal cycle only.

(*B*) *Endocrine Control*. The possibility of a relationship between the secretions of the endocrine system and dormancy has been the cause of much speculation.

Glandular Portion of the Hypophysis

Observations on the pituitary are not in agreement as relating to hibernation. In mammals the gland diminishes in size, and the cells in the pars-anterior completely lose their characteristic staining reactions to acid and basic dyes (Cushing & Goetsch, 1916). These observations were corroborated by the experimental work of Coninx-Girardet (1927), who found that a decreased activity in winter was very definitely paralleled by the reduction in the number of basophilic cells and a more open grouping of the cells of the pituitary. The histological conditions during spring and summer indicated increased activity for the breeding periods. On the other hand, Mann (1916), Rasmussen (1921), Bugbee, Simon and Grimes (1931), were all unable to discover any change in the hypophysis which was produced by hibernation. Johnson and Hannawalt (1930), injected pituitrin in doses of two hundred and twenty-two to four hundred and fifty times the human dose, on the basis of relative weight and found that in sixty-six of the one hundred and fifty marmots, it produced no change in the tendency to hibernate. They concluded that the secretion of the posterior lobe of the pituitary is not an important agent in production or prevention of dormancy.

Observations on the glandular portion of the hypophysis of *Rana pipiens*, made by the writer, showed marked changes between the tissues of the frogs in hibernation during the winter and those of the summer season. The nuclei were angular in shape and much larger, with fewer nucleoli during hibernation than in the active animals of summer. The nuclei as well as the cytoplasm of the dormant frogs were less basophile.

The Gonads

According to Oslund (1928), there is a seasonal variation in the testes and also in the relative quantity of constituent

elements in most animals that breed once each year. In the hibernating animal the change appears to be more pronounced than in the non-hibernating forms. Gonadal activity in general, tends to prevent hibernation in marmots in the spring, but not at other seasons of the year. Castrated animals hibernate to a significantly greater degree during the breeding season than do the normal individuals (Mann, 1916, Rasmussen, 1917–18, Drips, 1919, Shaw, 1926, Johnson, 1930).

In some hibernating animals the seasonal cycle of the testes reveal striking changes in the tissues of these organs (Moore, 1926). Hanseman (1902) observed that the hibernating marmot had only slight amounts of interstitial tissue between the seminiferous tubules. Typical Leydig cells were lacking and spermatogenesis was in abeyance. In the height of the breeding season, two months after the period of awakening, the Leydig cells were so large and numerous as to give the testes the superficial appearance of a large sarcoma. According to Ganfini (1903), the Leydig cells were not absent from the testes but were smaller during hibernation. Regaud (1900) found that active spermatogenesis in a mole was not accompanied by hypertrophy, for following the breeding season the interstitial cells became maximal and remained so after the disappearance of the generative portion. Two hibernating mammals may be entirely opposite with respect to the increase in Leydig cells, and decrease in the reproductive activity (Moore, 1926).

Gonadal hypertorphy has been found in toads and frogs during hibernation (Freidmann, 1898, Champy, 1913–24, Aron, 1921–24). In 1926, Barthelemy placed *Rana fusca*, in artificial surroundings and found sexual maturation, if kept at from 2–3°C., but, at 8–10°C., they remained active and did not mature sexually. He states that hibernation cannot be kept up indefinitely even if the temperature remains low, because it is ended by sexual maturation.

The author's findings in these studies on *Rana pipiens*, are comparable to those of *Rana clamitans* by Aron (1921), and *Rana fusca* by Barthelemy (1929). The frogs of November to March inclusive, all showed development of the secondary sexual characters. A very well defined seasonal cycle was seen in respect to variations in color of the testes. They were white in the summer and deep yellow in the winter. The frogs of March and April exhibited deep yellow or orange coloration in the testes; in May twenty-five per cent were deep yellow and twenty-five pale yellow, while in July the color was six per cent yellow and in August all of them were almost white.

Force (1933) noting this same difference in the color of the testes, attributed it to the age of the individual. She described the testes of fresh specimens as showing the deepening of color, with white for the younger frogs, and a range from white to deep yellow or orange as an indication of the age of the individual. Her observations agree with ours, but her interpretation differs from our conclusions, that the yellow testes indicate an animal that has reached maturity before the breeding season. Our interpretation is based upon the fact that two hundred frogs of a uniform size, displayed a definite range of color from white to yellow, but that the height of the yellow coloration came just before the breeding periods and the white just after. Close observation proved that in August some of the larger frogs of these experiments had white testes, while in March animals which were smaller in size had yellow organs.

In the histological, as well as the gross

structure of the testes of frogs placed in the refrigerator in winter, there was a marked difference from that shown in the animals placed in the cold in summer. Although the frogs were lethargic, the development of the gonads continued up to the time of awakening from hibernation. Frogs became active in March or April regardless of the fact that the temperature in the refrigerator remained exactly the same. The testes of the male frogs contained many sperm, and the coelome of the female was filled with eggs. Controls which were kept at room-temperature showed no enlargement of the gonads. The testes remained white in color and small in size, with very few spermatozoa. Ovaries contained very small eggs. In the event that frogs were not removed from the refrigerator immediately upon awakening from hibernation, they remained active for twenty-four hours, when death ensued.

The Adrenals

In mammals there is an enlargement of the adrenals in the spring, but very little change occurs in the winter, except in the blood vessels (Mann, 1916). The span of life for adrenalectomized marmots of the summer is from one to ten days, while the operated animals in winter, survive the normal period of hibernation in apparent health (Britton, 1931). Likewise, winter-frogs survive ablation for one or two weeks, whereas the summer-frogs die in as many days.

A definite cycle was observed in the gross appearance of the adrenal glands of the frogs throughout the year. This cycle was noted in the color of the bodies, in the staining reactions, in the character of the nuclei and in the varying presence of acidophilic granules. The glands of *Rana pipiens* and *Rana catesbeiana*, during hibernation and the breeding period, were deep yellow in color, with a beaded appearance. The glands of the frogs in June, July and August were merely inconspicuous red spots deeply implanted in the surface of the kidneys. By September the color became white or pale yellow, growing darker through October, November and December, reaching a deep yellow or orange in January and February, remaining thus until the breeding season.

In sections stained with haematoxylin and eosin, the cells of the cortex and medulla appeared to be more or less intermixed. The part corresponding to the cortex, consists of polyhedral cells, and those of the medulla were irregular and interspaced with large capillary blood-spaces. The most noticeable difference in the tissues of the adrenal glands of the dormant and active frogs is perhaps the variations in the affinity for basic dyes. In the hibernating frog, the nucleus and the minutely dispersed chromatin, as well as the cytoplasm were slightly basophile. Cytoplasmic granulations were seen, but they stained indifferently.

Acidophilic granules were found in the adrenal tissues of the active frogs. Another difference in the adrenal tissues of the active individuals was manifested in the shape of the nuclei. In the active frog, they were larger and round in shape, while in the dormant animal they were flat or triangular.

The Pancreas

Artificial hibernation has been produced in marmots by the injection of sufficient insulin into the blood stream to cause profound hyperglycemia. When this is done, the animal loses its power of control over temperature and if placed in a moderately cool environment, passes into a state of artificial hibernation, the characteristics of which are poikilothermism, a loss of consciousness, a lowered metabolism and

a loss of sensibility to painful stimulation. This state of torpor may be prolonged by the administration of insulin at regular intervals. The injection of glucose terminates the condition; as the temperature rises, shivering begins and the animal returns to normal (Dworkin and Finney, 1927). According to Dische, Fleishmann and Travani (1931) the production of artificial hibernation through over-doses of insulin, requires a higher degree of hyperglycemia than is encountered in the animal hibernating naturally.

Aron (1925) found the structure of the island cells of frogs in autumn and winter to differ from those of spring and summer. The island cells of the dormant animal showed repressed functioning, while the animals of spring and summer manifested activity. Cells of the hibernating type were totally absent in summer, though a few of the summer type are always present.

The gross structure of the pancreas gave little evidence of a seasonal difference between hibernating and active individuals. The color of the pancreas in a dormant frog is white as compared with that of the active animal, which is pink; this variation is due to the presence of erythrocytes in the glands of active frogs. Microscopically, a difference was shown in the cells of hibernating and active frogs. In winter, the cells were large and rich in cytoplasm, which had little affinity for haematoxylin. The nuclei were flattened or perhaps triangular and were not basophilic.

In the pancreas of the active frogs, the nuclei of the cells were round, containing many nucleoli. Around the nuclei of certain cells were many acidophilic granules. The cytoplasm was quite basophilic, in general, giving a darker aspect. Actual measurements of the size of the cells and nuclei were not made, but the microscopic examinations indicated that the nuclei as well as the cytoplasm of the cells are larger in the hibernating frog. The difference shown in the appearance of the pancreas of hibernating and active frogs could not have been due to errors of technique, for the tissues were similarly fixed, mounted on the same slide and stained together.

In the active frog, the islands appeared distinctly paler than did the surrounding cells, but in the dormant individuals all tissues were equally devoid of stain. No secretory granules were observed in any of the island cells.

The Thymus

In literature on mammals, no reference was found pertaining to the thymus as relates to a cause of hibernation. However, in the frog the thymus presents the most striking seasonal cycle of all the tissues studied. In the external appearance a seasonal cycle was observed. This gland is quite large in dormant animals and is of a deep orange color. It has very little blood in the veins or arteries. As frogs reach the end of the period of dormancy the color changes to a pale yellow, diminishes in size, and dissection is attended by much bleeding.

In the active frog the condition is quite the reverse, for the reason that the gland is quite small and so well supplied with blood that superficially it appears to be one large clot of blood.

Frozen sections of the thymus of a hibernating frog showed that the cells contained a yellow substance which resembled fat. These globules appeared unstable when freed from the lymphoid reticulum and varied in size, from a few large, to many very small ones. Routine histological technique dissolved out the lipoid material. (See Figures 1 and 2.)

Figure 1 shows a section of the thymus of a hibernating frog which was killed

February 27, 1934. In some parts of this tissue the cells contain only a large globule which squeezes the nucleus to the side, as in ordinary fat cells, while in other portions, the tissues are made up of cells containing numerous small globules which are grouped around a central nucleus. These cells are polygonal rather than round. Figure 2 shows a section of thymus gland from a frog which was kept in the cold and killed in July. The cells were not vacuolated and contained basophilic granules.

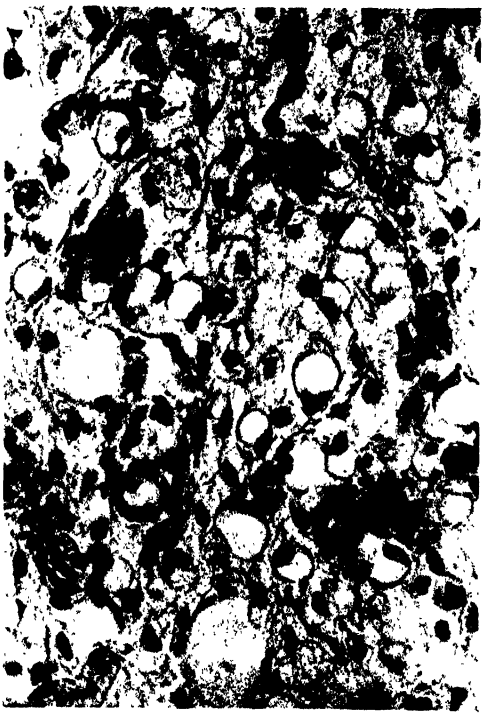


FIGURE 1. SECTIONS OF THYMUS GLAND OF A HIBERNATING FROG

Magnification 300. Frog was killed February 27, 1934 and tissues were fixed in Bouin's fluid and stained with haematoxylin and eosin.

The Thyroid

No histological evidence has been adduced to prove that the thyroid has a part in the initiation of dormancy, or, in causing the awakening (Mann, 1916, Johnson and Hanavolt, 1926, Gorer, 1930), but it appears that the increased activity of the gland in the spring accompanies the awakening (Coninx-Girardet, 1927). Histological changes have been found in the thyroid of bats and hedgehogs, that would indicate excessive secretion of colloid just after awakening in the spring. In the fall there is a diminishing of colloid and a flattening of the cells (Adler, 1920-27, Schenk, 1922).

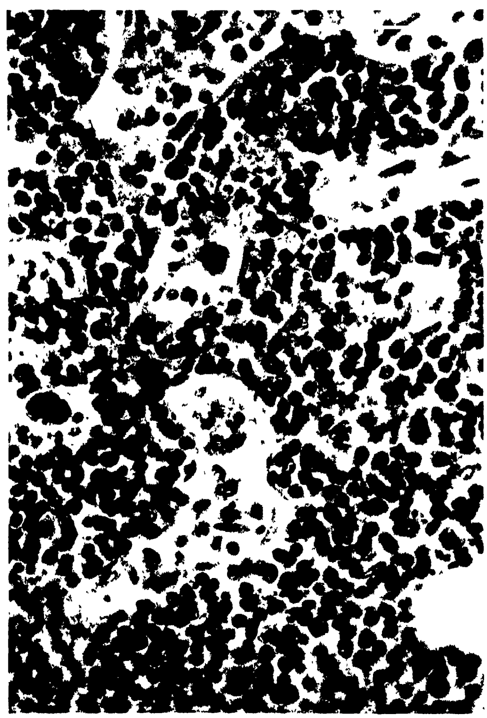


FIGURE 2. SECTION OF THYMUS GLAND OF FROG KEPT IN THE REFRIGERATOR AND KILLED IN JULY

Magnification 300. Fixed and stained as in Figure 1.

Sklower (1926) found that during adult life in frogs, the thyroid undergoes certain cyclic changes through the storing of colloid in winter and releasing it in the summer. A seasonal cycle in the gross anatomy of the thyroid glands of the female frogs of *Rana pipiens* was observed, in that this gland was larger at

the breeding periods, and smaller during hibernation of the animal. In the hibernating frog the thyroid gland of the female exhibited a deep yellow color, though the male showed less coloration. Glands observed in the non-hibernating frogs appeared to be red, due to the fact that there was much blood in the tissues. The thyroid gland of the hibernating frog was definitely inconspicuous and all attempts to dissect it in the active animals were attended by profuse bleeding.

The Hibernating Gland

Reference to a structure, called the "Hibernating Gland," appears often in the older literature, as a cause of hibernation. The species in which this gland has been found, its location in the body and the authority therefore, are to be found in a table originally compiled by Auerbach (1902), added to by Polimanti (1902) and finally extended by Rasmussen (1923).

This glandular type of adipose tissue was described as being functionally related to the endocrine glands, especially the thyroid and adrenals, and in order of their importance, the regions most often mentioned as a site of the gland, are the thoracic, cervical, axillary, scapular, renal and inguinal (Cramer, 1920). No reference of such a structure in frogs could be found in the literature, but an interesting similarity was observed by the author, between the structure as described for the so-called hibernating gland, and the thymus of the frog.

The Spleen

Mann & Drips (1917) made the statement that within twelve hours after a mammal became torpid, the spleen was found to be greatly enlarged and much darker in color. Microscopically, the organ presented a most intense congestion, but the removal of the spleen was found to have no effect on hibernation.

A similar condition was observed in this work. Besides congestion there was apparently little change in the spleen, though a seasonal difference was strongly manifested in the size and color. In approximately two-thirds of the individuals studied, the spleen of the hibernating frog was found to be from two to three times the size of that exhibited by the active animal, and the color was purple, while that of the active frog was usually reddish brown.

Jordan and Speidel (1925) studied the spleen of the frog in the spring after hibernation, when the marrow in the long bones is hemopoietically active. During this period the spleen showed little activity, and appeared to be merely a reservoir for the blood-cells.

In general, the tissues of all the ductless glands studied revealed characteristics in common, for the seasons of the year. In the hibernating frogs the nucleus contained much less chromatin, were larger, less basophilic than those of the active animals. The cytoplasm lost its basophile character and if granules were present in the cytoplasm they did not stain with acid or basic dyes. In the hibernating frogs the nuclei were more or less triangular in shape and there was usually only one nucleolus present. In the active frogs the nuclei were round in shape and contained several nucleoli. The nucleus and cytoplasm showed definite affinity for basic dye, while the inclusions of the cytoplasm were very acidophilic.

Kater (1927) compared the skin and stomach of the hibernating frogs with the active animals. The author's findings on the glands of frogs proved to be very similar to his work.

The results of these observations on the glands of frogs, do not justify the conclusion that the function or lack of function of any one of the ductless glands is a direct cause of the tendency to hibernate. Undoubtedly the ductless glands have an important part to play in the development, duration and awakening from hibernation, but it is the writer's view that the functioning is displayed only as a regular phase of the seasonal cycle.

Chromatophores

A change of color is directly associated with dormancy. The color of the hibernating frog was a dull black. The frogs used as controls lived until January, and were black also. When the experiment began in October, these frogs were a light tan color, but changed to black as the season advanced. These changes of color are induced by an alteration in the form of the chromatophores. The migration of pigmented cytoplasm within the melanophores produced the darkening of the color of the skin. Green skin darkens because the melanophores expand, and when the limit of expansion is reached the skin of that region is very dark.

Change of color is also influenced by various external and internal factors. Low temperature, humidity and darkness will cause the expansion of the melanophores and consequently a darkening of the skin. High temperature, desiccation and light will induce a contraction which results in a lighter color. Frogs used for the controls showed a complete change of color during October, November and December, and all the animals unable to make the change in color died. In September, all frogs exhibited a golden brown color.

In these studies, a change of color was not directly due to the action of environmental factors. All experimental animals were kept in the dark, yet all of them passed through the change of color, i.e. dark for winter, changing through shades of brown to green in the spring; a greenish brown in summer and silvery brown in the month of September. All frogs which were unable to make this change of color died.

The Muscles

Although the response of the muscles to electrical stimulation during hibernation, would appear to be a much more obvious matter than the chemical dynamics which these muscles exhibit, the latter seem to have received considerably more attention. Increased tonus of certain groups of muscles cause the hibernant to assume a characteristic position. Muscles have been classed as, the red, those which maintain posture, and the white, those by which movement of the body is accomplished. Rodents roll themselves in their nests, the striped ground squirrel sits on the posterior surface of the hind legs, with the back strongly arched. The various species differ slightly in minor details of position, but in general, the outside curvature of the body almost describes a circle, and only a small gap in the middle preventing the completion of a perfect sphere (Johnson, 1931). This position is maintained by certain muscle fibers. The red muscle fiber which differs chemically and physiologically from the white, is a tonic one and its contraction holds the body in position.

In addition to the seasonal change in the tone of the postural muscle Wacholder (1932) found a seasonal variation in the reaction to acetycholine in the tone of these muscles. This drug causes a strong contracture. In the hibernating mammal, most of the muscles give a stronger contracture for a longer period of time,

with a weaker solution of acetycholine, than at other seasons of the year.

By chemical, histological and pharmacological experiments, Krüger, Duspiva and Fürlinger (1933) have demonstrated these muscle fibers, and have given to them the terms "tonus and tetanus portions." These two types of muscles differ structurally. In winter months, a decrease in the stimulation sensitivity of the tetanus portion was noted. These portions were characterized always by a higher creatin phosphate content. Ferdmann and Feinschmidt (1932) showed that in the hibernating frog there is a decrease in several substances which are important for the contraction of muscles. They believed that decreased excitability of the gastrocnemius muscle of the frog is accompanied by the slowing of decomposition of phosphagen. Phosphagen is not present in the tonus muscle portions (Krüger, 1933). Katz (1934) found a seasonal variation in permeability of frog-muscles to creatin.

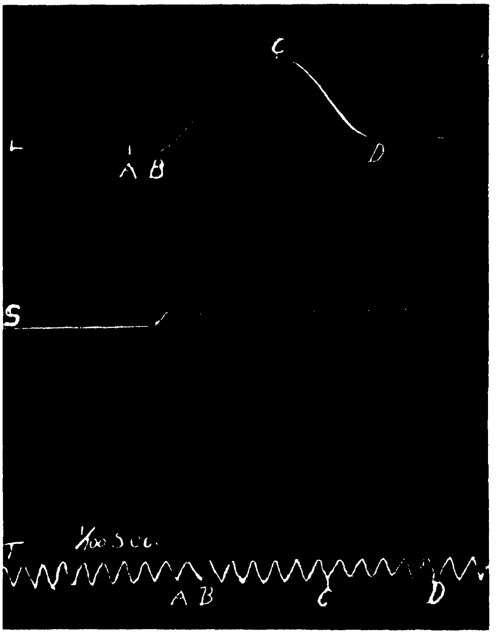


Figure 3. A Single Twitch of the Gastrocnemius Muscle of a Normal Frog Made in October

A–B, latent period.
B–C, period of contraction of muscle.
C–D, period of relaxation of muscle.
S, signal magnet indicating application of stimulus.
T, vibration of tuning fork at 100 per sec.

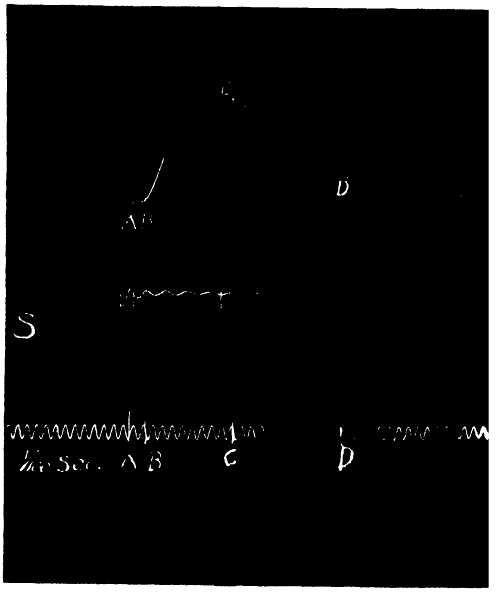


Figure 4. A Single Twitch of Gastrocnemius Muscle of a Hibernating Frog, Made in February. Contraction is Response to a "Make" of Current

B—C, shows the apparent secondary release of energy during contraction of muscle.

C—D, shows the delayed relaxation of the hibernating frog.

The increase in tonus, found in the muscles of *Rana pipiens*, has been mentioned and fully described in the discussion of temperature, earlier in this paper. These positions, characteristic of the hibernating frogs, display the definite reactions of the "tonus portions" during dormancy. According to Krüger (1933), these muscles were pure "tonus fibers" (*rectus abdominis*, *pectoralis major*, and the muscles of the shoulder girdle), and the mixed muscles (*gastrocnemius*, *ileofibularis*, *semimembranosus*) are strongly contracted and if gently touched at the surface of the body, they slowly increase in tonus with a wave-like movement.

Cold could not have been the chief stimulus for this increase in tonus in these hibernating frogs, for all the frogs were subjected to the same environment; yet three different results were obtained, i.e. extreme tonic contraction in the hibernating frog of winter, moderate tonus in the sub-active and very little tonus for the active individuals of summer. With the animals used for controls, and kept under atmospheric temperature environment, a progressive increase in tonus was observed as the winter approached, and only the frogs which were influenced by this increase lived through the winter.

A seasonal rhythm was indicated by the curves in response to repeated electrical stimulation applied to the gastrocnemius muscle of *Rana pipiens*. Progressively each month, from January to April, the muscles were able to do more work than was recorded for the preceding month. The averages for the different groups for successive months, showed that the right gastrocnemius muscle of *Rana pipiens*, performed 3870 Gr. mm. of work in January; 5364 Gr. mm. in February, 7721.4 in March, and 13511 Gr. mm. of work in April. With the technique used, the frogs of January fatigued in eight minutes, while the active frogs of April performed work for ten minutes before the onset of fatigue.

The characteristics of the curve for the fatigue of muscles in the hibernating frogs are (1) a contracture, (2) an increased irritability to the tetanizing current when applied to an apparently fatigued muscle, and (3) a decrease in the sensitivity of the muscles to electrical stimulation. To bring a threshold response in the hibernating frog, it is necessary to set the inductorium at point nine, while five points below this number could be effective in finding a contraction in the active animal.

The single contraction of the gastrocnemius muscle of the hibernating frog differs from that of the active individual. There was an apparent secondary release of energy during the contraction, while the curve of the active frog sloped uniformly to the contraction maximum. This departure from the mormal type was constant for the fresh as well as the fatigue muscle. To determine whether or not, this unusual contraction was confined to dormancy, the curves of several other species were made. *Acris*, a non-hibernating frog, two different species of *Pseudacris*, and a narrow mouthed toad (*Gastrophryne*), of the same size range, were used. *Acris* and the hibernating *Rana pipiens*, made totally different curves from those of the other three species. They both displayed the same irregular contraction which was not observed in either *Pseudacris clarki* or *Gastrophryne*. This secondary phase of contraction with *Acris* is more distinct and especially noticeable in the response to the tetanizing current.

The Heart

No agreement was found in the literature, relative to the rate of heart-beat in its relation to temperature. Some observers have found that the curve showing the relationship, is a straight line within the range of normal temperature fluctuations (Taylor, 1931). Others have maintained that the curve is of logarithmic type. Barcroft and Izquerdo (1932) carried out an experiment to determine the effect of changes in temperature on the intact and excised heart of frogs at different seasons of the year. One set of their tests made in December and January, gave a different result from those made in July. The effect of alterations in temperature on the heart-beat of the intact frog in winter, gave results which were incon-

sistent with one another, and in most instances were quite irregular. Usually, for the excised heart of frogs in winter to give a linear curve, it was necessary to use the relationship between the logarithm of frequency of the sinus beat and the reciprocal of absolute temperature between 5 and 20°C. The excised and also the intact heart in summer gave a straight line when pulse rate was plotted directly against temperature.

Taylor (1931) confirmed the above results for his experiment on the intact and excised heart, which he made in June. He has shown also, that the British frogs differ from the South African forms in the response to temperature, and accounts for it in part, by the fact that he found the heart of the South Africa frog subject to vagal tone at temperatures neither very high nor very low, and in the British frog the vagus tone appeared to be absent.

In *Rana pipiens*, used in the writer's experiments, the rate of heart-beat showed a seasonal cycle. The heart-beat of the dormant animal was irregular, and instances of heart-block were common. The ventricular beat failed many times, and a complete missing of the beats was not uncommon. A response to a rise in temperature was often shown in the slowing rather than an increase in the rate of the heart-beat. The same results were observed in the reactions of the sluggish frogs of winter. The frequency of the heart-beat in both the intact and excised heart of frogs of summer, bore a linear relationship to temperature, but in the hibernating frog the tone of the vagus was practically absent. The vagus did not maintain an inhibitory action on the heart-beat at any temperature during the months of winter, though the heart could be completely inhibited for a minute or more in the summer when the tetanizing current was applied to the vagus trunk. Thus it appears that a seasonal rhythm is to be found in the activity of the cardiac muscle of *Rana pipiens*.

The Blood

The relation of the blood to hibernation has been studied but little. Vierodt was the first to describe a change in the number of erythrocytes during hibernation, (Heilkundi, 1854). He found in a marmot at the beginning of hibernation, 5,800,000 erythrocytes per cubic millimeter of blood, but at the end of lethargy only 2,300,000 corpuscles were shown; and he also noted a diminution in the size of the cells.

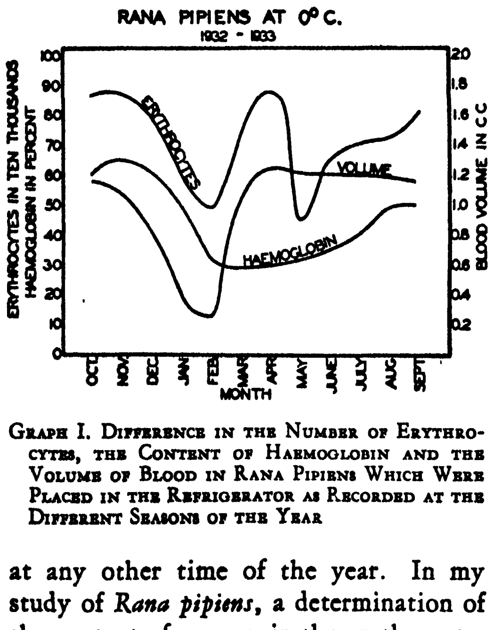
The hibernating *Rana pipiens* had a decreased number of erythrocytes as compared with the other seasons of the year. This reduced number was also found in the sub-active frogs during the winter months. The difference in the number of erythrocytes as beween the hibernating, sub-active and active frogs was very small, but the seasonal rhythm was found in all of the frogs studied in that, during hibernating periods the number of erythrocytes was reduced, while the highest count was made at or near the breeding seasons.

As the period of lethargy approached, a progressive fragmentation of erythrocytes was shown in the blood of the hibernating frogs. When the spring time came there was renewed activity of the hemapoietic tissues and also many small round cells with large nuclei.

Jordan and Speidel (1925) gave the life of any one erythrocyte of a frog as not over one hundred days. Since fragmentation was quite generally observed during the winter, it is possible and reasonable to expect the number of erythrocytes in the dormant animal to be reduced. This reduction in the number of erythrocytes is much greater than it would appear

from taking one blood count, as reference to Graph 1 will show that the decrease in blood-volume parallels the reduction of the red blood cells. The blood-volume also increases in the spring as to the erythrocytes, thereby making a greater increase than is apparent, and thus accounting for the change in the appearance of the erythrocytes in the frogs of spring as compared with those of the late frogs of summer.

Asakawa (1926) found that the erythrocytes of *Bufo* during hibernation showed a higher content of oxygen than



Graph I. Difference in the Number of Erythrocytes, the Content of Haemoglobin and the Volume of Blood in Rana Pipiens Which Were Placed in the Refrigerator as Recorded at the Different Seasons of the Year

at any other time of the year. In my study of *Rana pipiens*, a determination of the content of oxygen in the erythrocytes was not made, but a seasonal cycle was found in the content of haemoglobin, with the lowest percentage present in the hibernating frogs of the winter season.

In addition to the red corpuscles contained in the blood, there are many specialized bodies, called spindle cells. These are supposed to be normally present in the blood of frogs to the number of 16,000 per cubic mm (Nobel (1931)). In frogs of summer these spindle cells are very unstable and are not seen unless great precaution is taken, but in the blood of hibernating individuals they were easily seen. It is thought that they assist in the clotting of the blood; in the dormant frogs this process is easily watched for in some instances the time of clotting covers a period of five minutes, while in the active animal of summer, the process takes place very quickly.

Dubois (1898) and Johnson (1931) thought that in mammals, a very slow circulation was maintained during hibernation. In *Rana pipiens*, the circulation was slow, and during lethargy the blood appeared to be confined to the large vessels and the viscera. By microscopical examination of the foot and tongue, and by dissecting the hibernating frog, a very slow circulation of the blood was shown. Dissection may be performed on frogs in the winter almost without loss of blood. This is of course due in part, to a decreased volume, and to an increase in the specific gravity of the blood of a hibernating animal. The averages shown in the specific gravity of the blood vary from one and five-hundredths in the active animal, to one and seven-hundredths in the hibernating frogs, although different frogs of both groups showed a wide variation.

Since the volume of blood is not constant, any attempt to define it as a percentage of the weight of the body must be misleading. The smaller frogs according to their length, have a volume relatively greater than that of the heavier individuals of the same group. No marked difference was found in the volume of blood between sexes, nor between the experimental and control animals. In the entire series, the frogs used in this study, ranged from fifty-five to eighty millimeters in length, and from nineteen to twenty-five grams in weight at the

beginning of the work. The volume of blood in the dormant frog did not exceed one-fourth cubic centimeter.

SEASONAL CHANGE OF WEIGHT

It is generally believed that there is a considerable loss of weight in the body of mammals during hibernation. There is but a slight agreement in the amount of loss. Many believe that animals which enter hibernation are fat, and when they awaken they have become lean in body (Murray, 1896). Bailey (1926), and Johnson (1931) report slight losses, while a slight, temporary increase in weight has been reported by Horveth (1878) and by Dubois (1896). This condition has been accounted for by Valentin (1896) as caused by an accumulation of moisture, or by fixation of oxygen with retention of carbon dioxide (Pembrey and White, 1896).

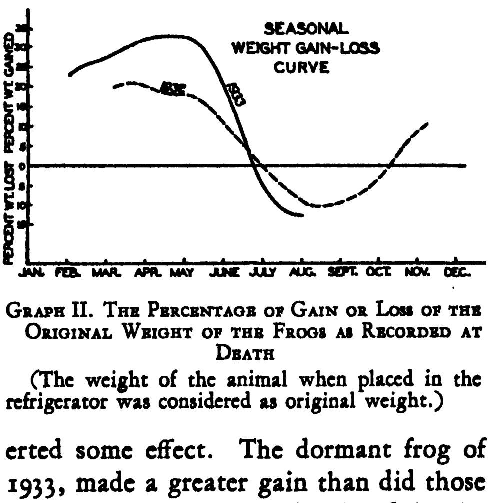
According to Barthelemy (1926), when frogs (*Rana fusca*) were placed in a temperature of from 2–3°C., in October, they gained weight. Above 8–10°C., they remained active and lost in weight of body. He further states that during sexual maturation there is an increase in weight, and that hibernation is ended by sexual maturation.

The frogs (*Rana pipiens*) studied by the writer, when placed under experimental conditions of 0°C. temperature and saturated humidity, without food, gained weight from October to April, and lost weight from April to October, as did also the individuals placed in the atmospheric temperature. They showed an initial gain of weight when placed in the cold in October, and there was no loss from February to March, nor any gains from May to August.

Graph 2 is a curve of the change of weight as related to the seasons, showing April and May to be the months of greatest gains and August and September as that of greatest loss. Had there been an experiment beginning in November and December, this curve might have been completed for the annual cycle.

Although sexual maturation did take place during hibernation, and the coelome of the female was well filled with eggs at the time of emergence, the presence of eggs could not account for the increase in weight, because the increase in weight in the males almost equalled that of the females.

A variation was found in the degree of gain for the different years. The influence of atmospheric temperature upon the frogs before the experiment began, exerted some effect. The dormant frog of 1933, made a greater gain than did those of 1932, and the animals placed in the cold in March 1933, showed the greatest gain with the most sudden reactions.



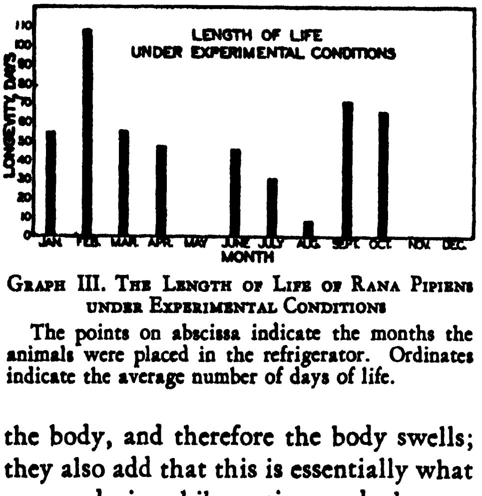
GRAPH II. THE PERCENTAGE OF GAIN OR LOSS OF THE ORIGINAL WEIGHT OF THE FROGS AS RECORDED AT DEATH

(The weight of the animal when placed in the refrigerator was considered as original weight.)

Reference to the weather conditions to which these frogs had been subjected just before the experiments began, shows that in December, the month preceding one experiment, the temperature averaged below normal for the year 1932, while February, the month preceding another group, was the coldest month for 1933, with March the wettest month. These two groups recorded increased gain of weight, with the second group showing

the greatest gains and the most sudden reactions.

The weather reports for February 1932, show unusually mild temperature in all sections of the United States, making the sixth consecutive month with abnormally high temperature for Wisconsin, where the larger number of frogs used in this work were obtained. Undoubtedly some physiological factor was responsible for the change of weight, but it appears that temperature must have played some part. Donaldson and Shoemaker (1900) asserted that at a temperature of zero degree centigrade, the kidney of the frog does not excrete water as fast as it comes into



Graph III. The Length of Life of Rana Pipiens under Experimental Conditions

The points on abscissa indicate the months the animals were placed in the refrigerator. Ordinates indicate the average number of days of life.

the body, and therefore the body swells; they also add that this is essentially what occurs during hibernation and that an increase in the content of water is evident in all the tissues of the body during dormancy.

Temperature could not have been the only cause of the gain of weight in *Rana pipiens*. The temperature in the refrigerator was practically constant yet they gave two responses in change of weight. The atmospheric temperature for the controls, for the most part registered in the seventies for the period preceding observations made in June, the initial response of which, was a loss of weight, while the temperature in September registered in the eighty and ninety range and the frogs gained weight. In October the increase in weight made by the controls showed when plotted, an irregular curve, superimposed upon a straight line of gain in weight, until February. Undoubtedly, cold is the optimum condition for the operation of the physiological factors responsible for the gain of weight.

Two hundred frogs grouped in eight experiments, formed the basis of curves, which show a gain of weight for the winter and a loss for the summer, in response to zero temperature and saturated humidity. Since the animals took no food at any time, the increase of weight must be accounted for in the exchange of water in the frogs. "In the normal frog the skin is the one organ of intake, and the kidney the important organ of output" (Adolph, 1933).

Reid (1890) studied the osmotic action of the skin of the frog and reached the conclusion that the easiest osmotic transference of liquid through the skin of the frog is in the direction from the outer toward the inner surface, suggesting the possibility of a definite absorptive force in the living skin. Maxwell (1913) questioned this view, while Townsend (1921) tried to apply the same principle to the behaviour of the skin of the toad and concluded that some sort of absorptive force of the skin seems to be the important factor in preserving the normal content of water in the body of the toad.

Having therefore, removed the influences of food, temperature. and humidity by these investigations, and finding still a seasonal variation well defined, we are forced to conclude that a seasonal change of weight must be due to the action of one, or of a combination of several physi-

ological regulators, which we may suppose are to be found in the nervous system and the hormones.

The reactions of *Rana pipiens* and *Rana catesbeiana*, were apparently identical in the seasonal change of weight but the reactions of *Acris*, a non-hibernating frog, were not comparable.

A definite seasonal cycle was clearly shown in this study, in the length of life under refrigeration, and it corresponds with the seasonal gain or loss of weight (Graph 3). The frogs put in the cold in January lived fifty days or more, while one animal lived for one hundred and eight days. The records of March showed decline from the level of figures for January, and the frogs of April lived for forty-eight days. No frogs were placed in the refrigerator in May. The duration of life in the animals of June and July decreased until August, when it was only nine days. September showed a gain of more than seventy days, although the records for October indicate a decline, due perhaps to inability to control experimental conditions in the refrigerator. It should be noted here that although under refrigeration, not all of the frogs hibernated, for some of them remained sluggish, and the term "sub-active" used in this work, is applied to these individuals, in order to include their reactions also.

In view of the findings of these investigations covering the three years of observation on the gains and losses of weight for *Rana pipiens*, it seems reasonable to conclude that a seasonal rhythm is shown in the change of weight in frogs, when placed in constant conditions of temperature and humdity.

GENERAL SUMMARY

A seasonal cycle was observed as follows: (1) Beginning in October and continuing throughout dormancy, the "winter-frogs" gained weight, (2) acquired a darker color of the skin, (3) the tonus of the muscles increased, (4) the volume of blood decreased, (5) the number of erythrocytes was reduced and the percentage of hemoglobin decreased, (7) the muscles soon fatigued, (8) they showed delayed relaxation and an uneven phase of contraction, (9) the heart, at the period of greatest lethargy was irregular and did not respond to changes in temperature according to van't Hoff's law, and (10) it was not completely inhibited by the stimulation of the vagus nerve, (11) the thymus, adrenal gland and the gonads changed from white to orange color, (12) cells became large, slightly basophilic with no acidophilic areas, and (13) the nuclei contained few nucleoli and were flattened or triangular in shape. In the summer, the opposite conditions were observed, beginning about March and continuing through the season. The cells of the glands studied were strongly basophilic, with areas of acidophilic inclusions, and contained many nucleoli. Regardless of environment, the same seasonal rhythm was observed for all *Rana* studied, and true dormancy could be obtained only in winter.

Frogs of non-hibernating forms (*Acris*), did not respond to a change of temperature, in like manner to *Rana*, or undergo a seasonal change of weight.

Hibernation appears to be one phase of the seasonal cycle which is an intrinsic function of the animal itself, and in *Rana pipiens* and *Rana catesbeiana*, is not fundamentally dependent upon environment.

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IS THERE A "HORMONE OF MENSTRUATION"?

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UTERINE bleeding, with endometrial necrosis, follows a number of experimental procedures in monkeys, usually after a latent period of 5 to 7 days, namely: (1) after removal of the corpus luteum; (2) after ovariectomy performed at any day of the menstrual cycle, except the first three; (3) after spinal transection; (4) after injection of estrin; (5) after injection of progestin. The cause of menstruation has been stated to be: (1) withdrawal of estrin (Allen, 1, 20, Corner, 2), or (2) withdrawal of progestin (Smith and Engle, 3), or (3) a hypothetical extra-ovarian factor (Hartman, Firor and Geiling, 4). The latter authors questioned the adequacy of the "hormone-withdrawal" theory. In the first place it seemed difficult to imagine a vasomotor mechanism operating days after complete disappearance of the alleged causative factor, estrin, as must of necessity be the case when the latent period comprises 8 or 10 days. Bleeding will also take place in the presence of large amounts of estrin, sudden reduction of dosage being sufficient to precipitate it (Hisaw, 5). Furthermore, induced bleeding, in castrates, often lasts 20 or more days; and with continuous injection of estrin (Hisaw, 5; Engle and Smith, 6; Hartman, unpublished) or progestin (Hisaw, 5, Engle and Smith, 6) over a period of months the bleeding sets in from time to time despite the injections. Likewise, estrin will not always inhibit the normal bleeding if injections are begun after the middle of the cycle (Hartman, 7) but is effective in this respect if begun early in the cycle (Corner, 8; Tietze, 9; Markee, unpublished). These and other facts seemed to argue for an extra-ovarian factor in menstruation.

Finding that the hypophysectomized monkey bled insignificantly or not at all after estrin injections, Hartman, Firor and Geiling in 1930 attributed a bleeding factor to the anterior pituitary. In 1934 and 1935 we took up the matter again for further testing.

Seven females were operated on in October 1934 and in January 1935. Six bled after estrin injections and in all of these more or less of the gland was left in the sella. The estrin (Amniotin Squibb) was generously furnished by the biological supply house of E. R. Squibb and Sons, through Drs. Anderson and Morrell, for these and many other experiments performed in the Carnegie monkey colony over a series of years. Monkey No. 310 did not bleed; at autopsy the anterior pituitary was found intact but the posterior lobe was completely missing. Our previous conclusion of 1930 was, therefore, in error, and the experiment pointed to the vaso-constrictor hormone, pituitrin, as the possible active principle. This seemed a logical conclusion for other reasons also (Hartman and Firor, 10).

In February 1935, seven females were operated on, an attempt being made to remove the anterior lobe completely, leaving the posterior lobe intact. All animals bled after a course of treatment with amniotin. At autopsy the sellae of

four animals were found to contain gland substance of all the lobes; the other three sellae contained posterior lobe and pars tuberalis only. The anterior lobe was, therefore, definitely ruled out as a source of the postulated extra-ovarian factor (Hartman and Firor, 10).

Among a group of animals operated on in January, 1936 there were two that bled spontaneously after the operation, as after castration. At the time of autopsy of animal No. 386, killed at the time of induced uterine bleeding 41 days after the operation, the sella contained small portions of both pars anterior and pars nervosa in a very degenerated condition, to all appearances too necrotic to function.

More nearly complete hypophysectomy marked the experiments of Smith, Tyndale and Engle (11) who removed the entire pituitary with the exception of the stalk and the pars tuberalis. All the animals showing a reddened sex skin bled following the operation and in all of them bleeding followed the cessation of estrin or of estrin plus progestin injections, if nor at the first trial, at least upon the recovery of the subjects with proper nursing care. These careful experiments of the Columbia group, therefore, definitely eliminated the anterior and the posterior pituitary from consideration as causative agents in menstrual bleeding, except in so far as the anterior pituitary regulates the ovarian cycle. There remains only the remote possibility that the stalk may be involved, for according to Fisher, Ingram and Ranson (12) the stalk and the pars tuberalis suffice to prevent diabetes insipidus, and Atwell and Marinus (13) found pressor substance in the stalk of the bovine hypophysis. However, with varying doses of pituitrin, continuously administered for 5 hours, Hartman and Geiling (14) were unable to produce menstrual bleeding in the female monkey. Markee, in this laboratory, likewise failed to induce bleeding by 6 hours continuous injection of pituitrin into the blood stream, although considerable if not complete blanching of the intraocularly transplanted endometrium could be observed.

With the pituitary gland thus disposed of (for the present) as a probable causative factor in menstruation we must look elsewhere for an extra-ovarian "cause," for the "withdrawal of estrin" theory is, in our view, far from complete. Estrin may, indeed, be withdrawn gradually without bleeding resulting, as Markee, working in the Carnegie Colony, has shown during the past year (unpublished). Nevertheless, it may be said that withdrawal of estrin, with consequent rapid regression (shrinkage) of the endometrium, seems to be an essential precursor of the bleeding and constitutes the common denominator of cyclic or induced menstruation. And yet it is a fact that the endometrium of the rabbit or the guinea-pig is stimulated to growth by estrin, regresses upon its withdrawal, yet does not blanch, slough and bleed—that is, it does not menstruate. Wherein lies this difference in response between the endometrium of the lower mammals and that of the catarrhine monkeys and man?

Further investigation of the problem has been greatly facilitated by the researches of Markee (15) on the behavior of the blood-vessels in intra-ocular transplants, observations that find corroboration in the histological studies of Bartelmez (16) on human uteri and of Darron (17) on the blood vessels of the monkey uterus. Markee has noted that the constant and essential event leading to menstruation is not the hyperemia of the endometrium, as we are currently led to suppose, but quite the opposite, namely profound vaso-constriction of the spiral

arterioles, causing death-like blanching of the tissues for a period of 4 to 24, most often 6 to 12, hours preceding the bleeding. The circulation in the basalis goes merrily on, however, during the blanching period. Hence the anemia, and not the preceding hyperemia, constitutes the immediate and indispensable precursor of menstruation. This feature is exactly the same in ovulatory and anovulatory cycles, in animals recently ovariectomized, and in those having been stimulated by estrin. It makes not the slightest difference whether a corpus luteum is present or not—the sequence of events is the same without it as with it. The corpus luteum must be relegated to a minor rôle so far as menstruation is concerned, even though progestin, probably by desensitizing the arterioles of the endometrium, may experimentally prevent the onset of the uterine bleeding (Engle, Smith and Shelesnyak (18)).

A further fact of the utmost importance was brought out by the histological studies of Bartelmez (16) and amplified by the direct observations of Markee (15), namely that but few arterioles in an entire uterus bleed at any given moment. If the mucosa should "menstruate" at all points at once there would be one momentary gush and all would be over in less than two minutes. Neighboring fields supplied by separate arterioles may blanch and bleed at extensive time intervals, hours in fact. This phenomenon might argue against a general hormonal control, though a local hormonal control must be reckoned with. Anyone who has read the classic book on the capillaries by Krogh or considered the action of the "H" substance demonstrated by Thomas Lewis in the control of blood vessel calibre must gain a profound respect for the potency of infinitesimally minute quantities of chemical substances in their vasomotor action. It is quite possible that the hematomata that precede the endometrial blanching release the vasoconstrictors that result in anemia, anoxemia, necrosis, and menstruation. This would make the phase of congestion that produces the hematomata through vascular stasis one of importance in the initiation of the menstrual bleeding.

In summary, our present conviction is that the menstruation problem resolves itself into a study of the spiral arteries of the zona functionalis—morphologically, physiologically, pharmacologically.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

EVOLUTION AND HEREDITY. *Theories and Problems.*

By Charles E. Walker. A. and C. Black, London; Macmillan Co., New York. $2.10. 8 x 5½; ix + 222; 1936.

The author of this work is a neo-Darwinian and his book is quite frankly a piece of propaganda (in the correct sense of that much abused term) on behalf of the theory of evolution by natural selection. That the author has exercised great care in his choice of material is evidenced by his bibliography which contains 184 dated items, of which 156 are over twenty years old.

When the doctrine of natural selection was given to the world more than three quarters of a century ago it was believed by those who accepted it to be adequate to account for the entire process of organic evolution but in these modern days of mutations, allelomorphs, and lethal genes it may be seen to be only one (albeit an important one) of many factors interacting to produce an intricate and complicated pattern of organic progress. The author has devoted about one third of his book to a lucid and unprejudiced critique of some of the non-Darwinian theories, among them those of Mendel, Johansen, De Vries, Kammerer, and Harrison (though his treatment of the last is a bit sketchy) and these chapters constitute the most valuable feature of the book. In fact, a layman entirely ignorant of the work of these men could pick up more by reading about them in Dr. Walker's book than by going to the originals.

Walker believes that the laws of Mendelian inheritance hold for chromosomatic but not for cytoplasmic inheritance. Further, saltatory variations such as those on which the mutation theory of De Vries is based are determined by the chromosomes while fluctuating variations such as Darwin hypothecated are governed by the cytoplasm. And characteristics which discriminate varietal forms within the species are chromosomatic, while those which discriminate one species from another are cytoplasmic.

There is an index of five finely printed pages, but the lack of adequate illustrations is a serious defect.

MAN: A SPECIAL CREATION.

By Douglas Dewar. Thynne and Co., London. 3s. 6d. 7⅛ x 5; 123; 1936.

In this little book Dewar has tried to show that man, and in fact every living thing, has come into existence by special creation and that every organism is today, with minor variations, exactly as it was when it was created. This theory of course opposes the theory of organic evolution and even more, the theory of the descent of man. The author tries to prove his point by arguments based not on the evidences of special creation, but on the weaknesses and fallacies in the theory of organic evolution. The author's knowledge concerning natural selection and his interpretation of modern theories in genetics has led him to believe that much of Darwinism is "special pleading based not on the rock of scientific proof, but on

the sands of the imagination." He is quite alert in pointing out the "inconveniences and inconsistences" in the Darwinian school of thought and his discussion is worthy of serious consideration.

Dewar is no fundamentalist quack. On the contrary he is a distinguished naturalist of long and varied field experience. While we hold no brief for him or his conclusions, nevertheless we are of the opinion that his observations and ideas cannot be neglected. His strongest conviction is that "it must be realized that since the theories of Organic Evolution and the Descent of Man lack definite scientific proof, they should be taught not as proven facts, but rather as very broad generalizations."

MEMOIR ON FOSSILS OF THE LATE PRE-CAMBRIAN (*Newer Proterozoic*). *From the Adelaide Series, South Australia.*

By T. W. Edgeworth David and R. J. Tillyard. Angus and Robertson, Sydney. 7s. 6d. $8\frac{3}{8}$ x $5\frac{3}{8}$; xi + 122 + 13 plates; 1936.

The significance of this work is far greater than one would suspect from its title, for if the conclusions reached by its authors are upheld they will go far toward revolutionizing our ideas concerning the ancestry of the Arthropoda.

The Upper Proterozoic strata which constitute the Adelaide series lie in a horizontal position, substantially the same as when first deposited. There has been no tilting, no faulting, and no metamorphosis, and consequently its fossils are in a remarkable state of preservation. Until their discovery in 1928 the only pre-Cambrian arthropod fossil was *Beltina danai* described by Walcott in 1899 from Montana, but these specimens were so fragmentary that it is not possible to fix their taxonomic position with any certainty. On the other hand, the new genus *Protadelaidea* here described has been so well preserved that its construction has presented no very great difficulty.

Briefly the conclusions arrived at by the authors are as follows; The Arthropoda were already differentiated as a phylum in the Lower Proterozoic from which no fossils have as yet been recovered. But this primitive arthropod did not resemble the annelids so much as some of its modern descendants do, for it had but few segments and these were mostly cephalic. The thorax and abdomen were but slightly developed and bore no appendages. From this primordial ancestor there have been four distinct lines of descent—one to the crustacea, one to the trilobites, one to the pycnogonids, and one to the Arthrocephala, a new class of which *Protadelaidea* is the type. From the Arthrocephala all the other types of arthropod have been developed, but they are not directly descended from *Protadelaidea*.

The book is profusely illustrated and contains an exhaustive bibliography, but there is no index.

THE MAMMALIAN FAUNA OF THE WHITE RIVER OLIGOCENE. *Part I. Insectivora and Carnivora. Transactions of the American Philosophical Society held at Philadelphia for Promoting Useful Knowledge. New Series, Vol. 28, Part I.*

By William B. Scott and Glenn L. Jepsen. American Philosophical Society, Philadelphia. Distributed by University of Pennsylvania Press, Philadelphia. $2.50. $11\frac{3}{4}$ x 9; 153 + 22 plates; 1936 (paper).

The White River Formation is composed of beds which are located principally in Nebraska and South Dakota. These strata were laid down by rivers in the Oligocene epoch and are unusually fossiliferous. A part of them is known as the "Bad Lands."

The authors have studied the fauna of this region using the specimens in various museums. Except for tortoises, by far the larger part of the fossils were of mammals which were present in great numbers and diversity. The authors feel that too many species have been named and consequently concentrate on generic distinctions. Among the Insectivora the Lepictidae is the most common family and *Ictops* the most abundant genus. The Carnivora include the creodonts, which while now extinct, are as well represented among these fossils as the Fissipedia. Among the latter the Mustelidae are rare and show little diversification, while the Canidae and Felidae are

more numerous. The text is illustrated by excellent plates.

A STUDY OF THE FOSSIL HORSE REMAINS FROM THE UPPER PLIOCENE OF IDAHO. *Proceedings of the United States National Museum, Vol. 83, No. 2985.*

By C. Lewis Gazin. Smithsonian Institution, U. S. National Museum, Washington. Free. 9½ x 6; 40 + 11 plates; 1936 (paper).

A detailed description of the skeleton and teeth of *Plesippus shoshonensis* from a site near Hagerman, Idaho, with measurements of 7 individuals and 3 related forms, viz., skull 18, teeth 17, vertebrae 6, forelimb 30, hindlimb 30. Associated fauna are listed. *Plesippus* apparently bridges a gap between earlier Pliocene horses and Quaternary *Equus*.

GENETICS

HUMAN GENETICS AND ITS SOCIAL IMPORT.

By S. J. Holmes. McGraw-Hill Book Co., New York. $3.50. 9 x 6; viii + 414; 1936.

The first eight chapters concern general genetics and biometry—perhaps more briefly than might be desired, yet lucidly. The inheritance of certain more important human physical and mental traits is treated in six more chapters. While a mass of excellent material is presented here, certain topics are treated rather briefly (as blindness and deafness), others are omitted (blood groups, taste reactions), and in the highly controversial field of "mental heredity" scant consideration is paid to the rôle of environment.

The final eleven chapters are devoted to various topics of general human biology, such as mate selection, past and present trends in population, national origins, war, cross-breeding, selective mortality, and proposals for practical eugenics. Criticism may be made of his treatment of human "instincts" (pp. 274–277), the hereditary bases of which are extremely doubtful; the theory that replacement of primitive peoples by whites has been due to natural selection (p. 285) which implies superiority of white hereditary traits, when their superiority in cultural traits such as military technique has been so obvious; usage of the word "race" when either culture or nationality is implied.

This book should be a valuable aid in college teaching, with its generous index and bibliography (containing few references to environmentalist works, however) and a series of questions at the end of each chapter.

DIE VERERBUNG INNERER KRANKHEITEN.

By Wilhelm Weitz. Ferdinand Enke, Stuttgart. RM. 14 (paper); RM. 14.60 (cloth). 9¾ x 6¼; xi + 197; 1936.

This book presents a summary of some of the findings regarding inheritance of disease. From the extensive list of clinical conditions here discussed, it seems that there are only a few known for which someone, sometime has not found an unusual or apparently unusual occurrence in single families. The author is particularly interested in the different incidence of diseases in identical and fraternal twins and here records most of the available data. While he does not actually admit that he believes in the inheritance of predisposition to all the diseases mentioned, yet the book would have greater value if he had selected the actual from the imagined evidence of presumption of heredity. The student will find this a good source of references. There is an index.

MENSCHLICHE ERBLEHRE. *Band I. Vierte, neubearbeitete Auflage.*

By Erwin Baur, Eugen Fischer and Fritz Lenz. J. F. Lehmanns Verlag, München. M. 15 (paper); M. 17 (bound) in Germany. M. 11.75 (paper); M. 12.75 (bound) outside of Germany. 8⅞ x 6; viii + 796; 1936.

The fourth edition of this classic on human genetics follows closely in form and substance the previous edition (see Q. R. B., Vol. 3, No. 1, 1928). All of the sections have been augmented by material derived from recent discoveries in the field of genetics and the section on Somatic Inheritance by E. Fischer has been completely revised and rewritten. One

addition, not too welcome, is the vague and inconclusive discussion by F. Lenz on racial psychology and racial mental characteristics.

GENERAL BIOLOGY

General Biology.
By James W. Mavor. Macmillan Co. New York. $4.00. $8\frac{1}{2} \times 5\frac{1}{2}$; xxiii + 729; 1936.

A Laboratory Manual in General Biology.
By James W. Mavor and Leonard B. Clark. Macmillan Co., New York. $1.75. $11 \times 8\frac{1}{4}$; v + 201; 1936 (paper).

Laboratory Directions in College Biology.
By W. H. Wellhouse and G. O. Hendrickson. F. S. Crofts and Co., New York. 85 cents. $10\frac{3}{4} \times 8$; 82 + 10 charts; 1936.

In writing *General Biology*, Doctor Mavor has aimed at stating

> ... simply and clearly the main facts and principles on which a sound and teachable course in biology can be based. Such an aim requires that the balance between the botanical and zoölogical portions be such as to provide for a real grounding in each of these. The order in which the material is presented has been chosen both with regard to the logical development of the subject and to a close correlation between laboratory and class room work.

The book is divided into five parts: (1), Nature of Life; (2), Plant Life; (3), Invertebrates; (4), Vertebrates, (5), Principles. These discussions are entirely conventional in character. The volume has the virtue of clarity, however, and should prove understandable even to the college freshman. Each chapter is followed by a group of "thought-provoking" questions although no specific references to pertinent literature are included outside of the generalized list mentioned in the preface. The illustrations are not particularly distinctive and many of them are copied. There is an index. This book can be recommended, along with many others, as an adequate introduction to the fundamentals of general biology. It is, of necessity, abbreviated and leaves much unsaid.

A Laboratory Manual in General Biology has been designed for collateral use with the text reviewed above, with which it is closely correlated. The manual is novel in that it is a combination notebook and instruction syllabus all rolled into one. Certain aspects of the volume are praiseworthy—it covers a wealth of intelligently chosen material, is clearly written and well organized. On the other hand, the reviewer was not favorably impressed with its underlying pedagogy which frequently requires that the student be presented with already prepared drawings and requested merely to label them adequately. This, it would seem, discourages original observation and leaves the laboratory worker with the impression, already far too prevalent, that an organism really looks like a textbook diagram. However, the manual has its points and is to be decidedly advocated if its mother text is adopted.

In contrast to the preceding guide, *Laboratory Directions in College Biology* is composed simply of a series of instructions that cover 33 different topics of general biological interest and make suggestions for their study. The book is less didactic than that by Mavor and Clark. This has an advantage in that it permits the student to follow more readily his own designs and may, God willing, stimulate him to a modicum of original thinking. It has a disadvantage in that it does not leave the user with as detailed an appreciation of the field. A commendable aspect of the Wellhouse book lies in its inclusion of several elementary exercises calling for field work and the observation of organisms in Nature—an aspect of biology usually denied the young undergraduate. One of these studies, entitled "Leadership," is concerned with the flock and herd organization of certain common vertebrates and represents a new and entertaining departure from the staid contents of traditional freshman biology.

Green Laurels. *The Lives and Achievements of the Great Naturalists.*
By Donald C. Peattie. Simon and Schuster, New York. $3.75. $9\frac{1}{4} \times 6$; xxiii + 368 + 20 portraits + 11 illustrations; 1936.

Naturalists are interesting folk. They follow their dictates with a candid fervor

and spirit that sets them apart from the run-of-mine individual who considers it smart, if he considers it at all, to be blasé about his interests. The best way to appreciate the point of view of the Nature scholar is to be one. The next best way is to read this delightful book which captures, in no uncertain terms, the philosophy behind some of the world's great and near-great naturalists. The author says in the Foreword:

I am writing about the naturalists, distinguished—as well as they can be—from the biologists. These latter I think of as the indoor men, the naturalists as the outdoor men. To put it another way, the naturalists deal with living beings *in situ*—in their active, vital inter-relations; the biologists are more concerned with isolated organisms, living under controlled laboratory conditions . . . I am human enough, moreover, to dwell upon the more piquant personalities. We love, alas, not so much for virtue as for charm.

Throughout its fifteen chapters the contributions and personalities of many men are discussed in an entertaining yet realistic fashion. The author emphasizes his subjects' accomplishments only as they reveal naturalistic tendencies. Such old standbys as Buffon, Réamur, Leeuwenhoek, Linnaeus, Lamarck, Cuvier and Darwin are given, as is their just due, a notable place in the book. In our opinion, however, the volume reaches its greatest heights in its treatment of some of the lesser lights—Bartram, Michaux, Wilson, Audubon, Say, Rafinesque—who roamed through the early American wilderness and did much to establish native biology in this country.

It is patent that this is a book destined for wide reading. Its lack of technicalities along with a general human interest fit it for the intelligent public. On the other hand, biologists will welcome it for its realistic appraisal of men important in the development of their science. An index, selected list of references and numerous illustrations supplement the text.

LIFE OF THE SHORE AND SHALLOW SEA.

By Douglas P. Wilson. Ivor Nicholson and Watson, London. 12s. 6d. $9\frac{1}{4}$ x $7\frac{1}{2}$; 150 + 129 figures; 1935.

This is the best book of its type that this reviewer has ever come across. It constitutes an introductory treatise on elementary oceanography, and while written principally from the standpoint of biology it covers such diverse matters as the tides, the continental shelf, the geological activity of the waves, variation in chemical content of the sea water, etc. Instead of being ordered taxonomically the biological material is arranged ecologically, which makes easier reading for the beginner enabling him to meet the inhabitants of the ocean in their own homes instead of on the shelves of a museum. There is also a very interesting chapter dealing with invertebrate embryology, which is helpful because so many of the ocean's inhabitants have larval planktonic stages totally unlike the adults.

The book has a comprehensive index, and also many illustrations from diverse sources, chief among them being the author's camera. It is an excellent model of what a beginner's book ought to be, whether it be judged from the standpoint of science, literature, or art. Unfortunately for those who live on this side of the Atlantic, only the British shores are treated in this volume. We should invited Mr. Wilson to take up a temporary residence in this country in order that he might produce a companion work dealing with the coastal waters of America.

ENGLAND HAVE MY BONES.

By T. H. White. Macmillan Co., New York. $2.50. 8 x $5\frac{1}{2}$; vii + 334; 1936.

This book, the journal of an Englishman, is "a book about the tangible side of country life." For the most part it consists of the author's impressions of and reactions to his experiences with such pastimes as fishing, hunting, and learning to fly. These he has recorded in an unusually vivid style. The entries were made with an eye to publication; therefore, along with the happenings of the day, Mr. White has included bits of legend, some rather sketchy natural history, his experience with some pet snakes, and a varied assortment of other incidents and facts.

The book is in no sense technical and

will prove interesting chiefly to those people who have had similar experiences, and especially to those who have a passion for fishing, or for flying, or, in general, a sportsman's love of the country. It is not a record of a specific period with a beginning and an end. It is, instead, a record of a typical year of the author's life. The primary purpose is not merely to present his experiences, but, rather, to tell about things observed with "seeing eyes" for the benefit of "people who have lost them." All things considered, it is an interesting though not a profound book. No bibliography or index.

EXPERIMENTAL MODIFICATIONS OF THE EFFECT OF ULTRAVIOLET LIGHT ON PROTOPLASM. I. AMOEBA PROTEUS. *University of California Publications in Zoology, Vol. 41, No. 8.*

By W. A. Black. University of California Press, Berkeley. 35 cents. $10\frac{1}{4} \times 6\frac{3}{4}$; 28; 1936 (paper).

In this paper the effect of ultra-violet radiation on amoebas immersed in various salt solutions is reported. The investigation was stimulated by the studies of earlier workers who claimed that a change in the ionic environment of an amoeba so modified the internal relationships that any effects of irradiation were fundamentally altered as a result. To get at this question the author used light of a wavelength equal to or less than 2540Å and irradiated amoebas in various dilute solutions (chlorides of Na, K, Mg, and K). The general conclusions reached were (1) that the protoplasmic changes resulting from this treatment were typical of injury and not characteristic for irradiation; and (2) that the results did not support the earlier contention mentioned above.

METHODS OF TISSUE CULTURE IN VITRO. Containing following article: *Outlines of Histological Methods with Special Reference to Tissue Culture,* by *Clayton G. Loosli,* 55–81.

By Ralph Buchsbaum. University of Chicago Press, Chicago. $1.00. 9 x 6; [8] + 81; 1936 (paper).

The first part of this book is designed to present

. . . a simplified method of tissue culture for a single investigator in an ordinary biological laboratory. All of the requirements for best growth have been included, but the attempt has been made to eliminate procedures of doubtful value, . . . and to recommend equipment which is as inexpensive as possible.

This section is clearly written and should be of great assistance to the biologist who wishes or must learn the tissue culture technique and equip his laboratory without personal assistance. The second part of the book discusses in an elementary yet competent manner the essentials of ordinary histological methods. Taken all in all the volume is a useful addition to technical biology and should find many friends among laboratory workers. The text material is supplemented by a bibliography of selected references to guide the reader into deeper aspects of the subject if desired.

THE MICROSCOPE. *Sixteenth Edition Revised and Enlarged by the Addition of a Chapter on Micro-Incineration.*

By Simon H. Gage. Comstock Publishing Co., Ithaca. $4.00. 9 x 6; viii + 617; 1936.

This widely-used and standard textbook (previously noticed in Q. R. B., Vol. 7, No. 3, p. 348) has been revised in its sixteenth edition by adding a section on the method of reflecting ultra-violet radiation by mirrors coated with aluminum, and a chapter on micro-incineration. The latter discussion aims at ". . . giving definite information concerning the presence, amount and location of the fixed mineral salts in the tissues and cells of the body." These additions to the present volume bring the subject matter up to date and augur well for the continued popularity of the book among biologists and medical men.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 452. Methoden der Süsswasserbiologie.* Containing following articles: *Die Massenzucht von nannoplanktischen Grünalgen als Futter für Wassertiere,*

by Einar Naumann; *Neue Einrichtungen und Arbeitsmethoden im limnologischen Laboratoriumsbetrieb*, by Einar Naumann; *Technik der Unterwasserbohrung auf Bohrfähren*, by Erich Wasmund; *Quantitative Methoden zur Untersuchung des Nannoplanktons*, by Hans Utermöhl.

Urban und Schwarzenberg, Berlin. RM. 13.50. 10 x 7; 213; 1936 (paper).

The scope of the limnological material included in this number of *Abderhalden's Handbook* is sufficiently indicated by the subtitles for the different parts.

HUMAN BIOLOGY

MIGRATION AND ECONOMIC OPPORTUNITY. *The Report of the Study of Population Redistribution.*

By Carter Goodrich, Bushrod W. Allin, C. Warren Thornthwaite, Hermann K. Brunck, Frederick G. Tryon, Daniel B. Creamer, Rupert B. Vance, Marion Hayes and others. University of Pennsylvania Press, Philadelphia. $5.00. 10 x 6¾; xvii + 763 + 11 plates; 1936.

This book presents the results of a study undertaken "to consider what movements of population within the United States might be necessary and desirable and what part, if any, the government should take in encouraging them." The book is divided into two parts. The first examines the conditions of those regions where the standard of living (measured by the number of radios and telephones and the income tax return) is below the average of the country as a whole. These are the southern Appalachian coal plateaus, the old cotton belt, the cutover region of the Great Lakes states and the great plains. In this part are also included chapters on the trends of labour demand in the several branches of occupation. The second part of this volume contains a discussion of migration in this country, the attempts in England, Germany and Russia to promote migration to more-favoured regions and the similar measures enacted in this country during the present administration. The authors have apparently gone to great pains to derive from the meagre data available an adaquate and objective analysis of the conditions.

The findings are not surprising and may be summarized as follows: (1) the above mentioned regions and the prevalently agricultural, have suffered the most from the present crisis and show the greatest natural increase in population; (2) the chances of future increase in employment are greater in urban communities and for industries than they are in the rural region and for occupations such as mining and agriculture; (3) centralizing industries show no intention of decentralizing nor to move to the above named regions. Therefore, the authors argue, since there is so little hope that the people of these regions can obtain the means of relieving their distress, let us move the superfluous individuals to other and more favored regions. Just how this is to take place and what will be the consequences, the authors do not state either clearly or definitely, although they note that in other countries government interference has been practically ineffective, and so far the various New Deal schemes likewise. Without doubt this book is an important contribution to the knowledge of present economic conditions but, as the authors note, much more information is necessary before any definite plan regarding migration can be made. Included in the book are numerous useful tables and maps.

THE MESOLITHIC SETTLEMENT OF NORTHERN EUROPE. *A Study of the Food-Gathering Peoples of Northern Europe during the Early Post-Glacial Period.*

By J. G. D. Clark. The University Press, Cambridge; Macmillan Company, New York. $9.00. 10 x 7½; xvi + 284 + map in pocket; 1936.

Archaeologists and all those interested in pre-history are deeply indebted to the author for this very excellent treatise. One cannot praise too highly the care and thoroughness that has gone into the preparation of the volume, not only in the textual matter but also in the numerous illustrations (photographic plates, drawings, charts, and maps) and in the excellent bibliography. While it has long been recognized that physical environment greatly influenced the development

of primitive cultures, it has been less generally understood that in the area under consideration changes in environment in the last few thousand years have been "so profound as to alter its influence on cultural development and so rapid as to afford a natural time-scale for the dating and synchronizing of human culture." The region, bounded on the west by the mountain backbone of Britain, on the south by the highlands of Southern Germany, on the north by the mountains of Scandinavia, and open to the east by the great plains of Russia and Siberia, presents evidence that during the early post-glacial period there were "three groups of cultures, three traditions, distinguished by differences of origin and by adaptation to differing types of environment." Food-gathering formed the economic basis of all these cultures.

Of the many interesting phases relating to the Mesolithic peoples that are presented in this book we call attention to only one: namely, the microscopic analysis of ancient pollen grains. Such analysis gives very precise information concerning the general drifts of forest development. In a series of seven appendices additional data are presented in condensed form. The volume is well indexed and in a folder inside the back cover is an enlarged map of the Maglemose culture.

HEADS AND TALES.

By Malvina Hoffman. Charles Scribner's Sons, New York. $5.00. 9½ x 6¾; xx + 416; 1936.

Malvina Hoffman's tale of the execution and installation of the type portrait statues that constitute the Hall of Man of the Field Museum in Chicago makes a most interesting and diverting book. It is a *melanges* of autobiography, travel, gossip, philosophy, ethnology, anthropology, art, and what else have or want you. Its success in the best seller class is well deserved. We recommend it most heartily.

The book illustrates in a remarkable way that biologically curious attribute of man—the *general* competence of the professional. The competence that the professional in any field, art, science, or literature, acquires *by training*, is necessarily in the nature of the case a highly specialized competence. Yet it has been repeatedly observed that when a really competent professional is suddenly called upon to do a job of work in a field remote from that for which he is specially trained, he turns out a perfect performance. He exhibits, in short, *general* competence. Miss Hoffman was by training (under Rodin among others) a sculptor, highly successful in a distinctly specialized department of that art. Without any implied derogation it is reasonably exact to say that when she took on this Field Museum job she knew but little more about physical anthropology than the proverbial catfish, and certainly no more than the average art student, which isn't much. Nevertheless in the end she finished a first rate piece of *anthropological* work, over and beyond its artistic merits.

HANDBOOK FOR RECREATION LEADERS. *U. S. Department of Labor, Children's Bureau, Publication No. 231.*

By Ella Gardner. U. S. Government Printing Office, Washington. 15 cents. 9⅛ x 6; vii + 121; 1936 (paper).

In this well indexed pamphlet we are told how to teach children, and adults as well, to play. First, the discussion is of community parties and picnics and reeks with helpful suggestions about stimulating the shy and the bored into the general spirit of fun. The bulk of the pamphlet embodies descriptions and rules of over two hundred games ranging from riotous out-of-door sports to singing games and tests of mental skill. The last part consists of poems and songs with suggestions for dramatizing them. The gay and abandoned artistry with which our great humanitarian government shows us how to have fun is charmingly demonstrated in the illustration on facing page, which faces page 78 in the original.

MEASURES OF MEN. *Ten Specialized Studies in Physical Anthropology in Mexico, Central America and the West Indies. Middle American Research Publication No. 7.*

By Harold Cummins, Mary S. Lane, Stella M. Leche, Ruth Millar, Inez D. Steggerda and Morris Steggerda. Department of Middle American Research, Tulane University, New Orleans. \$5.00. $10\frac{3}{4}$ x $7\frac{1}{2}$; 331; 1936 (paper).

Steggerda gives a brief resumé of the work of three investigators, including himself, on a total of 1050 Mayan Indians of Yucatan. He and Millar compare finger and hand dimensions among Mayans, Negroes and whites. Together with Cummins, the finger prints of 127 Mayans are analyzed in detail, showing a lower frequency of whorls than heretofore found among North American aborigines. The Steggerdas and Lane analyse in detail the palm prints of 224 Mayans, comparing them generously with other series. They resemble most closely the Chinese.

Professor Cummins describes fully the controversy over Valsik's scheme of adding the four main line formulas of the palm to make an index—the "papillar number." Americans have considered this methodology invalid. The author suggests adding the values of lines A and D alone. He presents also a retabulation of H. H. Wilder's collection of palm prints, to the terms of the revised methods.

Four papers by Leche present dermatoglyphic data on five series as follows: 78 Aztecas, 78 Mixtecas, 50 Zapotecs, 100 Chamulas, 116 Tarascans. All were selected to minimise white ancestry. All but the last series include two determinations of functional lateral dominance—eyedness and handedness which Dr. Leche is correlating with asymmetry in palm and finger patterns with promising results. Three series include 22–28 physical measurements, taken after instruction from Dr. E. A. Hooton.

Rasse und Heimat der Indogermanen. *By Otto Reche. J. F. Lehmanns, Munich.* 8 Marks (bound); 6.50 Marks (paper). $9\frac{3}{4}$ x $6\frac{1}{2}$; 216; 1936.

The author, professor of anthropology and ethnology in the University of Leipzig, presents in this book a careful and critical investigation, carried on for a period of thirty years, of the much discussed problem concerning the race and original home of the Indo-Germanic (Indo-European) peoples. Believing that comparative philology, archaeology and prehistorical research may indeed contribute much to the study of the subject but that the problem can finally and decisively be solved only by the science of anthropology, he investigates first of all

the racial composition of the various Indo-European peoples both in Asia and Europe, reaching the conclusion that they are fundamentally Nordics. A similar examination of the remains of the population of the most important neolithic cultures of Europe shows these likewise to have been preëminently men of the Nordic race. Proceeding then to a search for the home and origin of this Nordic race he finds that the special morphological, physiological and psychological racial characteristics all point to western, northwestern and central Europe, and not to eastern Europe or western Asia (Siberia, v. Eickstedt), as its home and hence also as the home of the original Indo-Europeans—and to the latest glacial (Würm) period, the late Mousterian or Aurignacian as the time of its origin. The Nordic race is a product of selection (*Züchtungsprodukt*) of the latest European glacial period (ca. 100,000–80,000 B.C.).

[At this point Reginald the Office Boy suggested that if we wanted to save space, as we always do, why didn't we in place of the above paragraph just say *Heil Hitler*, and inform the reader that the book is provided with 113 illustrations, 5 maps, an index of authors and a subject index.]

GROUP SETTLEMENT: ETHNIC COMMUNITIES IN WESTERN CANADA. *Volume VII. Canadian Frontiers of Settlement.*

By C. A. Dawson. Edited by W. A. Mackintosh and W. L. G. Joerg. The Macmillan Co. of Canada, Toronto. $4.50. 10 x 6¾; xx + 395; 1936.

This volume deals with five group settlements (Doukhobors, Mennonites, Mormons, German Catholics and French-Canadians) made in the Canadian west where their numbers comprised from half to all of the population. These ethnic groups, forming "cultural islands" were each bound together by language, sectarianism, nationalism and collectivism in various combinations which they were determined, almost fanatically, to maintain. The author, professor of sociology at McGill University, and his assistants have produced an interesting study of the social organization of these groups, and of the influences which in the past were (and still are) at work either in preserving or breaking down their permanency. Statistically the colonies have been more than ordinarily successful. There are minor exceptions, especially in those regions where soil and climatic conditions have been adverse. Formal institutions developed quickly in these homogeneous groups and the pioneers early acquired a sense of security. On the other hand, in recent years, with the rapid development of western Canada, "bloc communities" have been a source of much friction with the government. According to the author "it seems clear from the evidence analysed that the unplanned play of external forces in the long run tends to eliminate much, perhaps all, of the distinctiveness of separatist colonies." The volume contains numerous illustrations and a large amount of tabular material. It is well indexed.

THE SCIENCE OF SOCIAL DEVELOPMENT. *A Study in Anthropology.*

By F. A. Brooke. Watts and Co., London. 12s. 6d. 8½ x 5½; xiv + 337 + 5 plates; 1936.

This book is a summary, in a rather detailed outline form, of the progress of civilization. It is divided into two parts. The first and larger is concerned with the probable origin and development of the customs, manners, and institutions of man which comprise his culture. The second part shows how the generalities set forth in the first part can be applied specifically. The author has chosen for this illustration the countries of the British Isles, with emphasis upon the development of society in England.

Mr. Brooke has not attempted to present original theories. His material has been drawn from many sources which are acknowledged in abundant footnotes. What he has achieved, however, is a bird's-eye view of man's social development. He does not assume that "the last word has been said in this study of human progress"; but he does maintain that "on the whole it is an accurate account of the institutions and systems that came into being and were developed during the long and arduous progress of human society."

The material is well organized and is presented simply and clearly; but the outline form which contributes clarity detracts somewhat from the interest as it imparts a decidedly pedagogical flavor. This is at cross-purposes with the author's original intention since the book was written for the layman in anthropology. The book contains an index and a rather extensive bibliography.

ASPETTI DEMOGRAFICI DEI GRUPPI CONFESSIONALI IN UNGHERIA. *Con Particolare Riguardo agli Ebrei.*

By Stefano Somogyi. Instituto per l'Europa Orientale, Rome. 8¾ x 6; 238 + 1 folding chart; 1936 (paper).

The author compares certain demographic characteristics of the Hungarian Jews with those of the remaining Hungarian population. He finds that, as a group, the Jews marry at an older age, exhibit lower fertility, lower infant mortality and higher masculinity than do the Hungarians belonging to other religious denominations. Are these characteristics due to race and religion? The author thinks not, but believes that they are a consequence of the fact that the economic status of the Hungarian Jews is higher than that of the general population. In addition, he points out that historical evidence contradicts the widespread belief in the racial purity of the Jews; in Hungary, at least, they are as much an ethnic mixture as the remaining peoples. Therefore, he concludes, the Jews should not be considered racially different from the rest of the population, but rather as a religious denomination which is an integral and important constituent of the Hungarian nation.

By analogy to other studies on population it can be assumed that the author is essentially correct in his interpretation of the cause of the demographic differences between Jews and non-Jews. However, more precise and detailed evidence is necessary. There is an adequate bibliography.

CONTROL IN HUMAN SOCIETIES.

By Jerome Dowd. D. Appleton-Century Co., New York. $3.00. 8¾ x 5¾; xvii + 475; 1936.

This book, primarily to be used as a college text, is divided into four parts. Part I concerning the origin of control, explains that folkways and public opinion are the most effective means of social control when society is in the genetic stage. As society passes later into the telic or conscious stage the leaders realize that organization forms a more efficient control, guided in part by the other two forces. Part II surveys the history of social control which may be roughly divided into three eras: paternal control, the rebellion against paternal control, and the rise of democratic or social control. Part III is an analysis of the problems of social control in play and recreation, education, industry, marriage and family, religion, control in the state, and in the arts. The last section is devoted to a discussion of principles of control applied to the present chaos in the Western world. The reactions of the Western world to the World War and the following depressions are outlined and the author optimistically feels that out of the chaos and many rapid changes that have taken place there will arise some era of peace and stability with a socialized form of government that will be useful and helpful to mankind.

There is an index and a guide to the study of social control (24 pages) which gives reading references for each chapter, and a number of questions. The book should be a valuable reference and textbook.

LIVING HIGH. *Or at Home in the Far Andes.*

By Alicia O'R. Overbeck. Lovat Dickson and Thompson, London. 10s. 6d. 8½ x 5½; xii + 307 + 14 illustrations; 1936.

This is a grand story, told by the Baltimore-born wife of an American mining engineer who took a job with a company operating high, high up in Bolivia—so high that everybody's physiology had to be readjusted to a deficiency of oxygen if they were to go on living at all. They stayed there six years—and being the kind of persons they were, found these "good" years. A better account of the powers of

physical, physiological, and psychological adaptiveness inherent in the human organism would be hard to find than is embodied in this delightfully written account. Children were born and grew in this harsh environment and loved it. The book ends with "no complaints to make, no grievances to air."

> But in Bolivia we left behind us that which we can never regain—that shining part of life when nothing is dull or drab, that part of life when you can dance all night and ride all day without weariness, when long muleback trips over cruel hard trails, when biting cold, when breath-taking altitude, when stark isolation are adventures, wonderful as a fairy tale. To this brightness we must all say *adiós* sooner or later; so let us say it cheerfully and in grateful spirit for having been allowed to live it to the full.

WORKERS' NUTRITION AND SOCIAL POLICY. *International Labour Office. Studies and Reports Series B (Social and Economic Conditions) No. 23.*

International Labour Office. League of Nations, Geneva; World Peace Foundation, 8 West 40th St., New York. $1.50. 9½ x 6¼; vii + 249; 1936 (paper).

At the nineteenth session of the International Labour Conference in June, 1935, the following propositions were laid down: (1) that adequate nutrition is a basic factor in the health and well-being of the workers and their families; (2) that there is widespread evidence that larger numbers of workers in town and country are not sufficiently or suitably nourished, even in industrially advanced countries; and (3) that a proper consideration of workers' nutrition would help to solve some of the difficult social-economic problems of today. The general scope and character of this report is based upon these propositions. It is international in character and gives many data concerning the consumption of foods in different countries (by income groups), analyses of nutritive properties of workers' diets, production of foods, prices, market organization, family incomes, etc. A section is included on the co-operative movement. Very nearly one-third of the report is taken up with a series of appendices in which additional data are arrayed in tabular form.

THE ISNEG LIFE CYCLE. I. BIRTH, EDUCATION, AND DAILY ROUTINE. *Publications of the Catholic Anthropological Conference, Vol. 3, No. 2.*

By Morice Vanoverbergh. Catholic Anthropological Conference, Washington. $1.75. 10 x 7; 81–186; 1936 (paper).

This paper is one of a series that the Catholic missionary has published on the Isneg, a head-hunting tribe inhabiting one of the subprovinces of Northern Luzon, P. I. The whole life of the Isneg, from cradle to grave is dominated by fear of spirits. Father Vanoverbergh, in the course of his daily routine, has had exceptional opportunities for observing the effect which superstitution, magic, and spirit worship have had upon their behavior. Both pagan and Christian Isneg seek some explanation different from the natural one for any event that does not look ordinary to them. The birth of twins in a family is a dilemma. Two children at once must be the product of two men—even if the father knows his wife to be a virtuous woman. The problem, however, is settled at once and definitely by one of the infants being killed. One of the most interesting sections of the paper is that on the moral code of the Isneg when the writer assumes the garb of an Isneg moralist "merely to give a better perspective to the whole treatment of this very important subject."

THE STUDY OF MAN. *An Introduction. Student's Edition.*

By Ralph Linton. D. Appleton-Century Co., New York. $3.00. 8¾ x 5¾; x + 503; 1936.

As a professor of anthropology, the author of this volume felt that there was no one text-book "broad enough in its scope to provide beginners with a grounding in the essentials of Anthropology." He has written a book, therefore, to overcome this difficulty. The book is really a survey of the field of anthropology. Man, his origin, his racial differences, his culture, his society, are all considered, but in a broad, general manner. Furthermore, the author has succeeded in showing the relationships be-

tween these various aspects. Realizing that anthropology is still a young science, he has attempted to present facts and theories in an unbiased discussion and to avoid extravagant claims. In fact, it is so unbiased that it tends frequently to raise questions rather than to reach conclusions.

Although this book was designed primarily as a text for beginning students in anthropology, it may also serve the layman as an introduction into the field of sociology. It is not technical and is written in an interesting readable style. Both bibliography and index are included.

YALE UNIVERSITY PUBLICATIONS IN ANTHROPOLOGY. *Numbers One to Seven* and *Eight to Thirteen.*

Edward Sapir and Leslie Spier, Editors. Yale University Press, New Haven. Numbers One to Seven \$2.00. 9¼ x 7; 143 + 1 plate. Numbers Eight to Thirteen \$2.50. 9¼ x 7; 163. 1936 (paper).

Cultural anthropologists may well congratulate themselves upon the commencement of the Yale University Publications in Anthropology, under the able editorship of Edward Sapir and Leslie Spier. In this notice it will be possible only to mention the titles and authors: Population Changes among the Northern Plains Indians, by Clark Wissler; Regional Diversity in the Elaboration of Sorcery in Polynesia, by Peter H. Buck; Cultural Relations of the Gila River and Lower Colorado Tribes, by Leslie Spier; Hopi Hunting and Hunting Ritual, by Ernest Beaglehole; Navaho Warfare, by W. W. Hill; The Economy of a Modern Teton Dakota Community, by H. Scudder Mekeel; The Distribution of the Northern Athapaskan Indians, by Cornelius Osgood; Profane Literature of Buin, Solomon Islands, by Richard C. Thurnwald; An Outline of Seneca Ceremonies at Coldspring Longhouse, by William N. Fenton; The Shawnee Female Deity, by C. F. Voegelin; Human Wolves among the Navaho, by William Morgan; Musical Areas in Aboriginal North America, by Helen H. Roberts; Rank and Potlatch among the Haida, by George Peter Murdock.

ENCHANTING WILDERNESS. *Adventures in Darkest South America.*

By Hans Tolten. Translated from the German by Ferdi Loesch. Selwyn and Blount, London. 15s. 8½ x 5½; 285 + 23 illustrations; 1936.

An interesting, if in some respects rather unpleasing account of the struggles of a young German to establish himself as a planter in the northern territories of Argentina some dozen years or so ago, and after failing in that enterprise, of his efforts to eke out a living in any way. It is full of adventure, sometimes a bit gruesome, and gives a fairly clear and definite picture of the country and its indigenous native inhabitants. On the whole perhaps the most striking thing that comes out of it is the extraordinary parallelism between the way in which the whites there, intent upon exploiting the country, are treating the Indians, and the way in which the whites of North America treated the native Indian population here a century or so earlier. The sensitive reader finishes the book with a feeling of nausea and degradation. And smug folk still prate of the "dignity of man." A considerable section of the book deals with egret hunting—again a most unpleasant topic. The thoughtful biologist and student of population will find much in the book to ruminate over.

BEVÖLKERUNGSFRAGEN. *Bericht des Internationalen Kongresses für Bevölkerungswissenschaft Berlin, 26 August—1 September 1935.*

Edited by Hans Harmsen and Franz Lohse. J. F. Lehmann, Munich. 22 marks; 25 percent discount outside of Germany. 9¼ x 6¼; xxvi + 972; 1936.

This stout volume containing some 125 papers presented before the somewhat nationalistic International Congress on Population sponsored by the Germans in 1935, is divided into two parts. In addition to the seven opening addresses Part 1 contains papers on the following sub-

jects: the international significance of the declining birth rate; internal migration and urbanization; change relationships between population and administrative development; marriage and the family; theories of population movements; methods of demographic statistics. Part 2 treats: anthropological and racial problems; differential fertility; race hygiene, heredity and eugenics; state demographic problems; and public health problems.

The individual papers are published in the language in which they were presented but are provided with abstracts in two or more other languages (English, French, German, Italian or Spanish). The volume is supplied with a list of the names and addresses of the participants, and author and subject indices.

YEAR-BOOK OF LABOUR STATISTICS 1935–36. *International Labour Office, Geneva.* $1.50 (paper); $2.50 (cloth). (U. S. Agents: World Peace Foundation, 40 Mt. Vernon St., Boston). 9½ x 6¼; viii + 227; 1936.

The contents of this volume were formerly included in the International Labour Office Year-book. As in previous years it presents the official statistics on employment and unemployment, hours of labor, wage scale, cost of living, workers' family budget, emigration and immigration and labor disputes for a number of countries and for the several groups of occupation. In the preface it is noted that since the sources of information, i.e. the statistical bureaus of the individual nations, have great diversity of scope in collecting the data, international comparisons are "extremely difficult and only possible with considerable reservations; fluctuations within the same country are generally comparable, . . ." This is one of the unfortunate aspects of this type of statistics and greatly limits the utility of such publications as this. There is an appendix giving the population of the different countries. The sources of information are listed.

LE DESTIN DES RACES BLANCHES. *Deuxième Édition.*

By Henri Decugis. Librairie de France, Paris. 42 francs. 9¾ x 6¼; x + 565; 1936 (paper).

The fact that a second edition of this book has been printed within a year of the first is sufficient indication of the interest that it has aroused. As it was pointed out in the review of the first edition (cf. Q. R. B. Vol. 11, No. 1, 1936), a book on social and economic trends based on facts and presented in a simple and straightforward manner is uncommon and therefore justly deserves whatever popularity it can achieve. In this edition, the statistical data have been brought to date and six new chapters have been included. These discuss the ethnic composition of the Western nations, the monetary crisis, trends in marriage and divorce, increasing subordination of individual rights, and changes in political ideals. The new chapters do not add materially to the quality of the book, but they do serve to emphasize that the author is one of the few remaining liberal thinkers.

TRAIL-BLAZERS OF SCIENCE. *Life Stories of Some Half-Forgotten Pioneers of Modern Research.*

By Martin Gumpert. Translated from the German by Edwin L. Shuman. Funk and Wagnalls Co., New York. $2.50. 8 x 5¾; viii + 306; 1936.

This series of biographies has been written to show "the great battle which mankind has waged within the short period of recorded history in a single domain—that of physical science." The author emphasizes the "ineradicable opposition to genius" which persecuted these great men from the sixteenth century almost to the present day.

The list includes Cardan, Vesalius the anatomist, Servetus who discovered the circulation of the blood through the lungs, Swammerdam, Wolff, Lamarck, Mayer, Jackson and Morton, von Pettenkofer and Harvey Cushing. In each sketch the biography of the individual and his achievements is accompanied by a discussion of the social, political, scientific and religious conditions of the day. Our only criticism is of the confusion that results when the author allows himself to digress too far along these lines.

CHINESE COOKERY BOOK.

By S. K. Cheng. The Shanghai Restaurant, London. 3s. 9d. $7\frac{1}{4} \times 4\frac{3}{4}$; viii + 102; 1936.

Either you like Chinese cooking or you don't. There is no half-way position. If you don't like it this review should not even be read since the reviewer is so prejudiced in its favor that he fears he cannot record here an unbiased judgment. If you do like it get this book for it gives many recipes and instructions for the home preparation of dishes such as those served by the Shanghai restaurant in London. All of these recipes make entertaining reading. One, among many others, which struck the fancy was that for *Tun Suin Arp Peen* calling for 6 oz. duck meat, 1 oz. bamboo shoots, 2 oz. onions, 1 oz. water chestnuts, $\frac{1}{2}$ oz. mushrooms, 1 oz. celery, 1 oz. Chinese pickled onions, sour sweet sauce, and $\frac{1}{4}$ oz. Wun Yee (lichen).

The book is to be recommended to interested parties with the sad but realistic reservation that the American housewife who can produce tasty Chinese dishes in her own kitchen is yet to be found.

APPRAISING PHYSICAL STATUS: THE SELECTION OF MEASUREMENTS. *University of Iowa Studies in Child Welfare, Vol. 12, No. 2.*

By Charles H. McCloy. University of Iowa, Iowa City. $1.00 (paper); $1.35 (cloth). $9\frac{1}{8} \times 6\frac{1}{8}$; 126; 1936.

This study is devoted to an analysis and evaluation of measurements used in appraising the physical status of the child. The author believes that in estimating the health of a child, it is just as important to appraise his physical features as it is to study his family history, or the results of a physician's examination and laboratory analysis.

There is a review of the principal measures available for determinations of weight, fat, limb girths and breathing capacity. In recommending procedures, the author keeps in mind the practical demands of the school or clinic situation. Thus the prediction of optimum weight is based upon simple measurements of height, weight, chest circumference, width of hips and width of knees. Standard landmarks, instruments and techniques of measurement are described and illustrated.

TRUANTS. *The Story of some who deserted Medicine yet triumphed. Based on the Linacre Lecture Delivered at Cambridge, 6 May, 1936.*

By Lord Moynihan. The University Press, Cambridge; Macmillan Co., New York. $1.40. $7\frac{1}{2} \times 4\frac{1}{4}$; 109; 1936.

Thomas Linacre, founder and first president of the Royal College of Physicians, was known as a scholar and philosopher as well as physician. It was as a corollory to this fact that Lord Moynihan in a commemorative address quite fittingly spoke of others who were trained in medicine but became "truant disciples to science, to literature, to law, to the State." In this book he accounts briefly for several of these versatile gentlemen, and there are probably but few people who will not find several surprises among these truants. Copernicus, Galileo, Robert Boyle, John Locke, Berzelius, Thomas Young, Linnaeus, Clemenceau, General Leonard Wood, Rabelais, Conan Doyle, Robert Bridges, Oliver Wendell Holmes are some of the better known names to be found.

NORTHEASTERN AND WESTERN YAVAPAI. *University of California Publications in American Archaeology and Ethnology, Volume 34, Number 4.*

By E. W. Gifford. University of California Press, Berkeley. $1.50. $10\frac{1}{4} \times 6\frac{3}{4}$; v + 108 + 1 folding map; 1936 (paper).

The Yavapai Indians prior to reservation days inhabited a large tract of land in central and western Arizona. Because of their wide geographical range they are considered to offer an unusual opportunity for a study of adjustments to various environments by a hunting and gathering people. Information relating to the subsistance, material culture, social organization and customs, religion and various characteristics of this tribe of Indians is given in the form of notes as acquired from a few informants (13 in number) most of whom had lived together with members of other tribes for many years on the San

Carlos Apache reservation. This source of information seems to the reviewer inadequate for an interpretation of the history of Yavapai culture.

WORLD POPULATION. *Past Growth and Present Trends.*

By A. M. Carr-Saunders. Oxford University Press, New York. $4.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; xv + 336 + 2 folding charts; 1936.

This work has been born of a desire to evaluate the world's population in 1650 and also at the present day, to classify the populations as to age, race and sex, to account for the changes observed to have taken place between the control dates, and to draw inferences from these conclusions upon which prognostications may be based. It contains a detailed examination of those factors which one would expect to determine changes in population, such as birth and death rates, war and migration, specific fertility and birth control. The logistic curve as a description of observed population growth is not discussed. The book is adequately indexed and lucidly written.

THE BANTU TRIBES OF SOUTH AFRICA. *Reproductions of Photographic Studies. Vol. IV, Section 11, Plates XLI–LXXX. The Vachopi of Portuguese East Africa. With an Introductory Article on the Vachopi; a Bibliography, and Descriptive Notes on the Plates by Henri-Philippe Junod.*

By A. M. Duggan-Cronin. Deighton, Bell and Co., Cambridge. 25 shillings. $11\frac{3}{4}$ x $8\frac{1}{4}$; 59 + 40 plates; 1936 (paper).

The Vachopi have a distinct and genuine language of their own. This and other cultural traits as well as certain physical characteristics indicate plainly that from an ethnological point of view they must be considered as a distinct Bantu people which may formerly have been much more numerous than at present. Today they number between 150,000 and 200,000 souls. This section of Volume IV on the Bantu tribes, containing a chapter on the language and culture of the Vachopi, a bibliography, and 27 very handsome reproductions of photographic studies of the Vachopi with explanatory notes, was made possible through the generosity of the Carnegie Corporation acting through the Research Grant Board of the Union of South Africa.

AFRICA'S GOD. *II. Dahomey. Anthropological Series of the Boston College Graduate School, Vol. I, No. 2.*

By Joseph J. Williams. Boston College Press, Chestnut Hill, Mass. $1.00. $9\frac{3}{4}$ x $6\frac{5}{8}$; 101; 1936 (paper).

The author finds that "while the tribal religion (in Dahomey) was basically monotheistic, the superimposed animism constantly distracted attention from the Supreme Being and tended, in ever increasing degree, to becloud the real relation of the intermediary creature towards the Creator, until eventually a phase of real idolatry seemingly asserted itself and the creature became the active recipient of the cult of 'latria' belonging only to God." The author is rather critical of the work done by Herskovits on Dahomean religious beliefs. There is a short bibliography, and an index.

THE ESKIMOS.

By Kaj Birket-Smith. Translated from the Danish by W. E. Calvert; translation revised by C. Daryll Forde. Foreward by Diamond Jenness. E. P. Dutton and Co., New York. $5.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; xiv + 250 + 33 plates; 1936.

The author, having lived among the Eskimos as one of them for nearly two years and visited northern Alaska and Greenland many times, seems to have both a thorough and a sympathetic knowledge of these people. The book should be quite fascinating for the general reader as well as being a valuable ethnological document. A summary of Eskimo tribal groups, rules for pronunciation of Eskimo words, and a long bibliography are to be found in the appendix.

YOUR FOOD SUPPLY.

By Allen H. Lester and Donald G. Ferguson. American Institute for Economic

Research, Cambridge. $1.00. $8\frac{1}{2} \times 5\frac{1}{2}$; 62; 1936 (paper).

Believing that during the next few months food prices will rise 15 to 50 per cent above present levels, the authors of this enlightening and helpful booklet devote the first and larger portion to a discussion of the reasons for price increases in each of the principal classes of foodstuffs. The second part is devoted to a discussion of budget protection, for housewives as well as for institutions. Some specific suggestions are made; but for more detailed information the authors list in an appendix a number of government publications "of assistance in buying, preparing, and storing food."

DATING PUEBLO BONITO AND OTHER RUINS OF THE SOUTHWEST. *Pueblo Bonito Series, Number 1.*

By A. E. Douglass. National Geographic Society, Washington. $1.00. $10 \times 6\frac{1}{2}$; 74; 1935 (paper).

This is an extremely interesting account of the dating of Indian ruins in the Southwestern U. S. From a comparison of rings found in the roof logs and beams, with the rings of other logs or trees of known age, an overlapping of ring sequences has made it possible to construct a calendar which not only dates quite precisely these prehistoric ruins, but also gives direct information concerning climatic conditions of those times. This method of crossdating tree rings is illustrated serially through several plates from the year 698 to 1929 A.D.

WOMAN, WINE AND A SAUCEPAN.

By Elizabeth Craig. Chapman and Hall, London. 3s. 6d. net. $7\frac{1}{4} \times 4\frac{3}{4}$; 189; 1936.

People versed in the noble lore of cookery have known for many years that a number of dishes are improved by the addition of certain wines in proper proportions. The present volume should delight the gourmet and amateur chef since it gives many recipes for comestibles which call for wine in their preparation. A glance through the pages shows that sherry, madeira and claret are extensively used and that particular attention has been given to soups, egg dishes, fish, game, poultry, meats and sauces. There is also a section on the serving of wine and a number of suggestions for making those "long and tall" summer drinks. For interested parties this book can be recommended as a useful and readable guide.

RETZER HEIMATBUCH. *Volume 1: Von der Urzeit bis zum ausklingenden Mittelalter* (1526).

By Rudolf Resch. Stadtgemeinde, Retz. 12.40 marks. $9\frac{1}{4} \times 6\frac{1}{2}$; xvi + 430 + 90 + 1 folding map. 1936.

Retz is situated in Austria near the Czechoslovakian border, slightly northwest of Vienna. This nicely produced and illustrated volume contains the history of the community from prehistoric times, through the founding of the town (*circa* 1050), its destruction during the Hussite wars (1278–1425), its rebuilding and to the end of the Gothic period. An appendix supplies a list of source material, genealogies and a history of the houses standing in 1935 and the successive occupants from the time of their construction.

ESSAYS IN ANTHROPOLOGY *Presented to A. L. Kroeber in Celebration of his Sixtieth Birthday, June 11, 1936.*

By various authors. University of California Press, Berkeley. $6.50. $10\frac{1}{4} \times 6\frac{1}{2}$; xxiii + 433; 1936.

These thirty-six essays by as many different authors are too varied in both subject matter and quality to be discussed as a group. Some speculations on New World prehistory by A. V. Kidder appealed to us as being the paper of the most general interest of any in this collection. He maintains that civilization sprang up independently in the Old and New Worlds although he is not a polyenist as regards new world culture. A bibliography of Professor Kroeber is included.

88 DANISH DISHES *or Dining in Denmark.*

By Hetna Dedichen. Andr. Fred. Høst and Son, Copenhagen. Kr. 3. $7\frac{3}{4} \times 5\frac{1}{4}$; 65; 1936 (paper).

These recipes have been collected as those most typical of Danish cooking. It is evident that soup, fish, pastry, and cake play a more prominent part in the Danish cuisine than in the American. Recipes for preparing vegetables and meat are also given, and the description of the "Cold Table" is both colorful and a bit overwhelming in its suggestion of plenitude, not to say plethora. The Danes, like all Scandinavians, eat hearty.

THE FUTURE OF OUR POPULATION.
By C. P. Blacker. The Eugenics Society, London. 6d. $8\frac{1}{2} \times 5\frac{1}{2}$; 14; 1936 (paper).

A brief analysis of trends in rates of births and deaths in England and Wales since 1710, together with estimates of population through the year 2000 (in detail through 1949) based upon the assumption that these trends will continue. Net reproductive rates of 1933 are given for ten other countries. That of Britain is shown to be insufficient for maintenance of its population. Changes in age composition are explained.

DER URWALD RUFT WIEDER. *Meine zweite Forschungsreise zu den Ituri-Zwergen.*
By Paul Schebesta. Anton Pustet, Salzburg and Leipzig. GM. 6. 9×6; 208 + 40 plates + 1 folding map; 1936 (paper).

While far from a systematic ethnography, this travelogue through the country of the eastern Ituri pygmies in the Belgian Congo contains many valuable observations of the natives' material and social culture. Unfortunately it is printed in German rather than Roman type. The 89 photographs are excellent and the index generous.

THE RISE OF MAN THROUGH HIS HANDIWORK.
By David Reisz. Better Education Association, Cleveland, Ohio. 35 cents. $7\frac{1}{8} \times 4\frac{1}{2}$; 36; 1936.

An effort to popularise the art and handicrafts of paleolithic Europeans.

REPORT ON THE WORKS PROGRAM.
Works Progress Administration. Division of Research Statistics, and Records. U. S. Government Printing Office, Washington. $11\frac{1}{4} \times 9$; v + 106; 1936.

THE CHILD. *Monthly News Summary, Vol. 1, No. 1.*
U. S. Department of Labor, Children's Bureau, Washington. $10\frac{1}{2} \times 8$; 28; 1936.

JUVENILE-COURT STATISTICS AND FEDERAL JUVENILE OFFENDERS, 1933. *Based on Information Supplied by 284 Juvenile Courts and by the United States Department of Justice. Seventh Annual Report. Bureau Publication No. 232.*
U. S. Department of Labor, Children's Bureau. U. S. Government Printing Office, Washington. 10 cents. $9\frac{1}{4} \times 5\frac{7}{8}$; iv + 114; 1936 (paper).

ZOÖLOGY

GEORG WILHELM STELLER. *The Pioneer of Alaskan Natural History.*
By Leonhard Stejneger. Harvard University Press, Cambridge. $6.00. $8\frac{1}{4} \times 5\frac{3}{4}$; xxiv + 623 + 30 plates; 1936.

This is a breath taking book. It is the narrative of adventure, the magnificent audacity of which is unsurpassed in scientific history. In 1741, before the intellectuals of Europe and America had ever heard of Alaska, Georg Wilhelm Steller, then 32 years of age, was discovering that country, exploring it, and collecting scientific material from it for the Academy of Sciences at St. Petersburg, and for his friends Linnaeus and Gmelin. Except for the exploration of Lower California by the Jesuit Eusebio Francisco Kino half a century earlier, Steller was the first naturalist to explore any part of the Pacific coast of North America.

Steller is remembered today chiefly for his discovery of the Alaska sea lion, because it perpetuates his name, but this was the least significant of the four marine mammals that he was the first to see, the others being the northern fur seal, the sea otter, and the Arctic sea cow. Of these, the sea cow became extinct within a generation, the sea otter is practically extinct today, and the fur seal would have disappeared long ago had it not been for

the multilateral treaty in which six nations united for its protection.

Despite its pinniped appearance the sea otter is not a seal at all, but a true otter, related to the weasels, and we can imagine Steller's surprise in finding it living in the ocean and feeding on fish and kelp; but the discovery of the sea cow must have astonished him even more, for the order of Mammalia to which it belongs, the Sirenia, consists of but two other legitimate species and two more of doubtful validity, all of them occurring only in tropical regions, and whose nearest living relatives appear to be the elephants. Berings Island on the edge of the Arctic ocean would be one of the last places where one would expect to find such a creature.

Lack of space here precludes mention of Steller's exploration of Lake Baikal, with its remarkable pseudo-marine fauna in fresh water; of his heroism during that winter in the Arctic when the crew of the expedition of whose health he was in charge was being decimated by scurvy—that severe penalty laid by nature on those who tried to penetrate too deeply into her secrets during the eighteenth and some previous centuries; or of his arrest for an offense which he did not commit; or finally of his death in Kamtschatka as a result of treatment received while under arrest. Let it suffice to say that Dr. Stejneger has rediscovered one of the romantic figures of history whom the world has forgotten, but who now lives again in the pages of this remarkable book. Dr. Stejneger has spent a quarter of a century searching three continents for material, and the result is a masterpiece of biographical literature.

WILD LIFE IN SOUTH AFRICA.

By H. A. Bryden. George G. Harrap and Co., London. 15s. 8¾ x 5½; 283; 1936.

Mr. Bryden is a famous old hunter and field naturalist, in the best old-fashioned sense of the word. He has ranged all over South Africa. A keen intelligence and wide knowledge supplements the naïve, sharp-eyed curiosity of the child about everything that lives. The resulting book—in part reprinted from articles published in such journals as *The Field* and *The Spectator*—is a delight to the biologist who is one because of a fondness for life and living rather than for mathematics or physical chemistry. The ecologist particularly will find it a mine of information useful to his purposes.

Space is lacking to demonstrate by quotation the extraordinary interest of the book. But we cannot refrain from noting briefly one observation. In accordance with an established custom a Bushman set fire to the dry grass in a glade, to provide new growth that would attract game.

> As the flames spread they dislodged numberless butterflies and moths, which, mounting into the air, flickered bewildered hither and thither above the red fire and the dun smoke. As if from space had come, attendant upon those teeming delicacies, scores of brilliant rollers, birds of about the size of and having something of the aspect of a jay, resplendent in the most gorgeous plumage of blues, greens, lilacs, violets, and browns, in many exquisite shades. These birds seemed to appear the instant that the fire had begun; their numbers grew rapidly, and they were now feasting greedily on the insects thus provided for them. The rollers, which at this dry season were mostly of one species (*Coracias caudata*), the lilac-breasted roller (the others having gone north with the rains), had been summoned from over a space of some miles of the neighbouring forest by some agency swifter even than the telegraph. How was it managed? How did the tidings spread? Possibly in much the same fashion in which vultures, soaring far apart in the sky, learn from the downward flight of their nearest neighbour that the flesh banquet awaits them below. Anyhow, in three or four minutes from the starting off the Bushman's fire these forest rollers, which are by no means plentiful over a given space, were on the spot and hard at work.
>
> The fire swept on; it drove the Bushman's wife and her babe pell-mell from their *scherm*; and still the devouring birds accompanied the fire, hawking fiercely at every butterfly and insect that they could lay their bills to. In twenty minutes the flames had swept the grass glade clean, the birds had vanished, the show was over.

The book unfortunately lacks an index.

COLOR CHANGES OF ANIMALS IN RELATION TO NERVOUS ACTIVITY.

By G. H. Parker. University of Pennsylvania Press, Philadelphia. $1.50. 9 x 6; x + 74; 1936.

This interesting discussion, originally presented as a Joseph Leidy Lecture at the University of Pennsylvania, reviews the recent studies of Professor Parker and his students "on the means of activating

color-cells in the higher animals and on the significance of these processes for the workings of the nervous system." The author is especially interested in suggesting that neurohumors (chemical activators that pass from the nerve terminal and elicit a response by the effector cell) may be important elements in the general activity of the nervous system. These neurohumors have been studied in certain fish where it has been shown that they play a definite rôle in the control of chromatophores. The following sentence, presenting succinctly the author's conception of neurohumoral activity, aids in picturing the relation to the nerve impulse:

> The conception to which we are finally led respecting the control of melanophores in *Fundulus* is as follows: this control is accomplished through two sets of autonomic nerves, concentrating and dispersing, and, though it is what would be termed a strictly nervous control, it is nevertheless based upon a special type of hormone, a neurohumor, which ordinarily passes directly from the nerve terminal to the effector cell, the melanophore, over an almost submicroscopic distance, but under other circumstances may make its way over stretches of a millimeter or two from its region of origin to distant effectors by way of the lipoid constituents of the intervening tissues.

This is a stimulating and provocative book. It can be recommended to biologists interested in the physiology of the nervous system on the ground that it delineates a new approach and pleads for further investigation. There is an index, a list of references and a number of text photographs.

WHAT PHYSIOLOGICAL PROBLEMS ARE OF INTEREST TO THE MARINE BIOLOGIST IN HIS STUDIES OF THE MOST IMPORTANT SPECIES OF FISH? *Rapports et Procès-Verbaux des Réunions, Vol. 101, 1ème Partie.*

By W. von Buddenbrock. Conseil Permanent International pour l'Exploration de la Mer. Andr. Fred. Høst and Fils, Copenhagen. Kr. 1. 10½ x 8½; 14; 1936 (paper).

THE MEASUREMENT OF SUB-MARINE LIGHT AND ITS RELATIONSHIP TO BIOLOGICAL PHENOMENA. *Rapports et Procès-Verbaux des Réunions, Vol. 101, 2ème Partie.*

Conseil Permanent International pour l'Exploration de la Mer. Andr. Fred. Høst and Fils, Copenhagen. Kr. 3. 10½ x 8½; 66; 1936 (paper).

The first part of this 101st report of the *Conseil International* contains one article by Professor von Buddenbrock on important problems for the marine biologist. The author outlines the problems of (1) sense physiology, including rheotaxis, geotaxis, phototaxis, and vertical traveling, (2) salt and fresh water habitats, (3) physiology of blood and respiration, and (4) nourishment. There is no discussion of original research, the pamphlet is merely an outline of problems. The second part of this report comprises six articles by different authors on the phenomena and measurement of sub-marine illumination. The articles are as follows: Submarine Illumination in Relation to Animal Life, by E. S. Russell; The Photo-electric Measurement of Submarine Illumination in Offshore Waters, by H. H. Poole; Light Penetration in the Western North Atlantic and its Application to Biological Problems, by George L. Clarke; Spectral Bands of Submarine Solar Radiation in the North Pacific and Adjacent Inshore Waters, by C. L. Utterback; On the Unit for Radiation in Oceanic Research, by Anders Ångstrom; and The Transparency of Sea Water, by Hans Pettersson.

THE LIFE HISTORY OF COTYLOPHORON COTYLOPHORUM; A TREMATODE FROM RUMINANTS. *Illinois Biological Monographs, Vol. 14, No. 4.*

By Harry J. Bennett. University of Illinois Press, Urbana. $1.50. 10½ x 7; 119; 1936 (paper).

This monograph records for the first time the appearance of the trematode *Cotylophoron cotylophorum* on the North American continent; describes its life-history as a parasite of ruminants; discusses its morphology, both in adult and immature stages; and presents a historical resumé of previous studies made on the genus. It is shown that the form displays a typical fluke cycle with miracidium, sporocyst and rediae stages confined to a snail (*Fossaria*); with cercaria and metacercaria acting as the infective stages of the vertebrate host; and with adult worms living in the cow's rumen where they

become sexually mature. The time required to complete the entire life-cycle was found to vary between five and eight months. This includes the period during which the worms are migrating from the duodenum, where the metacercaria excyst, to the rumen.

This study gives every indication of being carefully done. Due to its specialized nature it will have little interest for the general zoölogical reader but should, nevertheless, form a constructive addition to helminthological literature. There are a number of drawings depicting the anatomy of the form and a bibliography.

LARVES DE COLÉOPTÈRES AQUATIQUES DE L'EXPÉDITION LIMNOLOGIQUE ALLEMANDE EN INSULINDE.

By Henri Bertrand. Archiv für Hydrobiologie. Suppl. Bd. XIV "Tropische Binnengewässer, Band VI." Stuttgart, 1935. 93 + 11 plates.

A general account of aquatic larvae (Coleoptera) collected by the German Scientific Expedition of Dr. A. Thienemann to Sumatra, Java and Bali (1928–1929). The list of material for each family is preceded by a summary concerning bibliography, biology and systematic study (with dichotomous tables). The illustrations include 39 drawings in the text and eleven plates, concerned with the general morphology and specific features. Thirty-three larval types or genera are described in this work, twelve for the first time. The author, a well-known specialist on dytiscid larvae, working now on the biology of dryopids was helped in diagnosis by comparative study of his own collections and stocks from several museums, chiefly the U. S. National Museum and the National Museum in Paris.

VARIATIONS AND DISEASES OF THE TEETH OF ANIMALS.

By Frank Colyer. John Bale, Sons and Danielsson, London. £2 20s. net. $9\frac{3}{4} \times 7\frac{1}{4}$; viii + 750; 1936.

Sir Frank Coyler, consulting dental surgeon of Charing Cross Hospital and the Royal Dental Hospital of London has given with this volume a taxonomic reference work of great and permanent value, especially for students of comparative anatomy and dental pathology. Variations of the teeth in number and shape, and variation in position are described as found in specimens of the following mammalian orders: Primates, Carnivora, Artiodactyla, Perissodactyla, Hyracoidea, Proboscidia, Rodentia, and Marsupialia. Members of the family Hominidae are not included. Families, genera and even species of these orders are found to have a marked difference in degree of variation. The latter half of the book is devoted to chapters on abnormalities of the teeth resulting from injury and disease. The book is illustrated with over a thousand superb photographs.

References are given as footnotes in the text, but are not summarized into a bibliography. There is an index.

MOSQUITOES OF THE ETHIOPIAN REGION. *I.—Larval Bionomics of Mosquitoes and Taxonomy of Culicine Larvae.*

By G. H. E. Hopkins. British Museum (Natural History), London. 15s. $10 \times 7\frac{1}{2}$; viii + 250; 1936.

This monograph makes available for the first time a comprehensive and up to date account of all the known larvae of Culicine mosquitoes in the whole African region south of the Sahara, in Aden and Southwest Arabia, Madagascar and adjacent small islands, and the islands of the Gulf of Guinea. Introductory chapters on ecology, external morphology and technique are followed by taxonomic descriptions of twelve genera. Since the generic term *Mansonia* is now well established, it seems regrettable that the author has adhered to the earlier *Taeniorhynchus* which is so similar to the name *Taeniarhynchus* applied to a group of worms. This is especially true since the latter holds priority, and although obsolete at the present time is again under consideration for a group of tapeworms. The book is well written and the text is amply supplemented with drawings which show clearly those structures necessary for identification. A supplementary monograph on Culicine adults is due to be published in

1937. There is a list of references and an index to generic, specific, and varietal names.

Morphology of the Insect Abdomen. *Part III. The Male Genitalia (Including Arthropods Other than Insects). Smithsonian Miscellaneous Collections, Volume 95, Number 14. (Publication 3396).*

By R. E. Snodgrass. Smithsonian Institution, Washington. 40 cents. 9¾ x 6¼; 96; 1936 (paper).

This monograph constitutes the third part of Snodgrass' important study. Part I dealt with the general structure of the abdomen and its appendages, and Part II with the genital ducts and the ovipositor. The latter earlier received favorable comment in these columns. (Vol. 9; No. 2; p. 239.) Great care and a skillful selection of text-matter has characterized all three endeavors and entomologists are in the author's debt for making such diverse material readily available to them. The current study presents a straightforward, thorough, and critical account of the anatomical and ontogenetic relationships of the male genitalia of both insects and certain other arthropod classes. Among the insects, the Collembola, Protura, Thysanoptera, Ephemeroptera, Dermaptera, Plecoptera and Orthoptera are discussed. The miscellaneous arthropods are represented by the Onychophora, Pycnogonida, Xiphosurida, Arachnida, Crustacea and Myriapoda. Throughout the text the comparative point of view is admirably stressed and the phyletic interrelations of various groups, as interpreted through the male reproductive system, are emphasized. The volume boasts a good bibliography and a number of helpful illustrations.

The Biology of Mayflies. *With a Systematic Account of North American Species.*

By James G. Needham, Jay R. Traver and Yin-Chi Hsu. Comstock Publishing Co., Ithaca. $7.50. 9½ x 6¼; xvi + 759; 1935.

The Ephemerids have long been recognized as one of the most fascinating of insect orders. This is due, in part, to their interesting life-history which involves the structural and functional degeneration of the imagoes and a remarkable specialization of the nymphs for aquatic life. Despite these attractive features, however, the group has been neglected by entomologists. It is, therefore, a pleasure to welcome this lengthy and authoritative volume on the general biology and taxonomy of mayflies. The book is divided into two sections: one dealing with mayflies in general and discussing their anatomy, embryology, life-cycle, phylogeny and adaptations; the other stressing the systematics and ecology of North American forms. The text material is clearly presented and is supplemented by excellent taxonomic keys, numerous illustrations, and an index and bibliography.

An important addition to entomological reference works.

Zoologica: *Scientific Contributions of the New York Zoological Society, Vol. 21, Part 2, Numbers 3–11.* Containing following articles: *Bermuda Oceanographic Expeditions. Individual Nets and Data, 1932–1935,* by William Beebe; *Plankton of the Bermuda Oceanographic Expeditions. I.* and *II. Notes on Protozoa,* by G. H. Wailes; *Plankton of the Bermuda Oceanographic Expeditions. III. Notes on Polychaeta,* by Edith Berkeley; *Plankton of the Bermuda Oceanographic Expeditions. IV. Notes on Copepoda,* by Charles B. Wilson; *Plankton of the Bermuda Oceanographic Expeditions. V. Notes on Schizopoda,* by W. M. Tattersall; *Plankton of the Bermuda Oceanographic Expeditions. VI. Bathypelagic Nemerteans Taken in the Years 1929, 1930 and 1931,* by Wesley R. Coe; *Tissue Culture and Explantation in Nature: A Review of Certain Experiments and Possibilities,* by C. M. Breder, Jr.; *Preliminary Note on the Nature of the Electrical Discharges of the Electric Eel, Eletrophorus electricus (Linnaeus),* by C. W. Coates and R. T. Cox.

New York Zoological Society, New York. $1.00. 10¼ x 7; 60; 1936 (paper).

Seven of the nine articles in this number of *Zoologica* concern the Bermuda Oceano-

graphic Expeditions of William Beebe. An intensive faunal survey of a circular area about eight miles in diameter was made by the use of nets towed at different depths. A list of these nets with time and weather conditions is given, and a diagram showing the location of the area studied and the arrangement of the nets.

The planktonic contents of a sample of the nets are discussed in general and individual papers are devoted to protozoa, polychaetes, copepods, schizopods and bathypelagic nemerteans. The last is particularly good, discussing the specimens in great detail, and illustrated with ten plates.

The paper on tissue culture and explantation in nature deals with the hypothesis "that animal and plant cells when dislodged from their original location *in situ* by natural causes may continue living independently as distinct organic units," and with the difficulties of experimental verification of such an hypothesis.

A Review of Fishery Statistics in Relation to Wholesale-Index. *Conseil Permanent International pour l'Exploration de la Mer. Rapports et Procès-Verbaux des Réunions, Vol. 98.*

By W. Nellemose. Andr. Fred. Høst et Fils, Copenhagen. Kr. 4.00. 10⅝ x 8¼; 72; 1936 (paper).

In view of the difficulties engendered by currency fluctuations since 1913 in making comparisons of industries or commodities of different countries, this author has made use of the wholesale price index of the countries as a basis for evaluation and comparison of their fisheries. Since the wholesale price index is theoretically homogeneously calculated for practically all countries, it again theoretically offers not only a means of obtaining directly comparable values within a country from year to year, but when these values are converted into a monetary unit of the selected basic year they are supposed to be directly comparable from country to country.

Numerous tables and diagrams illustrate the relationship between quantity of production, value in pre-war shillings, and current values (values for any year converted into shillings at the current exchange rate) for the fisheries of all countries—excepting Spain—adhering to the International Council. Similar diagrams are shown for the comparison of the four most important species: herring, cod, haddock and plaice.

Forest Insects. *A Textbook for the Use of Students in Forest Schools, Colleges, and Universities, and for Forest Workers.*

By R. W. Doane, E. C. Van Dyke, W. J. Chamberlin and H. E. Burke. McGraw-Hill Book Co., New York. $4.50. 9 x 6; xii + 463; 1936.

In preparing the present volume the authors have had in mind the needs of forest supervisors, rangers, park superintendents, timbermen and others for a textbook giving practical information on (1) the insects that attack trees, and (2) certain methods for controlling these pests. Excluding several chapters devoted to the latter aspect of forest entomology, the book is essentially an extended list of brief discussions, arrangd in taxonomic order, about injurious forms. Each entry attempts to describe the species in question; to point out where and how the tree is injured by it, and to suggest corrective measures.

The book will probably serve adequately as a practical guide for foresters. It will not greatly interest biologists or even general entomologists since no effort has been made to organize the material around theoretical principles. A good index, a selection of references following each chapter, and an appendix listing the common coniferous trees of the United States along with their principal insect enemies form useful adjuncts to the text.

The Freshwater Mollusc Helisoma Corpulentum and Its Relatives in Canada. *Canada Department of Mines. National Museum of Canada, Bulletin No. 79, Biological Series No. 21.*

By F. C. Baker. J. O. Patenaude, Ottawa. 25 cents. 9¾ x 6½; 37; 1936 (paper).

First identified by Thomas Say in 1924 as a member of the old genus *Planorbis*, *Helisoma corpulentum* and its allied forms have presented perplexing problems to the naturalist. The present paper brings into an orderly array the Helisomas of the *corpulentum-trivolvis* groups, giving the salient diagnostic features of each form, the type localities and present location of the type material, and a detailed list of the material contained in the museums of the institutions from which specimens were loaned. Most of the specimens figured in the plates (63 figures, 5 plates) are from the collection in the Natural History Museum of the University of Illinois. A close study of these forms and their distribution leads the author to the conclusion that "following the last glacial advance there was an acceleration of evolutionary factors resulting in the appearance of the large species and races of Helisomas so abundantly distributed throughout Ontario and other parts of Canada."

Salar the Salmon.

By Henry Williamson. Little, Brown, and Co. Boston. $2.50. 8 x 5½; viii + 301; 1936.

This is an excellently written book which tells the story of a five year old salmon called Salar, a very old sea trout named Trutta, and Gralaks the grilse, from the time when they come into the estuary from the Atlantic feeding banks in the early Spring until the river cycle ends after the winter spawning. While the period covered is only one year, the entire life cycle of the salmon is described through fish of various ages.

Mr. Williamson's style is charming, the descriptive passages are exquisite, and the illustrations are delightful. It is obvious that the author has studied the subject thoroughly, but one doubts his knowledge of the mental state of fish.

Phytoplankton and the Herring. *Part II. 1933 and 1934. Ministry of Agriculture and Fisheries. Fishery Investigations, Series II, Vol. 15, Number 1.*

By R. E. Savage and R. S. Wimpenny. His Majesty's Stationery Office, London. 4s. 6d. net. 10½ x 7; 88; 1936 (paper).

This technical paper is a continuation of the work done by Savage and Hardy between 1921 and 1923. With the aid of diagrams and tables it covers two years of monthly observations along the Flamborough Line of stations, where there is found a correspondence of young fish, eggs and diatoms. The patches reached a maximum density of 1,377,300 cells per m^3 for *Rhizosolenia* and 2,487,900 cells per m^3 for *Biddulphia*. Evidence is brought forward that within a diatom patch there are relatively more eggs and juveniles in the zooplankton than in the area outside. It is suggested that diatom patches may form a nursery for the animal community which may orientate itself to the patch according to its physiological condition.

Spider Wonders of Australia.

By Keith C. McKeown. Angus and Robertson, Sydney. 6s. 7¼ x 5; ix + 270 + 33 illustrations; 1936.

This is a superb example of the best type of popular scientific writing. The author is Assistant Entomologist of the Australian Museum in Sydney. His book is a mine of information about the biology of spiders, with special references to Australian forms. It is charmingly written, and illustrated with half-tone plates of photographs, some of which are remarkable. There is a good index, but no bibliography.

We recommend the book most warmly. Every biological library, whether private, or high school, college, or university, should have a copy of it.

Comparative Studies of the Fluctuations in the Stocks of Fish in the Seas of North and West Europe. *Reports of the Proceedings of the Special Biological Meeting Held on May 11, 1936, at Copenhagen. Rapports et Procès-Verbaux des Réunions, Vol. 101, 3ème Partie.*

Conseil Permanent International pour l'Exploration de la Mer. Andr. Fred. Høst and

Fils, Copenhagen. Kr. 4. 10¼ x 8¼; 90; *1936* (paper).

Following a meeting in 1935 committees were formed in both Europe and America to collect data which would throw light upon the yearly fluctuations in the stocks of certain commercial fish common to the eastern and western Atlantic. This report embodies the results of the European survey relating to cod, haddock, mackeral, salmon, bluefish, halibut, plaice and hake. Available data were found wanting in some cases and insufficient in the others, but the reports are only intended to be preliminary to further investigations.

DISTRIBUTION OF THE ARGENTINE ANT IN THE UNITED STATES AND SUGGESTIONS FOR ITS CONTROL OR ERADICATION. *U. S. Department of Agriculture Circular No. 387.*

By R. M. Smith. U. S. Government Printing Office, Washington. 5 cents. 9¼ x 6; 39; 1936 (paper).

Iridomyrnex humiles—the Argentine ant——was introduced into the U. S. around 1890 and has now spread through the Gulf states and California, becoming an economic pest of considerable importance. Omniverous in its feeding habits, it causes losses to orchardists, planters, beekeepers and others, and is especially despised as a house infesting insect. This publication presents the results of studies carried out to determine the present distribution and abundance of the insect in the U. S. and describes in detail a method which has been tested and found effective in several Mississippi localities for the eradication or control of infestations.

STRANGE SEA SHELLS AND THEIR STORIES. *How they are made and grow. How they are colored and the patterns produced. Rare shells. Shells that build a raft. Shells that bore in rocks. Giant shells. The shell that sinks ships, etc.*

By A. Hyatt Verrill. L. C. Page and Co., Boston. $2.50. 8¾ x 6; xvi + 206 + 6 plates; 1936.

Anyone, young or old, with a natural curiosity about sea shells will have their interest greatly stimulated and soundly guided by this book. It is not just a descriptive list of various shells, for each shell has its own fascinating story delightfully and simply told. Who would not be interested in reading about, "Shells that Carry Daggers," "Shells that throw out Anchors," or "The Shell that Builds a Raft," or any other of Mr. Verrill's twenty intriguing chapters? The book is illustrated by the author in pen and ink drawings and a frontispiece of nineteen shells in full color. The lack of an index is stupid and deplorable.

BIRDS IN THE WILDERNESS. *Adventures of an Ornithologist.*

By George M. Sutton. Macmillan Company, New York. $3.50. 8½ x 6; xiv + 200 + 10 illustrations; 1936.

This simple and charmingly written book, illustrated by the author in extraordinarily beautiful water colors and pencil drawings, is more of an autobiography than a list and description of the birds. Entertainingly and artistically Sutton has described his adventures as an ornithologist from the far north to the swamps and jungles of the south. His accounts of the birds themselves are more interesting because they come somewhat incidentally into the text as adjuncts to the story of how he found them, and along with the delightful descriptions of their natural environment.

DIE BLATT-MINEN MITTEL- UND NORD-EUROPAS. *Bestimmungs-Tabellen aller von Insekten-Larven der verschiedenen Ordnungen erzeugten Minen. Lieferung 3.*

By Martin Hering. Gustav Feller, Neubrandenburg. Subscription price for 6 numbers: (Germany and Switzerland) 12 marks; (foreign, except Switzerland) 9 marks. 9½ x 6¼; 225–336 + 2 plates; 1936 (paper).

A continuation of an illustrated list of European leaf burrows, previous numbers of which have been noticed in Volume 11 of the Q. R. B. As in the preceding numbers the arrangement is in alphabeti-

cal order of plant hosts and here carries the list from *Forsythia* to *Myrica*.

NATURE PROTECTION IN THE NETHERLANDS INDIES. *A Translation from Supplement to Contribution No. 10 of the Nederlandsche Commissie voor Internationale Natuurbescherming. Special Publication of the American Committee for International Wild Life Protection No. 8.*

American Committee for International Wild Life Protection, Cambridge. 9½ x 6; 73; 1936 (paper).

Following a brief discussion of animal conservation measures in the Dutch East Indies, field notes are presented for most of the larger reptiles and mammals, including the several anthropoids. The observations concern distribution, behavior, superficial appearance, and numbers, being of particular interest to conservationlists.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 453. Methoden der Tierhaltung und Tierzüchtung.* Containing following articles: *Die Züchtung und Pflege von Laboratoriummäusen*, by T. Laanes; *Aufzucht und Haltung der gebräuchlichen Laboratoriumstiere: Amphibien und Reptilien*, by Emil Witschi; *Methoden der Zierfischhaltung und -zucht für wissentschaftliche Zwecke*, by Carl Kosswig.

Urban und Schwarzenberg, Berlin. RM. 6. 10 x 7; 118; 1936 (paper).

The first paper included in this number of *Abderhalden's Handbook* describes the methods of rat breeding and management in use in Dr. MacDowell's laboratory at the Carnegie Institution, Cold Spring Harbor; the second and third give thoroughgoing advice on the care of various amphibia, reptiles and tropical fish used for experimental purposes.

REPORTS OF THE PROCEEDINGS OF THE SPECIAL PLANKTON MEETING *held on May 27th, 1935, at Copenhagen and of the Special Meeting held at Videnskabernes Selskab's Premises on May 31st, 1935. Conseil Permanent International pour l'Exploration de la Mer. Rapports et Procès-Verbaux des Réunions, Volume 45.* Containing following articles: *A Review of some Aspects of Zooplankton Research*, by F. S. Russell; *Further Investigations upon the Photosynthesis of Phyto-plankton by constant Illumination*, by H. Höglund and S. Landberg; *The continuous Plankton Recorder: a new Method of Survey*, by A. C. Hardy; *Die Ergebnisse der internationalen hydrographischen Beobachtungen im Kattegat im August 1931*, by B. Schulz.

Conseil Permanent International pour l'Exploration de la Mer. Andr. Fred. Høst et Fils, Copenhagen. Kr. 3.00. 10⅝ x 8¼; 61; 1936 (paper).

A REVIEW OF THE PHYLLODOCIDAE (ANNELIDA POLYCHAETA) OF THE COAST OF CALIFORNIA, WITH DESCRIPTIONS OF NINE NEW SPECIES. *University of California Publications in Zoology, Vol. 41, No. 10.*

By Olga Hartman. University of California Press, Berkeley. 25 cents. 10¼ x 6¾; 17; 1936 (paper).

LAWS AND REGULATIONS IN SUMMARY CONCERNING SALMON AND TROUT FISHERIES. *Conseil Permanent International pour l'Exploration de la Mer. Rapports et Procès-Verbaux des Réunions, Volume 46.*

Compiled by T. E. Pryce-Tannatt. Andr. Fred. Høst et Fils, Copenhagen. Kr. 2.00. 10⅝ x 8¼; 37; 1936 (paper).

GENERAL CATALOGUE OF THE HEMIPTERA. *Fascicle IV. Fulgoroidea: Part 2. Cixiidae.*

By Z. P. Metcalf. Smith College, Northampton, Mass. $2.00. 9 x 6; 269; 1936 (paper).

BOTANY

THE LIVING GARDEN *or The How and Why of Garden Life.*

By E. J. Salisbury. The Macmillan Co., New York. $3.00. 8½ x 5½; xi + 338; 1936.

The author of this book (awarded a Veitch Memorial medal) has attempted to reconcile two botanical points of view generally regarded as incompatible with each other: the purely scientific and the purely practical. He presents facts familiar to the botanist and facts familiar to the horticulturist, then points out their mutual interdependence. As he says, he

has "tried to present the plant as a living organism in relation to its garden home, interpreted in the light of the most recent research."

It would be impossible to survey the fields of horticulture and scientific botany in their entirety. Besides, the book is not meant to serve as a source of facts, but rather to provide the primary stimulus to that comprehension which, ever increasing, will further coöperation between the horticulturist and the professional botanist. Only relatively few environmental conditions and their effects are considered, first in relation to the living plant in general, to alpines and rock plants in particular, and with reference to their bearing on some horticultural practises such as vegetative propagation and hybridization.

Since the book was written for anyone interested in plants, technical terms have been almost entirely excluded. This results in some lack of precision; but, says the author, "To the layman the technical term may be just unintelligible jargon. . . Therefore, . . . I hope that . . . the presentation will be accepted as an honest endeavor to avoid both technicalities and misconceptions."

The material is presented in an interesting, readable style. The text is illustrated with fair drawings and excellent photographs. There is an index but no bibliography.

THE REDWOODS OF COAST AND SIERRA.

By James C. Shirley. University of California Press, Berkeley. $1.25. 9¼ x 6; 74; 1936.

In the midst of the Coast Redwoods of Humboldt Park there is a highway store where one may purchase gasoline, soda pop, and picture post cards. Conspicuous among the latter are two, one of which bears the caption "The World's Tallest Tree, 368 feet high." The other reads "Redwood Tree over 400 feet high." To the interested and intelligent tourist who inquires "How come?" the clerk replies, "We don't know anything about the trees. We get our post cards from traveling salesmen."

To meet the exigencies of this sort the present work has been prepared by the ranger naturalist of the Yosemite, who is also professor of botany at Phillips University, Enid, Oklahoma. Within its narrow compass there is arranged in an orderly and compact manner all facts which the visitor to the redwood forests wants and cannot get. Not only are the two recent species of redwood discussed from the standpoint of botany, history, literature, etc. but the fossil redwood forests of Calistoga, the Yellowstone, and Laurence Island are included, as well as some other trees, famous for their height and girth, including the eucalyptus of Australia, the kauri pine of New Zealand, the Cryptomeria of Japan, the Chapultepec cypress of Oaxaca, and the banyan tree of India. One looks in vain, however, for any mention of the chestnut tree at Catania under which one hundred horses of Napoleon's cavalry are said to have found shelter from a thunderstorm.

The book is attractively gotten up in a cover made to resemble redwood slab, and is illustrated with photographs from many sources. Not the least of its merits is the demolition of the all too prevalent idea for which no less a person than John Muir seems to have been responsible, that the Sequoia of the Sierras is not a redwood.

BOTANY. *A Textbook for Colleges. First Edition.*

By J. Ben Hill, Lee O. Overholts and Henry W. Popp. McGraw-Hill Book Co., New York. $4.00. 9 x 5¾; xiii + 672; 1936.

The present book, an outgrowth of the general botany course at Pennsylvania State College, has been designed to meet the needs of first year college students for a text that covers in considerable detail the fundamental aspects of plant biology. The volume is divided into two parts each corresponding essentially to a semester's study. Part I, entitled "The structure and physiology of seed plants," emphasizes the elements of plant anatomy, metabolism and reproduction. Part II, discusses the classification, distinctive features and ecology of algae, fungi, liverworts, mosses and spermatophytes. Due

to this dichotomy of "principles" from "types" the book should prove very adaptable for collegiate teaching, and, supplemented by laboratory exercises, should provide a thorough survey of the plant kingdom. In the main, the authors succeed in writing clearly yet maturely about their subject. Furthermore, they are not afraid to introduce the student to technicalities. The book exhibits few of the symptoms of spoon feeding so characteristic of many introductory texts. It is well illustrated and indexed.

GROWTH HORMONES IN PLANTS. *Authorized English Translation of Die Wuchsstofftheorie und ihre Bedeutung für die Analyse des Wachstums und der Wachstumsbewegungen der Pflanzen.*

By P. Boysen Jensen. Translated and Revised by George S. Avery, Jr., and Paul R. Burkholder with the Collaboration of Harriet B. Creighton and Beatrice A. Scheer. McGraw-Hill Book Co., New York. \$3.50. 9 x 6; xiv + 268; 1936.

The English translation of Professor Boysen Jensen's *Die Wuchsstofftheorie* is the first comprehensive review of the literature dealing with the rôle of growth hormones in normal growth and tropisms of plants. As a result of intensive study of the *Avena* coleoptile, methods have been developed to show the significance of the growth hormone in the normal growth of plants and in the different tropisms. Something is known of the properties and methods of preparation of the hormone, but the chemical formula is still empirical. Attempts have been made to explain the mechanism of transportation of growth hormone in plants, but definite quantitative proof is still lacking. Professor Jensen's review of the subject makes clear numerous weaknesses and gaps in our knowledge and thus helps to point the way for future research.

The bibliography is quite extensive and includes the 1935 literature.

DISINFECTION AND STERILIZATION.

By Ernest C. McCulloch. Lea and Febiger, Philadelphia. \$5.50. 9½ x 5¾; 525; 1936.

The comprehensive scope of this book may be indicated by a review of the chapter headings: the development of our knowledge of disinfection and sterilization; natural agencies which control microbial populations; germicidal properties of the body, its fluids, and secretions; the destruction of microörganisms by radiant energy emanations; the effect of temperature upon microbial life; pasteurization; other physical agents; how disinfectants are compared; the dynamics of disinfection; the acids; the alkalies; the heavy metals and their salts; the dyes; the phenols, cresols, alcohols and related compounds; the halogens; water purification; sewage treatment; miscellaneous disinfectants; the selection of a disinfectant. Each chapter is followed by a list of references.

While presenting the most up to date information available on the above subjects, it has been the aim of the author to make "no attempt to limit the presentation of data to irrefutable facts and indisputable hypotheses." It has been his hope thereby to stimulate investigation of the many slightly understood aspects of germicidal action. The book has an index.

PFLANZENGEMEINSCHAFT UND UMWELT. *Ergebnisse und Probleme der botanischen Standortforschung.*

By Paul Filzer. Ferdinand Enke, Stuttgart, RM. 5. 9 x 5¾; vii + 98; 1936 (paper).

Discusses various ecological matters that the author has observed in his travels. The first chapter considers the struggle for water which plants undergo in a dry region. This is followed by an account of plant life in a German beech forest. The third chapter deals with the struggle for existence that plants undergo in the mountains, and the special protections they have against the cold. Chapters on plants living on the edge of the sea, and on marine algae complete the volume. The book is written for the layman who has a knowledge of the rudiments of botany and wishes to inform himself more thoroughly for his travels.

Growth and Survival of Deciduous Trees in Shelter-Belt Experiments at Mandan, N. Dak., 1915–34. *United States Department of Agriculture Technical Bulletin No. 496.*

By Ernest J. George. U. S. Government Printing Office, Washington, D.C. 5 cents. $9\frac{1}{8} \times 6\frac{3}{4}$; 48; 1936 (paper).

The object of this extensive tree planting experiment was to find what kind of trees and what arrangements of trees would furnish the best protection to buildings and orchards on the northern Great Plains from damaging winds and drifting snow. Only the broad-leafed trees are considered in this report, although coniferous species are included in other experiments in the shelter-belt investigations. For the several experimental belts data were obtained on height growth, survival, adaptability and congeniality of species, the most suitable trees for outside rows and the optimum width of shelter belts. Several tables present the actual data of the experiment. There is a bibliography of 14 titles.

Food Plants of the North American Indians. *U. S. Department of Agriculture Miscellaneous Publication* No. 237.

By Elias Yanovsky. U. S. Government Printing Office, Washington. 10 cents. $9\frac{1}{8} \times 6$; 84; 1936 (paper).

This publication is a compilation of all the plants known to have been used as sources of food by the Indians of North America, with brief notes as to the ecology of the plant, the part used as food and method of preparation. There is a bibliography and an index which includes both common and scientific names. There is also a table which presents a summary of families, with number of genera and species of each.

Urdeutschland. *Deutschlands Naturschutzgebiete in Wort und Bild. Lieferung 20.*

By Walther Schoenichen. J. Neumann, Neudamm. 2 marks. $10\frac{3}{4} \times 8\frac{1}{4}$; 169–192 + 8 plates; 1936 (paper).

Continuing this beautiful publication noticed in earlier numbers of the Q. R. B., the present section discusses the old trees that still exist in Germany. There are several full page colored prints, reproductions of old masterpieces, and a great many fine photographs of some of the old oaks. About twenty pages are devoted to description, the rest to photographs. There are eight full page plates.

A Dictionary of British Wayside Trees.

By A. W. Holbrook. Country Life, London. 7s. 6d. $7\frac{1}{4} \times 4\frac{1}{2}$; 236 + 46 plates; 1936.

Arranged in alphabetical order, over sixty species are described in this book. The author attempts to show the layman how to distinguish between the familiar trees of Britain in winter as well as in their summer foliage. The book is illustrated by excellent photographs of the leafless winter trees, and pen and ink drawings of their buds, leaves, and fruit. The author is his own illustrator. There is an index of the trees described, and a glossary, but nothing in the way of bibliography.

Handbuch der Biologischen Arbeitsmethoden. *Lieferung* 455. *Ernährung und Stoffwechsel der Pflanzen.* Containing the following article: *Methoden der experimentellen Untersuchung mykotropher Pflanzen*, by Elias Melin.

Urban und Schwarzenberg, Berlin. RM. 5. 10×7; 94; 1936 (paper).

The methods outlined and discussed in this *Lieferung* of the *Abderhalden Handbook* are chiefly those which have been used by investigators of the physiological properties of the mycorrhiza plants. A seven-page bibliography is provided.

Black Rock Forest Papers, Vol. I. *Trends in the Nitrogen, Phosphorus, Potassium and Calcium Content of the Leaves of Some Forest Trees During the Growing Season, by H. L. Mitchell.*

Black Rock Forest, Cornwall-on-the-Hudson, New York. $11 \times 8\frac{1}{2}$; 29–44; 1936 (paper).

MORPHOLOGY

THE COMPARATIVE ANATOMY OF THE NERVOUS SYSTEM OF VERTEBRATES, INCLUDING MAN. *In Two Volumes.*

By C. U. Ariëns Kappers, G. Carl Huber and Elizabeth C. Crosby. Macmillan Company, New York. $16.00. 10½ x 7⅞; Volume 1, xvii + 864; Volume 2, xi + 865–1845; 1936.

These are volumes to which the adjective "distinguished" may be liberally yet honestly applied for they are notable in their authorship, scholarship, format and organization. Indeed, it is rare that a reference work in any field attains the standard found in the present books. The entire enterprise

> ... is an outgrowth of *Die vergleichende Anatomie des Nervensystems*, which was written by C. U. Ariëns Kappers in 1920–1921. When, in 1926, the present text was begun, the original plan on the part of both Dutch and American collaborators was to translate the German text into English and to make such revision as should be deemed necessary in the light of later literature. However, so great additions had been made to the knowledge of the comparative anatomy of the nervous system during the years following its publication that it soon became evident that, in order to do justice to the situation, much of the text must be entirely rewritten, and additions made to the figures. The idea, then, of presenting a translation was abandoned. Thus ... the present book offers a new presentation of the material available in comparative neurology, based on the available literature and on results of the research programs of the Institute of Brain Research at Amsterdam and the Laboratory of Comparative Neurology at the University of Michigan, both of which have collaborated in the preparation of the present text.

The first volume, containing an introduction and seven chapters, discusses The evolution and morphology of nervous elements; the comparative anatomy of the spinal cord; the medulla oblongata; the lateral line and acoustic systems; the effectory system of the midbrain and the medulla oblongata; the coördinating apparatus; and the cerebellum. The second volume, in addition to a subject and an author index, has chapters on the mesencephalon and the diencephalon; the telencephalon; and the development of the cortex in mammals. The material of each chapter is presented in taxonomic order with the cyclostomes, plagiostomes, ganoids, teleosts, amphibians, reptiles, birds and mammals typically discussed. An extended and superbly complete bibliography follows each chapter, and citations to the literature are cross-referenced in the text. Many excellent illustrations supplement the discussions.

This book, covering intensively and authoritatively an important field of biological research, will unquestionably find a permanent niche among the classics of scientific literature. It is, in truth, a monumental study.

ANATOMY OF THE HUMAN BODY. *Twenty-third Edition.*

By Henry Gray (Thoroughly Revised and Re-edited by Warren H. Lewis). Lea and Febiger, Philadelphia. $10.00. 10¼ x 6½; 1381; 1936.

COMPARATIVE ANATOMY.

By Herbert V. Neal and Herbert W. Rand. P. Blakiston's Son and Co., Philadelphia. $4.75. 9 x 6; xxi + 739; 1936.

It would be both pedantic and somewhat ludicrous to review in any detail even a current copy of Gray's Anatomy. This lusty veteran, having withstood, in 23 editions, the rigorous manhandling of 78 years of intensive service, needs neither appraisal nor introduction. It, as the politicians say, "stands on its record." The current edition has been altered by correcting and modernizing the general text; by rewriting parts of the sections dealing with embryology, the central nervous system, the endocrines, and physiological anatomy; by adding new and more effective illustrations, and by appending recent references to anatomical literature. The book is strongly bound and well indexed and will, of course, continue to be the middle-man between cadaver and medical student.

In contrast to Gray's Anatomy which is of ancient origin, *Comparative Anatomy* by Neal and Rand is a new book just beginning its career. Whether the career will be as long and remarkable as Gray's remains to be seen. It is likely, however, that the book is destined for considerable longevity in its own right since it is a usable and skillfully prepared text covering the conventional subject matter of vertebrate morphology. The facts presented have been selected because they

"throw light upon the important problem of man's place in nature and . . . help the student to understand the major functions of his body." A chapter is devoted to each anatomical system (viz., integumentary, skeletal, muscular, digestive, respiratory, vascular, urogenital, endocrinal and nervous) and there are, in addition, brief discussions of the animal kingdom, histology, reproduction and vertebrate phylogeny. The authors rightly emphsize each structural element in terms of its specific function and their treatment, coupled with laboratory dissection, should provide the student with a sound appreciation of the vertebrate pattern. The illustrations are good and the glossary and index helpful.

BONES. *An Essay on the Development and Structure of the Vertebrate Skeleton.*

By P. D. F. Murray. The University Press, Cambridge; The Macmillan Company, New York. $2.50. $7\frac{1}{2} \times 5\frac{3}{8}$; x + 190 + 8 plates; 1936.

An extremely interesting, well-written, and critically sound digest of recent work on the morphogenesis of skeletal parts, and an exposition of the author's own views as to the nature of the processes involved.

The time is past when it was possible to describe the whole long and complex story of skeletogenesis in terms of the effects of mechanical stresses, whether of extrinsic and functional origin or arising in direct consequence of the developmental process itself. The form of the cartilaginous model is produced, at least in the case of the larger shafted elements, under the aegis of a growth pattern intrinsic from a very early stage within each element. Neither the functioning of muscles, nor mechanical interaction between the developing elements, can now be allowed any major rôle in the first production of form, even though the second set of factors does play an essential part in bringing about the separation of the coninuo us anlage into the several elements of the limbs.

The author regards the form and structure of bone as a compromise between many factors. The adaptiveness shown in bony structure to functional requirements has long been stressed in the literature of regulation. But Murray's book makes it perfectly plain that the mechanist explanation is not good enough by itself to furnish a reasonable picture of the basic biology of bone building.

THE EGGS OF MAMMALS. *Experimental Biology Monographs.*

By Gregory Pincus. The Macmillan Co., New York. $3.75. $8\frac{1}{2} \times 5\frac{3}{4}$; ix + 160; 1936.

This monograph gives an excellent, concise review of the experimental investigations on the growth and development of the mammalian ovum from the moment of its functional differentiation in the ovary until its implantation in the uterus. The several chapters discuss: the origin of the definitive ovum; its growth; atresia and parthenogenesis, together with a description of the intrinsic and extrinsic factors affecting the cleavage process; recently developed techniques for the experimental manipulation of living ova; activation of unfertilized eggs; and vesicle growth and implantation. A bibliography of 24 pages and author and subject indices are provided.

HOW WE CAME BY OUR BODIES.

By Charles B. Davenport. Henry Holt and Co., New York. $3.75. $7\frac{3}{4} \times 5\frac{1}{2}$; xii + 401; 1936.

A broad range of subjects and many facts are compressed into this little book. Its three parts cover: human ontogeny, the machinery of development—cells, genes, specialised tissues, adaptations, endocrines—the origin of life, contemporary human evolution—with an extended treatment of variation and heredity—primate relationships, and the question of the future of our species. Recent advances in genetics, organic chemistry, and physical growth are brought out. Richly illustrated with 236 photographs and charts, written in popular style, the book aims to stimulate interest in children, "as the most wonderful of natural creations."

DAS WERDEN DER TIERFORMEN. *Eine Einführung in die Grundfragen der Entwicklungsphysiologie.*

By H.-A. Stolte. Ferdinand Enke, Stuttgart. RM. 7. $10 \times 6\frac{1}{4}$; vi + 112; 1936 (paper).

This introduction to embryology commences with a description of the cells,

chromosomes, and the process of mitosis. Examples are drawn from the familiar sea urchin, *Tubifex* and amphibian forms. Two chapters are devoted to metamorphosis, regeneration, transplantation and explanation. The concluding chapter on the underlying general principles of morphogenesis is essentially preformationist in its philosophy. The author regards as the most fundamental characteristic of living substance the universal presence in it of paired structural opposites; "colloidal particles-solute, sol-gel formation in the nucleus: achromatin-chromatin and finally the total plasma-nucleus system are such opposites. At the boundary surfaces of these pairs the essential life phenomena manifest themselves."

There is a brief bibliography and an adequate index.

Adventures Before Birth.

By Jean Rostand. Translated by Joseph Needham. Victor Gollancz, London. 4s. 6d. $7\frac{1}{4}$ x 5; 157; 1936.

Credit is due to the author of this book for doing well a task that many fail in,—that of presenting for lay consumption a scientific subject without the aid of many scientific terms, while at the same time giving a clear interpretation of facts. The history of the germ cells, the development of the fertilized egg, embryo and foetus are chronicled in sufficient detail to render a satisfactory panoramic view of these processes.

Apparatus for the Dissection and Study of Embryos. *University of California Publications in Zoology, Vol. 41, No. 9.*

By J. A. Long. University of California Press, Berkeley. 25 cents. $10\frac{1}{4}$ x $6\frac{3}{4}$; 18; 1936 (paper).

In this apparatus electrically controlled knives (made of safety-razor blades) operate under the binocular dissecting microscope. A camera photographs the dissections stereoscopically. The text is supplemented by 3 photographic plates.

PHYSIOLOGY AND PATHOLOGY

Starling's Principles of Human Physiology. *Seventh Edition.*

Edited and Revised by C. Lovatt Evans. The Chapters on the Central Nervous System and Sense Organs Revised by H. Hartridge. Lea and Febiger, Philadelphia. \$8.75. $9\frac{3}{8}$ x 6; xiii + 1096; 1936.

A Textbook of Physiology *for Medical Students and Physicians. Thirteenth Edition, Thoroughly Revised.*

By William H. Howell. W. B. Saunders Co., Philadelphia. \$7.00 net. $9\frac{1}{8}$ x $5\frac{3}{4}$; 1150 + 6 plates; 1936.

Human Physiology. *A Text-Book for High Schools and Colleges. Seventh Edition, Revised.*

By Percy G. Stiles. W. B. Saunders Co., Philadelphia. \$2.25. $7\frac{1}{2}$ x 5; 446; 1936.

An Elementary Manual of Physiology. *For Colleges, Schools of Nursing, of Physical Education, and of the Practical Arts. Fifth Edition, Revised.*

By Russell Burton-Opitz. W. B. Saunders Co., Philadelphia. \$2.50. $7\frac{1}{2}$ x $5\frac{1}{4}$; 442; 1936.

A Text-Book of Physiology. *Second Edition.*

By H. E. Roaf. William Wood and Co., Baltimore. \$6.75. $8\frac{1}{2}$ x $5\frac{1}{2}$; viii + 679; 1936.

Starling's text has been completely revised and brought up to date by Professor Evans, Professor Hartridge revising the chapters on the nervous system and sense organs. The greatest changes and additions have been made on the chapters dealing with biochemistry. The references, rather more numerous than in most text books on physiology, are put in as footnotes and at the end of chapters.

Dr. Howell has been somewhat more conservative in revising his classic work, making many minor changes here and there but few large ones. He too has picked biochemistry as the subject requiring the greatest addition and rewriting.

Dr. Stile's text book and Dr. Burton-Opitz's text book, both being much more elementary than the two just mentioned, needed little revision since neither one goes into any detail on biochemistry or other rapidly changing fields of physi-

ology. The chapter on internal secretions has been expanded and revised in the former, while a new part on reproduction has been added in the latter.

Dr. Roaf's text, not previously reviewed in this journal, falls between the first two books and the last two books mentioned above in respect to its complexity and scope. The whole field is covered in a general sort of way but not very thoroughly and with but a few references to other text books. If a book is not intended to be profound, in compensation it should be easy to read. Dr. Roaf, in our estimation has not exactly rung either bell.

MÉTABOLISME ET OBÉSITÉ. *Publications du Travail Humain Série A. No. 7.*

By W. Liberson. Conservatoire National des Arts et Métiers, Paris. 25 francs. 10 x 6½; 157; 1936 (paper).

The author has obtained the basal metabolism of 688 healthy women whose only complaint was obesity. Incidentally, he notes that only about one-third of these women could really be called obese. [Reginald, the Office Boy queries as to whether these others were called luscious, or merely Mae.]

The observed relationship between basal metabolism and weight, stature and age, leads to the conclusion that stature is unnecessary in the calculation of the expected individual metabolism. He finds that Dreyer's formula, which places metabolism directly proportional to the square root of weight and inversely proportional to age, fits best his series of observations. The most significant point of this investigation appears to be that, contrary to general assumption, the expected or standard metabolism of obese women can be calculated directly from the general equations used for the calculation of the metabolism of the non-obese.

The author has carefully and critically reviewed the literature and although he seems to miss at times the significance of his statistical results, this monograph should stimulate others to base standards, such as these, on a sufficiently large number of individuals. The bibliography is fairly complete.

BIOLOGISCHE WIRKUNGEN DES ALKOHOLS AUF DEN STOFFWECHSEL. *Ergebnisse experimenteller Untersuchungen in dem von mir geleiteten Laboratorium unter Verwendung der Widmarkschen Mikromethode der Blutalkoholbestimmung über Wirkungen des Alkohols auf den Stoffwechsel, über die Verwertung des Alkohols bei der Muskelarbeit, über Beziehungen des Blutalkoholgehaltes zum Rauschzustande, über Insulinbehandlung der Alkoholintoxikation und die Insulinblockade der Ganglienzellen.*

By Adolf Bickel. Georg Thieme, Leipzig. .90 marks. 9 x 6; 31; 1936 (paper).

This volume discusses the results of experiments on (1) the effect of alcohol on metabolism, (2) the utilization of alcohol during muscular activity, (3) the relation of a state of intoxication and alcoholic content of the blood, (4) the treatment of alcohol intoxication with insulin, and (5) the insulin blockade of the ganglion cells. The author finds that frequently, though not uniformly there is a rise in basal metabolism after consumption of alcohol. During muscular activity alcohol is oxydized in considerable quantity, but however without constituting the source of muscular strength, as is shown from the decrease of the alimentary alcohol curve occurring with a simultaneous rise of the respiratory quotient above its alcohol value. The alcohol enables the carbohydrate supply of the body to stretch farther, and thereby promotes indirectly the revictualing of the muscles with sugar for their work. The normal supply of insulin in the body is without demonstrable effect on the metabolism of alcohol in the blood arising from alcoholic consumption. An increased blood sugar content seems to hinder the development of a state of intoxication. Subcutaneous injections of insulin bring about a decrease in the amount of blood sugar and alcohol in the blood. Insulin shortens the duration of a state of intoxication. This phenomena is brought about by what the author calls the insulin blockade of the ganglion cells.

THE VEGETATIVE NERVOUS SYSTEM. *A Clinical Study.*

By Wulf Sachs. Introduction by Walter

L. Brown. Cassell and Co., London. 15s. net. 8½ x 5½; x + 168 + 8 plates; 1936.

The book is an attempt to give the reader

a quick orientation in and a practical approach to the whole question of vegetative imbalance; would give, in other words, an ordinary description of the various tests, clinical observations, diagnostic criteria and the essential anatomical, physiological, and pharmacological data.

The author shows that, on the anatomical side, the principal additions to our knowledge of the vegetative nervous system since the pioneer discoveries of Gaskell and of Langley have been in relation to the ties between it and the central nervous system. Emphasis is placed on the important influence of the pre-existing state of tonus in the vegetative nervous system on the results obtained by subsequently stimulating it. A case record is reproduced in the appendix to demonstrate the scheme according to which all examinations were carried out. Fifty different tests were applied to each patient. The bibliography cites the most important references. A subject and an author index have been added.

Vitality and Energy in Relation to the Constitution.

By T. E. Hammond. H. K. Lewis and Co., London. 12s. 6d. 8½ x 5½; xii + 314; 1936.

Whenever the physician really becomes interested in treating the patient and not the disease alone, he becomes conscious of the existence of something, an X, which differentiates the patient who recovers from the one who succumbs to an infection. This unknown factor is by Hammond called the vitality of the individual. The term is never clearly defined but is stated as being akin to muscular tonus and its centre is supposed to be somewhere at the base of the brain. Although his concepts are criticizable and the terminology is vague, the main point of this work cannot be disregarded. It is that therapy to be efficacious must be based on knowledge of the individual constitution and its reaction to disease. The book is written in an engaging style and is replete with pertinent observations drawn from the author's long clinical experience. Especially interesting is the fact that his philosophy of constitution is drawn from the teachings of such great clinicians as Paget and Hutchinson. There is no bibliography and no mention is made of the recent vigorous revival of constitutional studies.

Dental Survey of School Children, Ages 6–14 Years Made in 1933–34 in 26 States. *Public Health Bulletin No. 226.*

By C. T. Messner, W. M. Gafafer, F. C. Cady and H. T. Dean. U. S. Government Printing Office, Washington. 20 cents. 9 x 6; vi + 248; 1936 (paper).

Almost one and a half million children were examined in a dental survey conducted by the United States Public Health Service in 1933–34. In this bulletin are presented all the data collected regarding the oral conditions and needs of the children. Separate tabulations have been made for each county of each of the 26 states. There is no general summary of the results nor have any conclusions been drawn from the mass of information obtained. It is probable that the chief reason for this uncommon reticence is that the dental examinations were conducted by a number of individuals and the technique of examination and of recording was not uniform. Just what was achieved by the survey is not clear to this reviewer. There is no bibliography. The reader is forewarned that the bulletin consists of 228 pages solidly filled with tables and 20 pages of text and tables.

Contraception as a Therapeutic Measure.

By Bessie L. Moses. The Williams and Wilkins Co., Baltimore. $1.00. 7¼ x 4¾; xiii + 106; 1936.

The Bureau for Contraceptive Advice in Baltimore was established to determine the effectiveness of currently advocated methods of birth control. It was in operation for five years and collected more than one thousand cases which are studied and analyzed in this book. Information concerning color, mental status,

previous use of contraceptives, number of abortions and type of contraceptive prescribed is given. The value of the Bureau's work is shown in the fact that 85 percent of the patients were unsuccessful in their use of contraceptives previously, while after treatment and advice the method failed in only 2.71 percent of the total number of cases. The improvement in the mental and physical health of the patients was marked. There is a good index and bibliography.

PRINCIPLES AND FOIBLES OF CANCER RESEARCH. *In regard to Etiology and Nature.*
By William Rienhoff, Sr. Waverly Press, Baltimore. $2.50. 9 x 5¾; vii + 200; 1936 (paper).

In this scholarly discussion of the etiology of cancer the author reviews critically although briefly a good proportion of the vast literature on the subject. His purpose is to clear "the overgrown field of investigation from luxuriant weeds and thereby admitting air and light to nurslings that have been looked for so long in vain because of their smothered existence, ..." These nurslings turn out to be the alleged proofs of the existence of cancer virus. In the author's opinion, the problem of the etiology and treatment of cancer can only be solved by intensive research on filterable viruses. All other theories and modes of investigation are to be discarded forthwith. The author may be entirely correct in his beliefs, but full acceptance of this or of any other theory depends on factual demonstrations and not on verbalism.

There is an extensive bibliography of some 27 pages.

LES AVORTEMENTS MORTELS.
By H. Mondor. Masson et Cie, Paris. 65 francs. 10 x 6½; 445; 1936 (paper).

This book, combining the results of a study of the world literature on the subject and the author's own extensive experience, is practically an encyclopaedia of all the harmful effects that might possibly result from abortion. Among the more important aspects of fatal abortion dealt with are: sudden death (especially from embolus); perforation of the uterus; intrauterine instillations; infarction, gangrene, and abscess of the uterus; postabortal peritonitis, septicemia and hemorrhage; attempts at abortion in the presence of an ectopic gestation; and attempts at curettage of the uterus in women who are not pregnant. Considerable space is devoted to treatment. The numerous case histories and illustrations are chiefly from the author's material. The book is adequately documented and contains an author index arranged by the chapter headings. This is an invaluable and unique reference work in its field.

ALLGEMEINE SINNESPHYSIOLOGIE. *Stellung und Bedeutung des sinnesphysiologischen Versuches im Bereich der Observation, des exakten Experimentes und der Begriffsbildung.*
By Yrjö Renquist-Reenpää. Julius Springer, Vienna. RM. 12. 9¼ x 6¼; v + 160; 1936 (paper).

This is a somewhat abstruse work on the importance of the senses in the fields of scientific observation, experimentation, and thinking. The author struggles with the fact that all observations depending on the senses for determination are necessarily bound to involve error, and he believes therefore that attempts should be made to get at the fundamental factors in the physiology of the senses. He discusses his material from the physical, chemical, mathematical, and physiological viewpoints, and deduces a great many equations of extremely complex nature to show the sensitivity of the skin, fingertips, taste receptors, etc. to various stimuli. The attempt is praiseworthy but the results are so highly complex that it is doubtful that this work will prove practically useful.

TISSUE IMMUNITY.
By Reuben L. Kahn. Charles C Thomas, Springfield, Ill. $7.50. 9 x 6; xix + 707; 1936.

An important treatise. The author presents as a basis for the work his own numerous studies, and correlates the re-

sults with manifestations of infection and immunity noted in the clinic. He states that "although the experiments in this volume have been directed mainly toward the enlarging of our understanding of the laws governing immunity to protein and microbial antigens, actually these experiments throw light on the mechanisms of different infectious diseases, and of many immunologic phenomena, whether advantageous or antagonistic to the host, constantly noted in the clinic." The experiments indicate that "tissues in immunity are defensive and not hypersensitive" and the question is raised whether in the light of newer immunologic knowledge the concept of tissue hypersensitiveness is tenable.

The author has introduced the plan of putting only the graphs in the text and the tabular matter at the end of the chapters. The volume contains a bibliography and an excellent index. We recommend this to clinicians, and students and investigators, including those in the field of parasitism, as a highly useful and stimulating book.

Digestion and Health.

By Walter B. Cannon. W. W. Norton and Co., New York. \$2.00. 8 x 5½; 160; 1936.

This is a rearrangement of the material presented by Dr. Cannon in a series of lectures commemorating William Beaumont whose experiments are the basis of the modern study of the physiology of digestion.

The book takes up the ways in which appetite, hunger, thirst, general health, and emotional excitement affect the digestive process. The description of the method used to record gastric hunger contractions and the discussion of the effect of the extrinsic nerves of the alimentary tract on the tonicity of the gastric masculature will interest and enlighten the general reader. The material is so clearly presented that it should be comprehensible to everyone. The inclusion of numerous graphs and drawings is of great value. There is a bibliography at the end of each chapter and a good index.

Hormone des Ovariums und des Hypophysenvorderlappens. *Untersuchungen zur Biologie und Klinik der Weiblichen Genitalfunktion. Mit einen Anhang: Hormonale Schwangerschaftsreaktion. Hormon des Hypophysenzwichenlappens. Zweite vermehrte Auflage.*

By Bernhard Zondek. Julius Springer, Berlin. RM. 58 (paper); RM. 59.80 (cloth). 9¾ x 6¼; xi + 638; 1935.

Since the first edition of this book appeared in 1931 the author has continued his research on the hormones of the ovary and hypophysis and their effects on the sexual apparatus and reproductive functions of the female. These further investigations are incorporated in this second edition, with the result that it is almost twice the size of the original. The additions include fifteen new chapters, chiefly on prolan, 56 new illustrations and changes or additions to the other chapters. The book is documented but a systematic bibliography has not been provided. There is a subject index.

Experimental Studies on a Transmissible Myelomatosis (Reticulosis) in Mice. *Acta Radiologia Supplementum XXIX.*

By Otto Kaalund-Jørgensen. Levin and Munksgaard, Copenhagen. Dan. Kr. 2. 10½ x 7¾; 142 + 15 plates; 1936 (paper).

The author finds: (1) that myelomatosis is readily transmitted by apparently metastasis-free brain tissue; (2) that myelomatosis can be transmitted by tumor material diluted 1 to 400,000 (equal to each mouse getting about 8 cells); (3) that myelomatosis cannot be transmitted even to pre-irradiated mice by positively cell free material; (4) that myelomatosis can be transmitted by other metastasis-free organs, and by non-leukotic blood. The experimental technique is described in detail, and at all stages the evidence is checked against the findings of previous investigators. The data are treated statistically throughout. There is an excellent bibliography, and, in addition, fifteen plates with explanatory notes accompanying each figure.

LANE MEDICAL LECTURES: STUDIES IN CARDIOVASCULAR REGULATION.

By G. V. Anrep. Stanford University Press, Stanford University. $1.50 (paper); $2.25 (cloth). 10 x 7; 118 + 14 plates; 1936.

In the five lectures here reported the author discusses certain aspects of the experimental work done in the field of circulatory physiology. He mentions mostly his own experiments and those of his colleagues and assistants and refers to those of other physiologists only when their findings tend to confirm, supplement or contradict his own. The subjects discussed concern the proprioceptive mechanism of cardiovascular regulations, the respiratory regulation of the heart rate, the dynamics of coronary circulation, the coronary blood flow, the blood flow through skeletal and plain muscles. The experiments and technique are described in great detail and illustrated by drawings and photographs.

MENSTRUATIONSSTÖRUNGEN HORMONALEN URSPRUNGS. *Eine Klinische Untersuchung.*

By P. N. Damm. Levin and Munksgaard, Copenhagen. 12 kroner. $9\frac{1}{2}$ x $6\frac{1}{2}$; 285; 1936 (paper).

This is a presentation of the results and methodology of experimental work conducted by the author in the Woman's Clinic of the University, Copenhagen, on the cyclical changes in the ovary and uterus; analyses of the follicular hormone; the nature of the gonadotropic hormone; and hormonal effects on menstruation and its disorders. Thirty-seven typical case histories are included. The significance of the work is largely clinical and its chief interest will be to gynecologists. The bibliography, 19 pages in length, contains only those titles consulted by the author during his investigations. An index is lacking.

FOOD, FITNESS AND FIGURE.

By Jacob Buckstein. Introduction by Harlow Brooks. Emerson Books, New York. $2.00. 8 x $5\frac{1}{4}$; xii + 252, 1936.

This is not a faddist book. It is written to give the layman an accurate picture of what is known about foods and their properties at the present time and to show how to apply this knowledge to an intelligent diet. The most important points to be kept in mind are emphasized by reiteration in various chapters, as, for example, the great importance of milk in the diet. An appendix supplies diet lists for reducing, gaining, or maintaining weight; a table of 100 calory portions of the more common foods; a set of charts showing common foods as sources of vitamins and food value as to amount of protein, calcium, phosphorus, iron and copper; standard weight charts for age and height of men and women. There is an index.

PREVENTIVE MEDICINE. *Fifth Edition, Reset.*

By Mark F. Boyd. W. B. Saunders Co., Philadelphia. $4.50. $9\frac{1}{8}$ x $5\frac{3}{4}$; 561 + 1 folding map; 1936.

The revision of this fifth edition of a well known text includes changes or new material on colds, ringworm, psittacosis, diphtheria, encephalitis, poliomyelitis, pneumonia, tuberculosis, malaria, typhus and relapsing fever. Recent developments in sewage treatment are noted and sections are added dealing with mottled enamel, and with the rat from a public health standpoint. The sections on vitamins, silicosis and general morbidity incidence have been enlarged.

DIABÈTE ET CHIRURGIE.

By H. Chabanier and C. Lobo-Onell, with the collaboration of E. Lelu. Masson et Cie, Paris. 22 francs. $7\frac{3}{4}$ x $5\frac{1}{4}$; xii + 168; 1936 (paper).

The presence of diabetes complicates the dangers of operative procedure, increases the liability to gangrene, septicemia and other infections, and adds the risks associated with coma and precoma states. The authors emphasize these dangers and outline their pre-operative treatment devised to reduce to a minimum the operative risk and post-operative toxic accidents in this group of patients. The book is chiefly of interest to surgeons.

S. O. S. Pour la Défense de la Race.
By Arthur Vernes. Préface by Alexis Carrel. Librairie Maloine, Paris. 6½ x 5; 62 + 1 folding chart; 1935 (paper).
Dr. Vernes is intent upon scaring us to death with his hair-raising story of the insidious enemy on the way to bring our civilization to ruin, as he says. We agree that syphilis is a dreadful disease, and perhaps a bit of exaggeration about it isn't going to do any harm. May the good doctor have success in his venture!

Diseases and Parasites of Poultry. *U. S. Department of Agriculture Farmers' Bulletin No. 1652.*
By John S. Buckley, Hubert Bunyea and Eloise B. Cram. U. S. Department of Agriculture, Washington. 10 cents. 9¼ x 6; ii + 70; 1936 (paper).
The cause and nature of each disease is outlined, and what is known about prevention and treatment. In the case of parasites the life history of the parasitic animal is included.

BIOCHEMISTRY

Colloid Chemistry of Cellulosic Materials. *U. S. Department of Agriculture Miscellaneous Publication No. 240.*
By Alfred J. Stamm. U. S. Government Printing Office, Washington. 10 cents. 9⅛ x 6; 91: 1936 (paper).
The author correlates all the information and hitherto isolated facts contained in what he feels to be the most important papers so far published in this field. The appendix includes the following tables: (1) Refractive indexes and double refractions of cellulose fibres, (2) fibre contraction, (3) dimensions of unit cell of cellulose, (4) time for complete development of cellulose fibres, (5) size and structure of units of wood, (6) pore radii of membranes, (7) tensile strength of various fibres, (8) particle size, molecular weight, and molecular dimensions of cellulose, (9) electrical conductivity, (10) potential of cellulosic materials against distilled water, (11) sorption of gases by cellulosic materials, (12) absorption of polar gases and vapors by cellulosic materials, (13) various equations and symbols. The bibliography lists 280 papers.

Le Venin des Araignées. *Monographies de l'Institut Pasteur.*
By J. Vellard. Masson et Cie, Paris. 45 francs. 10 x 6½; 311; 1936 (paper).
The author has done extensive work over a period of years at the Antivenom Institute of Butantan, São Paulo, on the physiological and chemical properties of spider venom. Naturally, in this book, the arachnids of South America receive the greatest consideration and space, although species of other parts of the world are discussed to some extent—especially those which are dangerous to man. The number of species whose bite is fatal or seriously dangerous is, the author claims, relatively small. Incidentally, he corroborates the statements that the toxicity of the Black Widow (*Latrodextus mactans*) which recently invaded parts of the United States has been greatly over-rated.

The treatment and vaccine sera listed and discussed in the final section are specific for Brazilian species. A bibliography of over 200 titles is included but there is no index.

Gefässerweiternde Stoffe der Gewebe.
By J. H. Gaddum. Introduction by H. H. Dale. Georg Thieme, Leipzig. RM. 18 (paper); RM. 20 (cloth). 9¼ x 6½; xii + 200; 1936.
This is a complete and comprehensive survey of all the recent work done in the branch of pharmacology which deals with the effect on vascular dilation of tissue substances. The author deals at great length with the physiological effect on the vascular system of histamine, azetylcholin, and adenosin. There is a section on tissue extracts of unknown constitution, dealing with the effect of extracts of mammalian blood on the various organs, and extracts from other organs, such as heart, liver, spleen, pancreas, intestine, brain, gonads, eyes, urine, etc. The book is translated from the English. It contains 28 pages of bibliography and an

index. A useful and valuable reference book.

LA CHIMIE DES FERMENTATIONS.

By Marc H. Van Laer. Masson et Cie, Paris. 75 francs. $10\frac{1}{8} \times 6\frac{5}{8}$; 350; 1936 (paper).

This is a good systematic treatise presenting the fundamental facts and theories concerning the properties and behavior of the enzymes, sugars, amino acids, bacterial metabolism, etc., that the French technical chemist should know for a fuller understanding of the phenomena of fermentation. The bibliography seems to us inadequate (21 titles), and a detailed table of contents substitutes for an index.

BIOCHEMICAL AND ALLIED RESEARCH IN INDIA IN 1935.

Society of Biological Chemists, Bangalore, India. $8\frac{1}{2} \times 5\frac{1}{2}$; 130; 1936 (paper).

SEX

THE FUTURE OF MARRIAGE IN WESTERN CIVILISATION.

By Edward Westermarck. Macmillan Company, New York. $2.50. $8\frac{1}{4} \times 5\frac{3}{4}$; xiv + 281; 1936.

This eminent authority on the history of human marriage takes issue with the proponents of the theory that in the occidental countries the institution of marriage is facing a crisis, bankruptcy and collapse, and that free love will take its place. He briefly restates his viewpoints regarding the essential elements of marriage and then proceeds to discuss those factors which are regarded as important causes of marital unhappiness. In this respect, he utilizes well-known observations regarding sexual maladjustment, adultery and jealousy, social and economic difficulties, the ages of the partners, desire for children and so on. These observations show that increasing knowledge, forethought and self-control would increase happiness in marriage. The author also outlines the practical consequences of trial, companionate and temporary marriage, concubinage and free love, and gives a number of reasons to show that these forms cannot become universal, at least under the present social and legal regulations. In conclusion, Westermarck expresses the belief that the institution of marriage will persist because

> the unity of sensual and spiritual elements in sexual love, leading to a more or less durable community of life in a common home, and the desire for and love of off-spring, are factors which will remain lasting obstacles to the extinction of marriage and the collapse of the family, because they are too deeply rooted in human nature to fade away, and can find adequate satisfaction only in some form of marriage and the family founded upon it.

This book also includes chapters dealing with such topics as monogamy and polygamy, divorce and sexual behaviour and morality. This last chapter is expecially praiseworthy and justifies the author's great reputation. While the facts and the theories presented in this volume have already been expressed by the author himself and by others, the clarity of style and the able summary of the facts places this book among the first on the required reading list of all cultured persons.

CHANGE OF LIFE IN MEN AND WOMEN.

By Marie C. Stopes. G. P. Putnam's Sons, New York. $2.00. $7\frac{1}{2} \times 5\frac{1}{4}$; x + 239; 1936.

Once again Dr. Stopes attempts to enlighten us on a perplexing subject. In this treatise she denounces numerous books already written upon the menopause because she says they emphasize in too lurid pictures the alleged inevitable miseries that women are to anticipate at this "crisis." But unfortunately for the scientific force of the *opus* she quotes with manifest approval such hot passages as accord with her own pet prejudices. Thus we are told about the "sensual puberty" of the climacteric that is alleged to create "a fresh spring-time, and bring about a second youth." My eye—or rather our collective and bilious editorial eye!

It is urged throughout that there is a climacteric in the male. In any precise scientific sense this is not true. Progressive senescence in the male involves the

reproductive organs as well as all others, but there is biologically nothing in the male that is justly comparable with the sharp cessation of ovulation at the menopause of the female.

In conclusion, it is stated, justly enough, that the change of life may be and should be no more a "crisis" than is weaning or adolescence, and that, with proper care and understanding, lives after the change, "should be better balanced and capable of even richer and fuller experience than ever before."

L'ESTHÉTIQUE MAMMAIRE À TRAVERS L'HISTOIRE.

By C. Claoué and I. Bernard. Librairie Maloine, Paris. 10 x $6\frac{1}{2}$; 24; 1936 (paper).

An extremely brief history of the esthetic concepts of the human female breast as depicted in painting and sculpture from prehistoric times to the present. The authors assert (as any salesgirl or designer of women's clothes knows) that there is now a swing-back from the boyish, immature type idealized in the post-war period to the more classical plumpness. The pamphlet is illustrated.

BIOMETRY

STATISTICAL METHODS IN BIOLOGY, MEDICINE AND PSYCHOLOGY. *Fourth Edition, Completely Revised.*

By C. B. Davenport and Merle P. Ekas. John Wiley and Sons, New York; Chapman and Hall, London. \$2.75. 7 x $4\frac{3}{8}$; xii + 216; 1936.

This edition of a hand book which has had great popularity ever since its first publication in 1899 remains essentially the same as the previous editions, except that a number of the symbols have been changed to conform to A. K. Kurtz and H. A. Edgerton's suggestions for a standardized usage.

Some pages have been added on the following subjects: analysis of variance, extension of the theory of small samples, multiple and partial correlation, and some applications of statistics to economics. Also a number of extra tables have been added.

STATISTICS FOR STUDENTS OF PSYCHOLOGY AND EDUCATION.

By Herbert Sorenson. McGraw-Hill Book Co., New York. \$3.50. 9 x $5\frac{3}{4}$; viii + 373; 1936.

This textbook is clearly written and comprehensive enough to give the student without mathematical training a fairly accurate knowledge of the elementary methods of statistical analysis. The subject matter includes the methods of calculating and discussion of the centering constants, measures of variability, normal curve, correlation coefficients, and probable errors. As is usual in most elementary textbooks, the limits of applicability of the correlation coefficients are not adequately emphasized. Numerous examples are given to illustrate the use and calculation of the constants. A selected list of textbooks is furnished and there is an appendix with tables of squares and square roots, areas and ordinates of the normal curve, and probability of a given deviation occurring in a normal distribution.

EXAMPLES IN FINITE DIFFERENCES, CALCULUS AND PROBABILITY. *Supplement to an Elementary Treatise on Actuarial Mathematics.*

By Harry Freeman. The University Press, Cambridge; Macmillan Company, New York. \$2.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; 86; 1936.

This is a supplement to the author's well-known textbook on actuarial mathematics. It contains 400 problems and their answers. In addition, there is a section containing helpful hints on the method of solving the problems. It is difficult to appraise the clearness of the questions and the accuracy of the answers without actually solving all the problems. A casual survey shows that in general they are interesting and stimulating. However, it seems that at times the author has taken more pains to make the questions difficult to understand rather than they intrinsically are. Notwithstanding, this is an indispensable addition to the student's library.

CALCUL DES PROBABILITÉS.

By J.-B. Pomey. Gauthier-Villars, Paris. 25 francs. $9\frac{3}{4}$ x $6\frac{1}{4}$; 85; 1936 (paper).

This outline of the theory of probability is designed especially for students of engineering. After an exposition of the elementary theorems, the author discusses the geometric interpretation of probabilities and proceeds to illustrate the theory with applications to problems in telephone traffic, kinetic theory of gases, Brownian movements, *et similia*. The order of presentation is not of the best from the pedagogic standpoint and it is evident that the book proposes to give only a very general idea of the subject.

PSYCHOLOGY AND BEHAVIOR

MECHANISTIC BIOLOGY AND ANIMAL BEHAVIOUR.

By Theodore H. Savory. Watts and Co., London. 7s. 6d. $7\frac{3}{4} \times 5\frac{1}{4}$; xv + 182; 1936.

In the words of the author, this book contains "a study of sub-intelligent behaviour in animals, and especially among invertebrates." The bulk of the volume is concerned with descriptions of reflexes and reflex actions, tropisms and tropistic behavior, and instinct. It then proceeds to show how habit and evolution are related to these actions that are interpreted as purely mechanistic. The points made are frequently illustrated from the author's study of spiders.

Although the author himself calls this a "study of sub-intelligent behaviour," he rather fails to give due weight to the fact that the term "sub-intelligent" action implies the existence of the "intelligent" action. His argument appears in the end to lead to the conclusion that the only behavior we really know anything about is that comprised within the pure reflexes and tropisms of the invertebrates, and that at best it is only a good working hypothesis that there may be intelligence involved in the behavior of higher animals—surely an odd conclusion for a primate to reach.

Finally, in spite of the fact that the preface gives the impression that breadth of view will be found in this study, we find, especially in the concluding summary, rather sweepingly dogmatic statements concerning the absurdity of vitalism and the incontrovertible rightness of the mechanist interpretation of vital phenomena. The book contains both index and bibliography.

THE PSYCHOLOGY OF FEELING AND EMOTION.

By Christian A. Ruckmick. McGraw-Hill Book Co., New York. $4.50. 9 x 6; xiii + 529; 1936.

A comprehensive textbook in a field of psychology where there has been a need for an up-to-date text. The treatment leans heavily on quotation from original sources, but is not particularly strong on the side of constructive, critical synthesis. It is of some interest to note that 44 pages are given over to "The Facial Expression of Emotion," whereas the work of Freud and all of his followers is disposed of in 14. The chapter on "The Rôle of Feeling and Emotion in Psychoanalysis" closes with the following paragraph:

> We might also question the assumption of subtlety on the part of so primitive and blind an entity as the id, or even the ego. The whole system, we recall, is a "feeling psychology." But the subterfuges that appear in hiding the real character of the libido are worthy of the astuteness and cleverness of a supreme order of intellect. How is it possible, then, to get the process of camouflaging started—a process that takes the acute mind and long practice of an analyst to unravel? If Freud has ever satisfactorily answered this fundamental question, the writer is unaware of it. To imply it in the general order of nature is probably too much for a present-day naturalist!

At the close of the volume the author gives his own views on education and culture. It occurs to us as possible that the work might have been better if he hadn't. There is an index, and at the end of each chapter a summary and a list of review questions. The wise and discreet teacher will be able to use this book effectively in classroom work.

PRINCIPLES OF TOPOLOGICAL PSYCHOLOGY.

By Kurt Lewin. Translated by Fritz Heider and Grace M. Heider. McGraw-Hill Book Co., New York. $2.50. 9 x 6; xv + 231; 1936.

The author of this book believes that "psychology needs concepts which can be

applied not merely to the facts of a single field like child psychology, animal psychology, or psychopathology, but which are equally applicable to all of them." He studied topology (the mathematics of space relationships) and made use of its concepts, "which soon appeared . . . particularly fitted to the specific problems of psychology." In this volume, therefore, he has attempted to develop a group of concepts that may be used as a framework for integrating all of the various branches of psychology. The goal of this method of attack is to discover laws that will enable the prediction of individual cases. These laws must consider the relationship between the individual and the situation, an impossibility with only our "averages" and "statistical characterizations."

Here we have a new idea, a new method in the field of psychology. The author is undoubtedly an idealist; but whether he is on the wrong track or whether he is the creator of a revolutionary point of view will in the fullness of time become clear. The book is interesting because of its novelty. The material, however, is presented in an extremely technical form. Both index and bibliography are included.

Anticipatory Responses in Serial Learning by Chimpanzee. *Comparative Psychology Monographs, Volume 13, Number 2.*

By S. D. S. Spragg. The Johns Hopkins Press, Baltimore. $1.25. 10 x 6¾; 72; 1936 (paper).

"Certain acts tend to anticipate the stimuli which previously initiated them, upon repetition of the situation in which the originally adequate stimuli occurred. Such responses are frequently designated 'anticipatory responses' . . . . As descriptive of organic behavior the term need make no assumptions regarding any foresight, expectancy, or prevision on the part of the organism. . . ." A series of stylus maze experiments, with five chimpanzees as subjects, demonstrated that extended training reduces all anticipatory responses. The author states that the cues for the solution of a maze problem when the extra-organic stimuli offered the animal at each choice point are presumably identical "may depend upon a summational effect from having made a certain number of choice responses of the same sort, or else upon some still more subtle type of cue which we tentatively designate as *symbolic*." The summational effect here referred to cannot be entirely disassociated from *foresight*, *expectancy*, or *prevision*, and it seems to us that "symbolic" is being used somewhat in the sense that others use "conditioned."

Studies in Infant Behavior III. *University of Iowa Studies in Child Welfare, Vol. 12, No. 1.*

By M. A. Wenger, Josephine M. Smith, Charles Hazard and Orvis C. Irwin. University of Iowa, Iowa City. $1.00 (paper); $1.35 (cloth). 9⅛ x 6⅛; 207; 1936.

This bulletin contains five reports which treat respectively of (1) conditioned responses in newborn infants; (2) brightness values of red, green and blue in the newborn; (3) skin resistance of infants; (4) age and reflex conduction rate in the patellar reflex; (5) changes in vertebral reactions pattern during infancy. Each of the papers demonstrate a careful experimental technique but in none are the results very definite since they are derived from too few observations. Of particular interest appear to be the first two papers. In one, by M. A. Wenger, it is asked whether or not newborn infants can be conditioned. The author's observations show that apparently in some infants this is possible although the reactions are unstable. In the second paper, J. W. Smith reports her experiments on the relative appreciation of color brightness by the newborn. From her findings it appears that the reactions of male infants correspond to those of adults with total color-blindness, while those of the females are similar to those of adults who do not see red. The author explains these factors by postulating a theory of ontogenetic development of color vision. An adequate bibliography accompanies each article.

STUDIES OF CEREBRAL FUNCTION IN PRIMATES. *Comparative Psychology Monographs, Vol. 13, No. 3. Serial No. 63.*

By C. F. Jacobsen, with the coöperation of J. H. Elder and G. M. Haslerud. Johns Hopkins Press, Baltimore. $1.25. 10 x 6¾; 68; 1936 (paper).

Monkeys were trained in two types of behavioral tests for (a) simple or immediate responses and visual discrimination and (b) delayed responses. Experimental lesions were then made in various areas of the cerebral cortex and the tests repeated. A summary of the results of these experiments show: (1) Unilateral lesions caused no impairment of performance on any tests. There was no evidence of hemispheral dominance. (2) Complete bilateral lesions of the frontal association areas caused loss of ability to perform delayed responses, and subtotal lesions caused a shortening of the time through which memory was effective. Memory for simple response habits and visual discrimination habits was not impaired either with partial or complete lesion, nor was the ability to learn new tasks of a similar nature reduced. (3) Lesions in other cortical areas produced slight, if any changes.

RESEARCH IN DEMENTIA PRECOX. *(Past Attainments, Present Trends and Future Possibilities.)*

By Nolan D. C. Lewis. The National Committee for Mental Hygiene, New York. $1.50. 9 x 6; xi + 320; 1936.

A general survey of the various aspects of psychiatric research with their particular applications to the "dementia precox" problem. The author, professor of neurology in Columbia University and an associate of various research institutions, has produced a dependable book. No attempt is made to present a thoroughgoing critical analysis of achievements that have been made, but some general principles are offered as well as a few specific leads for future procedure. Repeatedly does Dr. Lewis emphasize the need for greater precision in methods and for coördination of what is known and discovered. Each chapter is thoroughly documented, but the usefulness of the volume is impaired by the lack of an index.

ABNORMAL PERSONALITY AND TIME.

By Nathan Israeli. With a Foreword by Irving J. Sands. Introduction by Gardner Murphy. Science Press Printing Co., Lancaster. $2.50. 9 x 6; 123; 1936.

The author believes that in diagnosis of psychosis great benefit is derived from the observation of the patient's future outlook. He introduces a method of evaluating this by having the patients write future autobiographies. Examples are given of future autobiographies written by mental patients and adolescents with high I.Q.s. The technique is interesting and once the analysis of results can be based on precise definitions it should permit some important contributions to one aspect of human biology which has not received adequate treatment. There is a fairly complete bibliography.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

PHILOSOPHY AND THE CONCEPTS OF MODERN SCIENCE.

By Oliver L. Reiser. The Macmillan Company, New York. $3.50. 8½ x 5¾; xvii + 323. 1935.

This scholarly work is an apology for humanism. As few terms have been more abused than this the author feels constrained to explain that he uses it in the sense originated by the philosophers of the reformation. Yet there is a great difference between the humanism of Erasmus and that of his Italian contemporaries, and it is inevitable that a philosophy based on the concepts of modern science should differ widely from both, since during the past few centuries the changes in our scientific concepts have been as great as in our apparel.

The first part of the book is devoted to a discussion of the physical sciences, and indicates a thorough familiarity with the works of such modern writers as Korzybski, Schroedinger, Bertrand Russell, Heisenberg, Einstein, Eddington, Whitehead, etc. From their writings Reiser draws heavily, and to their conclusions adds many of his own. Perhaps the most startling of these is his synthesis of relativity and absolutivity of motion. Growth he believes to be a form of motion which

is not relative, since it is always in the direction of the increasing complexity of parts. (But how about the growth of *Sacculina* or *Entoconcha*, or the pupation period of the Diptera?) Also, since the theory of relativity in its most general form involves the curvature of space in a fourth dimension, to make room for absolute motion we need only postulate a fifth dimension in which the laws of relativity do not hold. This fifth dimension he finds in consciousness. But since Einstein has postulated ten dimensions, and as many as 137 have been credited to Eddington, it is difficult to see how Dr. Reiser finds a paltry five adequate for his philosophy.

The second part of the book deals with the social sciences. It is about half the length of the first part, not because the social sciences are less significant, but because being younger their contribution to human learning does not as yet bulk so large. The climax of this part comes with the author's identification of emergent evolution with *Gestalt* psychology, on the ground that in each case the whole is greater than the sum of its component parts.

There is an index of six pages, and the book is completely documented throughout. It is made hard reading by the use of unusual words, such as fixate, labile, appetition, nisus, which are not found in ordinary desk dictionaries.

The Great Chain of Being. *A Study of the History of an Idea. The William James Lectures Delivered at Harvard University, 1933.*

By Arthur O. Lovejoy. Harvard University Press, Cambridge. $4.00. $8\frac{3}{4}$ x 6; xii + 382; 1936.

This book is based on the second series of the William James Lectures on Philosophy at Harvard University which were given by Professor Lovejoy. He has, however, expanded these lectures somewhat, chiefly "by the addition of more citations of illustrative passages." The lectures trace the history of the idea of "being" from Plato through various periods of thought, such as mediaeval, eighteenth century, and Romantic, to contemporary thought. The author has attempted to show how this idea influenced not only the field of philosophy, but also other fields of thought such as religion, literature, and biology. And it is here that the "citations of illustrative passages" add interest as well as enlightenment. In the first lecture, the author says that the "Great Chain of Being" is a concept consisting of three ideas. He makes no statement as to just what those ideas are, but seems to agree with Leibniz for whom "the essential characteristics of the universe are . . . plentitude, continuity, and linear gradation."

Professor Lovejoy has not expounded his own nor any other single philosophy. His material has been drawn from many sources and, in his own words, the resultant study "aims at interpretation and unification and seeks to correlate things which often are not on the surface connected." The lectures were designed for a "mixed academic audience" rather than for specialists and the phrasing is, possibly for that very reason, remarkably uninvolved. The book contains no bibliography but there are notes (in an appendix) and an index.

The Retreat from Reason. *Conway Memorial Lecture delivered at Conway Hall May 20, 1936. Introduction by Julian Huxley.*

By Lancelot Hogben. Watts and Co., London. One shilling net. $6\frac{1}{2}$ x 4; xi + 82; 1936 (paper).

In this book Professor Hogben plumbs the depths of pessimism. That all is not well with contemporary civilization no sane person will deny, but few will follow him so far as to assert that it is without any salvage value whatsoever. To be sure, Professor Hogben does not state this in so many words, but anyone who wades through his lengthy Jeremiad is likely to get the impression that he believes it. His vituperations are directed chiefly against modern education. The function of education he believes to be the acquisition, dissemination, and application of knowledge and the greatest of these is application—"knowledge for use" he calls

it. Yet in actual practice we find that we are acquiring and disseminating knowledge faster than we learn to apply it, with the result that positions of political responsibility are everywhere occupied by persons who know everything about how a state should be governed and nothing about how to govern one. The countries of southern and central Europe afford Professor Hogben much material in support of his theory, but he is not unmindful of the situation in England, which is not above criticism, and the vigor with which he attacks his own as well as the adjacent countries is like that of the shepherd of Tekoa. Being a scholar, he has been able to assemble an array of facts and instances which he cites in support of his statements, which makes them very convincing, and very discouraging as well.

Jen Sheng: *The Root of Life.*

By Mikhail Prishvin. English Version by George Walton and Philip Gibbons. Foreword by Julian S. Huxley. Andrew Melrose, London. 5s. net. $7\frac{1}{4} \times 4\frac{5}{8}$; 157; 1936.

A man sick to death of war, leaves the battlefield strewn with dead and wanders into the wilderness to find a new soul. He meets Lu Wen, a Chinese philosopher who shows him the Root of Life, but he must wait many years until it develops before he may possess it. In the meantime he grows, conquering his hunting instinct when he looks into the beautiful flower eyes of Khu-a-lu the doe. Then comes the woman who is the very incarnation of the lovely Khu-a-lu, but she returns whence she came and he turns to the domestication of the almost extinct spotted deer.

If the usual sentimental nature book with animals thinking and speaking human emotions bores you, you will be delighted with Jen Sheng. Mikhail Prishvin has been compared with Grey Owl, but they are alike only in their deep understanding of wild animals, for Prishvin, with Russian seriousness finds a real philosophy in his experiences. He has given a vivid picture of the wilderness and beauty of Manchurian Russia, and a very interesting study of the life cycle of the spotted deer, which he sees with deep feeling and rare sympathy.

Jen Sheng is a small book, but with subtle simplicity sums up the very real philosophy of Soviet Russia's idealism, that "the power of the root of life lay not in its substance which also would soon be swept away, but in relation to cosmic time, and even to time which was still more vast."

Teaching Methods in Medicine. *The Application of the Philosophy of Contemporary Education to Medical Schools.*

By William D. Reid. William D. Reid, 510 Commonwealth Ave., Boston. \$1.00. 9×6; 111; 1936.

The expression "a born teacher" or even "a good teacher" can be applied by and large to but few university instructors in whatever department one may search. This little book, by the assistant professor of cardiology in the Boston University School of Medicine, makes a plea for the training of teachers, as such, of medical students. The average medical teacher is selected for a superior knowledge of his subject while little or no regard is given to his understanding of the methods of teaching. The author briefly discusses the psychology of learning; major, minor and general technics of teaching; and the application of these technics to medical education. He also gives a section on "selected topics" which are offered as samples in the application of the technics of teaching to medical subjects. Lists of suggested readings are given at the end of the chapters and the volume is indexed.

Transformisme et Hérédité. *Les Organismes et le Milieu Terrestre.*

By César Porto. Librairie Bertrand, Lisbon. 15 francs. $9 \times 5\frac{1}{2}$; viii + 328 + 4 folding charts; 1936 (paper).

L'Instinct. *Ses Causes Physiques, sa Base Organique et sa Psychology.*

By César Porto. José Corti, Paris. 15 francs. $9 \times 5\frac{1}{2}$; 288; 1935 (paper).

The author, former director of the technical schools at Lisbon, attempts to apply concepts of metaphysics and astrol-

ogy to, in the first case, evolution and heredity, and in the second, instinct. The second book is curiously enough a continuation of the first. There is much talk of cosmic rays, magnetic forces, etc., and even psychological factors, but the theories evolved and the hypotheses upon which they are based are not always clearly defined. The author apparently belongs to that group which either assumes that astrology has already been placed upon a scientific basis, or is trying to put it there. It is evident also that French is not his native language.

Neither book has a bibliography (indeed, a careful perusal revealed no reference to *any* other author) or index, but we opine they will not be greatly missed.

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THE QUARTERLY REVIEW of BIOLOGY

STUDIES ON THE ORGANIZATION OF THE GIANT GLAND CHROMOSOMES OF DIPTERA

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IT HAS been evident since the recent discovery of their true significance (Heitz and Bauer 1933, Painter 1933) that the giant salivary gland chromosomes of Diptera offer an exceptional opportunity for detailed studies on chromosome structure, particularly in relation to genes. The importance of the problems and the favorability of the material have attracted the attention of numerous investigators, with the result that various hypotheses or interpretations are now current as to the organization of these chromosomes. These interpretations are of four distinctly different types—a fact which serves both to indicate the difficulty of interpreting the evidence and to suggest the need of caution in accepting any of the hypotheses. Since some of the interpretations are being used, nevertheless, as a basis for considerations on such topics as the nature of genes (Muller and Prokovieva 1935, Wrinch 1935) it seems desirable to subject both the evidence and the interpretations to critical examination as they are presented.

One purpose of the present paper is to do this. More specifically, the purpose is to examine the nature of the evidence which has led to the view that the giant chromosome is composed of numerous discrete threads or chromonemata *which are visible as such* in the preparations, and that the chromatic "granules" in the chromosome represent individual genes or chromomeres. Since this paper was sent to press it has been possible to include some brief comments on recent papers and some evidence derived from *Chironomus* by one of us (Metz).

The most conspicuous characteristic of the giant chromosome is its relatively enormous size. The next most conspicuous feature is the organization into transverse chromatic discs or "bands" separated from one another by achromatic interspaces (Fig. 1).

Since the immediate problem concerning these chromosomes is that of interpreting them and their visible constituents in terms of "ordinary" chromosomes and their constituents, the question imme-

diately arises as to how the giant chromosomes attain their great size and what the chromatic discs represent. The size may be accounted for through growth alone, or through a combination of growth and multiplication ("division") of component parts. Particular attention will be given to these possibilities. The series of chromatic discs immediately suggests the series of "chromioles" or "chromomeres" in the uncoiled chromonema of an ordinary chromosome; but here again there is an enormous difference in size and the same possibilities of growth or multiplication suggest themselves as alternative explanations of the difference.

In considering the latter problem attention has been given by all investigators to the fact that many of the discs are granular in nature and may look like transverse rows of granules when the chromosome is viewed from the side. This characteristic has led to the suggestion (Koltzoff 1934, Bridges 1935) that each disc is compound and that each granule represents a gene or chromomere. On this view the chromosome is considered as having arisen through multiplication of the original chromonema and its constituent parts (genes, chromomeres) and the number of divisions of the chromonema is supposed to be represented by

FIG. 1. PHOTOGRAPH OF THE X CHROMOSOME (PAIR) IN A SALIVARY GLAND CELL OF SCIARA OCELLARIS COMST. From an aceto-carmine preparation ×1710. If this chromosome were derived by simple multiplication of visible chromonemata it should look like those shown in Fig 2.

FIG. 2. PHOTOGRAPH OF A MULTIPLE CHROMOSOME COMPLEX IN CULEX PIPIENS, (BY MR C. A. BERGER) Prophase, from an aceto-carmine preparation. Magnification same as Fig 1. The group includes three aggregates, corresponding to the three V-shaped pairs of chromosomes of this species. Each aggregate represents 8 (or 16?) closely associated chromonemata derived by multiplication of the original chromonemata of the pair. ×1710.

the number of granules in a disc. It is stated or implied on this view that the chromonema and its constituents possess definite size limits and that growth automatically results in duplication.

Thus the giant chromosome is regarded, on this view, as a cable made up of thread-like chromonemata on which the granules are arranged like beads. Two types of interpretation have been made on this basis, as will be noted later, but they need not be distinguished at this point. Since the essential feature of each is apparently the assumption that the constituents under consideration have definite size limits, and that the constituents visible in the giant chromosome are comparable to those in the chromonema of an ordinary cell (Koltzoff 1934, Bauer 1935), attention may first be given to the question of size relationships.

In considering these it should be recalled that in the gland cells under consideration the chromosomes are associated intimately in pairs, so what is called a "chromosome" is ordinarily a pair of homologues fused side by side. It should also be recalled that the giant chromosome is not comparable in condition to the condensed metaphase chromosome of an ordinary cell, but to the elongated chromosome or chromonema of the interphase or early prophase.

SIZE RELATIONSHIPS

In *Sciara*, as shown previously (Metz 1935a) the size relations between an ordinary chromosome of an embryonic cell and one of the giant chromosomes of a salivary gland cell are apparently on the order of 1:1000 or more in volume. It has not been possible to make exact measurements, but the following observations on *Sciara ocellaris* Comst. serve to indicate roughly the comparative sizes of the structures under consideration.

Our observations indicate that the diameter of a single giant chromosome (pair) may be greater than the diameter of an entire resting nucleus of an ordinary embryonic cell in the same larva. The volume of such a chromosome appears to be more than 100 times that of such a nucleus. A single chromonema in an ordinary embryonic cell is too minute and delicate to measure, but since its bulk is much less than that of the metaphase chromosome, which contains a considerable amount of "matrix" material, and probably two chromonemata, it seems probable that a giant chromosome is more than 1000 times as large as the chromonema, in volume.

If, therefore, the giant chromosome represents a bundle or cable of ordinary chromonemata it would be expected that the number of chromonemata would approximate that just mentioned. Since the number would be indicated by the number of granules in a disc, on the view under consideration, the granules should be present in corresponding numbers. In *Sciara*, however, the number of granules ranges from 5 or 6 up to possibly 30 or 40.

Similarly, great discrepancies are met when consideration is given to the size of the granules and to the length of the giant chromosomes. According to our observations the size of many of the granules in the giant chromosomes is many times as large as that of anything which could be considered a unit chromomere in the chromonema of an ordinary cell. Likewise, the length of the giant chromosome appears to be at least several times that of an ordinary chromonema. On the view under consideration a chromonema should include one granule from each disc, together with intervening achromatic material. It is obvious, however, that in volume the latter exceeds the former many times. It seems safe to say that in some cases a single granule exceeds the entire ordinary chromonema in bulk, yet the number of granules in a hypothetical chromonema in a giant chromosome, as represented by the number of discs in such a chromosome, is expressed in hundreds.

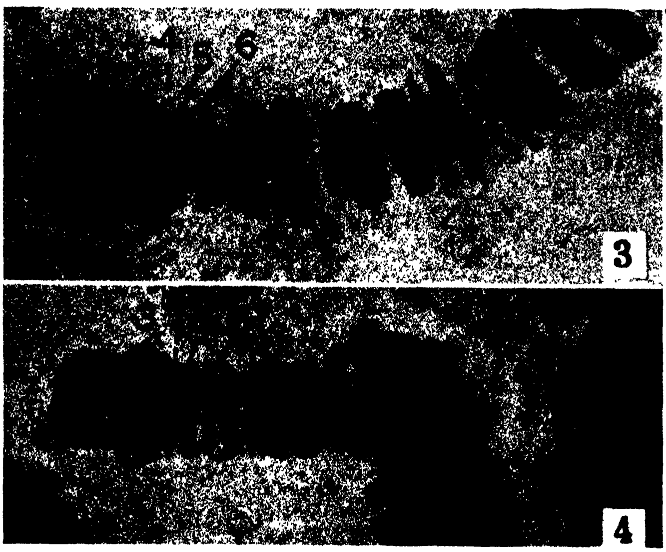
As noted previously (Metz 1937 and earlier papers) such considerations indicate either that the components of a chromonema do not have fixed size limits corresponding to those seen in an ordinary cell, or else that the visible structures under consideration (granules) are not chromomeres representing individual units.

COMPARISON WITH AN ACTUAL CASE OF COMPOUND CHROMONEMATA

Another line of evidence is even more significant in this connection. This is provided by the multiple chromosome complexes in the iliac cells of *Culex* larvae, as shown by Mr. C. A. Berger (1936, 1937 and

unpublished) in this laboratory. Here the chromosomes undergo repeated divisions while in the resting stage, and produce compound structures which in early prophase represent just what is postulated on the view considered above. The four, eight or sixteen chromonemata are associated side by side in the form of a cable in prophase, and later separate to make definite chromosomes which go through mitosis.

In Fig. 2 is shown a photograph of such a prophase figure, kindly provided by Mr. Berger. Each apparent chromosome in this figure represents 8 (possibly 16) chromonemata intimately associated side by side. For comparison a giant chromosome (the X) in *Sciara ocellaris* is shown at the same magnification in chromonema in a *Culex* composite chromosome (i.e. one eighth of one of the composites in Fig. 2) should be equivalent in size to a structure made up of one granule from each of the discs in the giant chromosome, plus connecting achromatic material. It is obvious, however, that no such relation exists. The latter structure would be many times as large as the single chromonema. The same conclusion is reached if a giant chromosome of *Drosophila* or *Chironomus*, rather than of *Sciara*, is used for comparison, although conditions differ somewhat in detail in the different genera. In *Chironomus*, for example, there is a finer and more delicate internal organization, with smaller and more numerous granules in the discs.



FIGS. 3 AND 4. PHOTOGRAPHS OF END 1 OF CHROMOSOME A OF SCIARA OCELLARIS (VAR. B), FROM DIFFERENT CELL OF ONE SALIVARY GLAND, TO SHOW VARIABILITY IN APPEARANCE OF INDIVIDUAL DISCS

From an aceto-carmine preparation. ×3000. Compare especially disc 6, which appears as a single band or row of granules in Fig. 3 and as two bands or rows of granules in Fig. 4 (see Metz, 1937). The numbering of the discs here corresponds to that in previous figures of this chromosome (Metz, 1935a, Fig. 5B, 5D).

Fig. 1. The ordinary chromosomes in *Culex* are larger than those of *Sciara*, so the contrast is not due to a basic difference in size in favor of the *Sciara* chromosomes.

Three features call for special comment in connection with this comparison.

(1) If the giant chromosome of *Sciara* were made up of 10 to 20 chromonemata (which would be the maximum number expected if each granule of a disc identifies a chromonema because most discs do not have more than that many granules) such a chromosome should be approximately twice the size of one of the composite structures in *Culex*. Actually, however, it is hundreds of times as large.

(2) On the hypothesis under consideration one

(3) If the giant chromosome is a multiple of the type under consideration it should be approximately the length of an ordinary chromonema, which would presumably be not greatly different from that of one of the early prophase composites of a comparable chromosome in *Culex*. In the *Culex* group (Fig. 2) there are three composites, each representing a pair of relatively long, V-shaped chromosomes multiplied. Each such chromosome is comparable to the V-shaped chromosome in *Sciara ocellaris*, which in the giant cells is approximately twice the length of the chromosome shown in Fig. 1. Comparison on this basis indicates that the giant chromosome is from seven to ten times as long as the comparable prophase composite in *Culex*.

It has not been possible to demonstrate that an ordinary fully extended chromonema in *Sciara* or *Culex* is not seven to ten times as long as it appears in prophase, for in the resting stage direct measurements cannot be made. Apparently, however, in *Culex* the earliest prophase composites which are suitable for study contain extended chromonemata, and these are not greatly different in length from those shown in Fig. 2. It seems probable, therefore, that an extended chromonema is not more than twice the length of these composites, or more than one fifth to one third the length of a comparable giant chromosome.

This fact in turn makes it probable, as inferred from other evidence also, that the structure which gave rise to the giant chromosome has undergone a great increase in length, through growth, irrespective of how the increase in diameter has been brought about. If that is true, the size limits of the ordinary chromonema are not fixed or even approximately fixed as would be expected on the hypothesis under consideration. If such an increase in length can be effected there seems no obvious reason why a corresponding increase in diameter cannot be brought about by growth without division.

If objection is made to this interpretation, and an ordinary chromonema is considered equal in length to a giant chromosome, serious difficulties of the type considered under item (2) are encountered, for this would require such attenuation as to make the chromonema practically invisible. The result may be visualized by considering one eighth or one sixteenth of one of the composites shown in Fig. 2, extended to twice the length of the giant chromosome shown in Fig. 1.

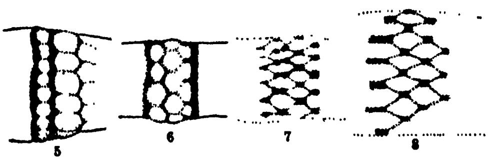
In the light of such evidence we can only conclude that as it stands the hypothesis under consideration is inapplicable to the giant chromosomes. It may next be inquired whether or not any modification of it is applicable. As originally put forward by Koltzoff and by Bridges it assumed that the chromonemata were, in *Drosophila*, usually 16 or 32 in number and arranged spirally around the periphery of the chromosome (pair), which was said to have a central axis. This view appears definitely to be ruled out by the findings of Metz and Gay (1934a, b), Koller (1935), Bauer (1935, 1936) and Metz (1935a, b) that no differentiated axis is present and that the general structure is uniform through the chromosome transversely at any one level (Fig. 16). The conception of visible multiple structure, however, is accepted by Koller and by Bauer, who consider the giant chromosome a uniform bundle or cable with the chromonemata extending essentially straight, instead of a hollow structure with threads extending spirally. It is interesting, in this connection, to note that this latter interpretation is based largely on study of the same material (*Drosophila* and *Chironomus*) used by Koltzoff and by Bridges in arriving at the former interpretation. This serves to show how difficult it is to interpret the evidence.

As intimated above, the visible structures in the giant chromosomes all appear to be much too large to represent unit parts of ordinary chromonemata. They can only be interpreted on this basis if it is assumed that the original units have undergone enlargement far beyond the limits reached in ordinary embryonic cells. If this assumption is made, and the original hypothesis modified accordingly, the giant chromosome might be looked upon as a structure in which the ordinary relationships between growth and division have been upset, and either the original chromonema undergoes a few divisions, followed by excessive growth without division, or else the chromonemata and their constituents divide only after they reach a much greater size than usual. Since such a view (apparently held by Painter and Griffen,

1936) abandons the conception of size limits, however, it has little theoretical advantage over the assumption that the enlargement is a result of growth alone, for the difference is only one of degree (see Metz 1937). Before examining the evidence for and against the assumption a third alternative may be considered.

It may be suggested that the assumption underlying the first mentioned hypothesis is correct and that the giant chromosome is a multiple in which the individual chromonemata are so numerous and small as to be invisible individually, but that they are associated in groups or clusters which are visible. On a view of this type example of such variation is shown in the accompanying Fig. 20. It might be maintained that large granules in small number represent higher but definite multiples, but we have been unable to find any evidence for such a view. In the case of some discs a small number of small granules is found.

It would similarly be expected on the view under consideration that the number of granules would be multiples of two; whereas, as indicated previously the number appears to be variable and not consistent in this respect. Of greater significance than either of these lines of evidence, however, are two others. One is the great variability in the size, number and general characteristics of the granules of a given disc in different cells, even in comparable chromosomes of the same gland. This feature has been considered somewhat in an earlier paper (Metz 1935b) and will receive further treatment elsewhere. The other line of evidence is that which shows that the materials of



Figs. 5–8. Free Hand Drawings of Selected Regions in Salivary Gland Chromosomes of Sciara ocellaris Comst.

5, 6, 8, from aceto-carmine preparations; 7, from an osmic-aceto-carmine preparation. Only one optical level is shown in each case.

5, an example illustrating the relationship between the achromatic droplets and the surrounding chromatin – the latter confined mainly to the discs. Also illustrates difference in number of droplets at different loci. 6, example of a region in which the chromatic material is more evenly distributed around the droplets, giving a definitely honeycomb type of structure. (Compare with region d in photograph in Fig. 13, and with diagram in Fig. 10.) 7 and 8, moderately stretched regions of the type in which the chromatic material is mostly localized in block-like granules connected by delicate lines, giving the appearance of a network. The evidence indicates that in such cases each granule is made up of material from two discs, as may be seen by comparing with Fig. 6.

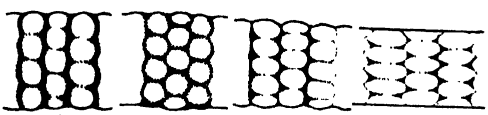
the granules would be compound, each made up of numerous invisible units (genes or chromomeres). Each granule could be considered a definite multiple, and hence a unit of higher order, or it could be considered as of no individual significance.

THE NATURE OF THE "GRANULES"

These considerations serve to focus attention directly on the individual granules and on any structures which might represent chromonemata, either single or compound. If the granules represent chromomeres, and serve to identify chromonemata, in either sense, it would be expected that the number would be uniform in the discs of one chromosome. But, as pointed out previously (see especially Metz 1935b), there is a wide difference in this respect in our material. An a "disc" may appear in the form of a single row of granules in one cell and two rows of granules in another, even in the same gland. An illustration of such a case is shown in Figs. 3 and 4. These are photographs of end 1 of chromosome A in *Sciara ocellaris*, both from the same gland. In the former, "discs" numbered 3, 4, 5 and 6 all appear single—i.e. each looks like a band or a single row of granules. In the latter (Fig. 4), however, 5 and 6 both appear double; each is represented by two rows of granules. This is especially obvious in the case of disc 6, although under the microscope it is clear also in that of disc 5. The difference between the conditions in the two chromosomes is apparently due to a different distribution of chromatic and achromatic materials in the two, as described elsewhere (Metz 1937). An accumulation of achromatic substance within the material of the "disc" gives it an inflated appearance, and under this condition the two faces look like separate discs, each seen as a row of granules in edge

view. The evidence is strong that a rearrangement of chromatic material is involved here, and that the granules in the one case bear no relation to those in the other. (It is, of course, not certain that any "disc" here is really single.)

This leads to consideration of another aspect of structure and to the question of normal variability. These can only be treated briefly at this time, but the general features may be summarized as follows. For the most part this summary represents an outline of the interpretation advanced in previous papers by Metz and Gay and by Metz (cited above).



FIGS. 9-12. DIAGRAMS ILLUSTRATING VARIOUS TYPES OF DISTRIBUTION OF CHROMATIC MATERIAL AROUND THE ACHROMATIC DROPLETS

In 9 the chromatin is mainly confined to the discs, with relatively little extending from one disc to the next between the droplets. In 10 more of the chromatin extends between the droplets, giving a honeycomb pattern. With still further accumulation between the droplets, and less in the discs, the pattern shown in Figs. 7 and 8 appears. In 11 and 12 the chromatin is confined largely to the regions of the discs, but tends to form granules in the interstices between the droplets. The discs or transverse rows of granules may be zig-zag, as shown here, or in the absence of longitudinal tension may be straight.

RELATIONSHIP BETWEEN CHROMATIC AND ACHROMATIC MATERIALS

1. In all the forms we have studied (*Sciara*, *Chironomus*, *Drosophila*) it seems clear that the achromatic material and the chromatic material in the giant chromosome are organized differently. The former is in the form of droplets, at least in the fixed chromosome, and hence is in this sense discontinuous. The latter, on the other hand, does not appear to be in this form, but rather to be essentially continuous.

2. Each two successive chromatic discs are separated by a layer or plate of achromatic droplets as indicated in Figs. 5, 6, 9–12, 13, 14, 17 and 18.

3. Although confined largely to the discs, the chromatic material also extends between and around the achromatic droplets separating one disc from the next. The degree of dispersion of the chromatic material varies widely. In some cases the discs are sharp and clear cut, with relatively little chromatic material around the intervening droplets (Fig. 15); in others much of the chromatic material is in the latter position (Figs. 5, 6, 14, 17, 18). At first it was thought that the sharply "banded" condition was normal and the condition of wide dispersal of chromatin, or "vesiculated" condition, was abnormal and due to injury or poor fixation; but the evidence now indicates that both conditions are normal and may even be reversible (Metz 1937).

4. In addition to the variation in appearance due to differences in dispersion of chromatic material there is considerable variation due apparently to differences in compression or tension within the chromosome. If the achromatic droplets are not tightly pressed together they appear rounded as indicated in Figs. 5 (at left) 9, 10, 18, and at *a* in 14 and 17). If, however, they are tightly pressed together, or slightly stretched, their interfaces become flattened and their appearance hexagonal as indicated in Figs. 5, 6, 11, 12, and 13 (at *d*).

5. The general structure, then, is essentially alveolar or honeycomb-like, modified by the stratification due to the discs (see papers of Metz and Gay, and of Metz cited above). In many cases such organization is clearly visible and may be photographed (Figs. 13, 14, 17, 18).

6. The numbers and sizes of the achromatic droplets differ in different regions—presumably reflecting qualitative differences in the materials (Fig. 5). Also there is considerable variation in a given region of a given chromosome in different cells, even when the chromosomes are of comparable size and in the same gland. Each region, however, exhibits its own characteristic tendencies. The variation is not purely a random one.

7. Since a plate of achromatic droplets separates two adjacent chromatic discs, the interfaces between the compressed droplets, together with more or less chromatic material, serve to form short longitudinal lines connecting the two discs, as indicated in Figs. 5, 11, 12, 13 (at *d*) 14, 17 and 18. These lines may be very conspicuous or very faint, depending on the amount of chromatic material present, as indicated above. In the "vesiculated" condition (Fig. 18) they are relatively conspicuous. It is especially significant that in the undistorted chromosome these short lines, for the most part, are not themselves aligned to give the appearance of continuous threads,

Figs. 13–20. Photographs of Selected Regions in Salivary Gland Chromosomes of Sciara ocellaris Comst, except 18 and 19 which are from Sciara coprophila Lint.

All are from smear preparations. Details are discussed in the text. Photographs made with 3 mm. oil immersion objective, 1.4 aperture, and 10X or 15X ocular. Original magnification in most cases X1500. Present magnification X3000 except in the case of Figs. 15, 17 and 18, as noted below. 13, right end of chro-

but are staggered. Those between two discs alternate in position with those between the next two discs as indicated in the figures just cited.

8. When chromatic granules are conspicuous in a disc they lie at the ends of these lines or interfaces. That is, they lie at the points in the discs where three or more adjacent achromatic droplets come together, as indicated in Figs. 11 and 12 and in suitable regions in the photographs of Figs. 13, 14 and 17. In a descriptive sense, at least, they represent thickenings in the discs at the points where there is a maximum amount of space between adjacent droplets, including those on both sides of the disc.

9. The number of granules in a disc is correlated with, and apparently determined by, the number of achromatic droplets adjacent to it. Similarly, the size of the granules appears to be determined by the size and number of the droplets, together with the amount of chromatic material in the disc; and the shape of the granules appears to depend on these same factors, together with the position of the droplets with respect to one another.

10. In some cases chromatic granules appear to be formed in a somewhat different manner, as indicated in an earlier paper (Metz 1935b). Here the chromatic material is located primarily in the position of the longitudinal lines or interfaces between the discs rather than in the discs themselves. The method of formation of such granules may be understood by comparing Figs. 21 (at 1, 2) 13 (at 1, 2) and 8; or 9, 10 and 8. In 21 and 9 the chromatin is mainly in the discs; in 13 and 10 it extends between the droplets separating adjacent discs, while in 8 (also 7) it lies mainly in the latter position so that the discs look like zig-zag lines which appear to join at intervals to form "granules."

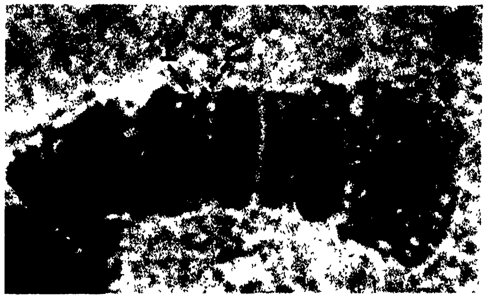


FIG. 21. PHOTOGRAPH OF RIGHT END OF CHROMOSOME A IN SCIARA OCELLARIS, FROM AN ACETO-CARMINE PREPARATION. ×3000

For comparison with Fig. 13, which shows the same region in another preparation. Compare especially discs marked 1 and 2 with the corresponding discs, similarly designated, in Fig. 13. In the present figure the chromatin is shown concentrated in the discs, which look like granular bands; very little extends between the droplets separating the discs. In the other case (Fig. 13) the chromatin is more evenly distributed around the droplets, revealing the honeycomb-like organization in which the two discs appear as zig-zag bands.

mosome A from an aceto-carmine preparation, showing the honeycomb structure at d. (Compare with Fig. 21.) Similar type of structure is shown indistinctly near the tip of the chromosome, at a. (Compare with Fig. 17, which shows this same tip region in another preparation.) 14, from an aceto-carmine preparation. Under the microscope the vacuolated region between the two dark bands at a shows a structure similar to that represented in Figs. 5 and 9. 15, small portion of a chromosome in a formalin aceto-carmine preparation showing the chromatin confined mainly to the discs, forming relatively sharp "bands". ×2600. 16, optical section of a chromosome (pair) in an osmic-formalin-aceto-carmine preparation, showing that the granules are not limited to the periphery of the chromosome but that the structure is essentially uniform throughout the cross section. 17, extreme tip region at right end of chromosome A, showing conditions similar to those represented in Fig. 5 and the diagrams of Figs. 9 and 10. From an aceto-carmine preparation. ×3250. 18, two selected regions from one chromosome group in a preparation treated with 2 per cent chromic acid followed by aceto-carmine, showing the conspicuous alveolar or vacuolated condition. Under the microscope the structure appears more definitely like that shown diagramatically in Figs. 9 and 10. The chromatic material extends between and around the droplets or alveoli instead of being limited mainly to the discs. Similar conditions are often found in ordinary aceto-carmine preparations. ×3270. 19, stretched region of a chromosome from the same preparation as Fig. 18, showing how the chromatic walls or interfaces between the droplets become pulled out so that in optical section they give the appearance of threadlike striations. 20, portion of chromosome A severely stretched. Aceto-carmine preparation. The photograph represents one optical level at or near the upper surface of the chromosome. Note the wide differences in number and appearance of the striations in different regions, the fact that these striations seldom form continuous lines except for short distances, that they are staggered on opposite sides of individual discs or rows of granules (e.g. at j) and that they extend parallel to the axis of the chromosome, not in spirals.

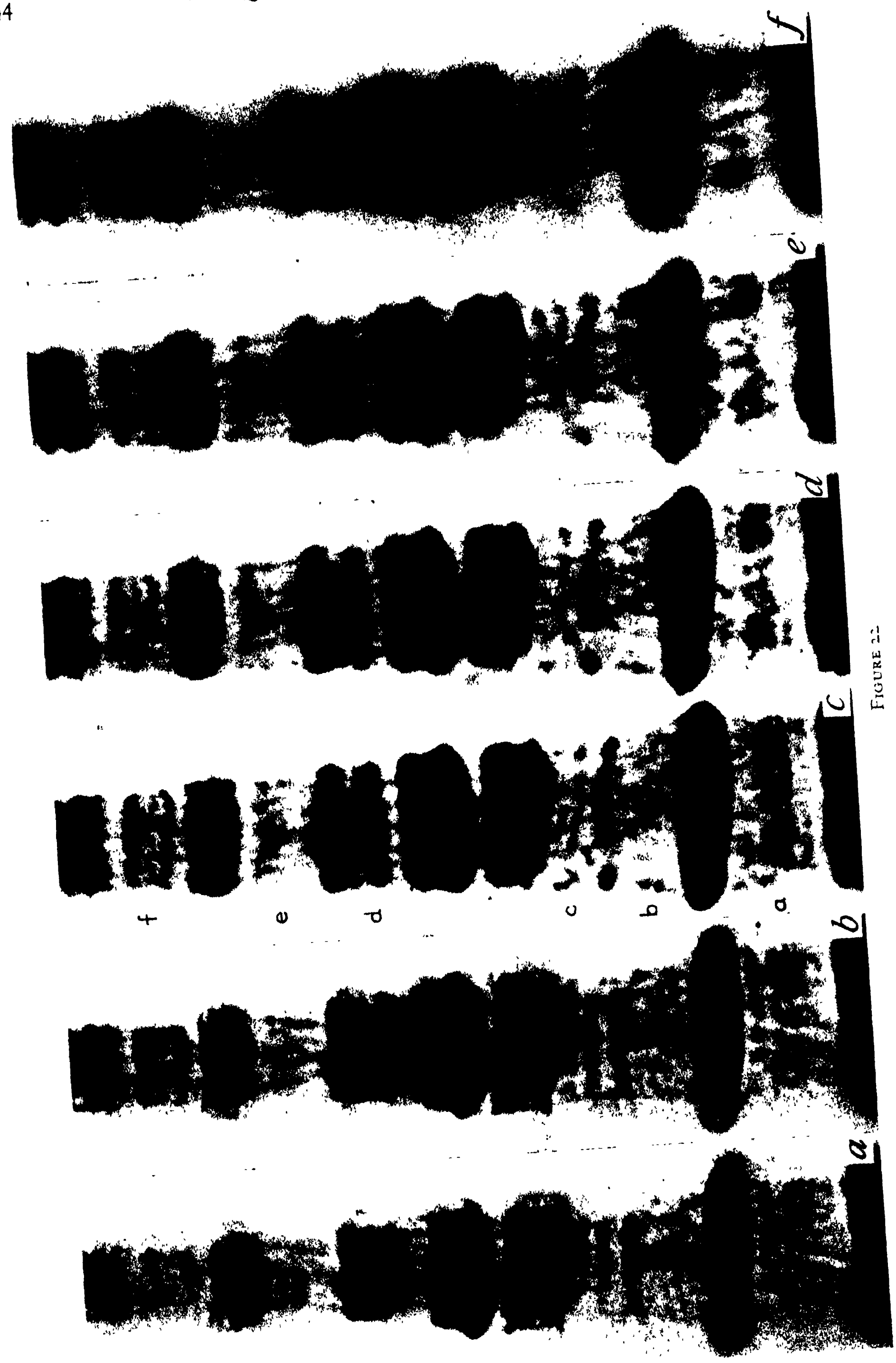


FIGURE 22

11. In cases of the type just described (item 10) it seems clear that each row of granules is made up of material from two different discs and that each disc contributes material to two different rows of granules, one on each side of it. This seems especially significant, particularly in view of the fact that both types of condition may apparently be found in the same region of a chromosome in different cells—i.e. the material which forms a granular disc in one case may apparently in other cases go into the formation of two separate rows of granules which lie on either side of the space which would otherwise be occupied by the disc (Metz 1935b). The region shown at *d* in Fig. 13 shows such characteristics. It will be considered in detail elsewhere. (See also legend, Fig. 21.)

12. Often small heavy-walled droplets resemble granules, and may even be indistinguishable from true granules on superficial observation. Such structures, however, are in all essentials like the droplets considered above and form a part of the alveolar or honeycomb structure. A disc made up of such "granules" is, of course, double.

13. When a giant chromosome is distorted by stretching, twisting or shrinkage the honeycomb structure described above becomes correspondingly distorted (Metz 1936) and striations of various types may become evident. As in the case of striations in other types of protoplasm, the granules lie in the striations (Figs. 19, 20, 22). These striations have been described as chromonemata by several authors, as intimated above. Our observations indicate, however, that they are not chromonemata and are not really thread-like in nature. Fundamentally they represent the interfaces, with more or less chromatic material, between the droplets separating one disc from the next, as indicated in the figures just cited. With progressive stretching such a structure becomes more and more disrupted, first giving a criss-cross or basket work appearance, then a series of striations which become more and more parallel as stretching or shrinkage increases.

Conditions have been described by Bauer (1935, 1936) in *Chironomus* which are said to give clear indication of the presence of chromonemata. Through the kindness of Dr. Bauer we have been permitted to examine the preparations in question. They appear clearly to illustrate the type of organization described above—a criss-cross of interlacing lines—and the same type of fibrillar structure is visible in parts of the cytoplasm, including the gelatinous secretion product of the glands. It is felt that in both the chromosomes and the non-chromosomal materials such apparent structure is the result of shrinkage, together in some cases with mechanical distortion. In this, as in other cases, examples of parallel striations can be found in restricted individual regions, apparently due to localized stresses. As noted elsewhere, however, such striations may extend in any direction, depending on the direction of stress.

FIG. 22. PHOTOGRAPHS OF SIX DIFFERENT OPTICAL LEVELS, IN SEQUENCE, OF A STRETCHED REGION IN A SALIVARY GLAND CHROMOSOME IN CHIRONOMUS (SP.), FROM AN ACETO-CARMINE PREPARATION. ×3000

Two additional optical levels showing the same type of structure were photographed, but are not included here. Note that the striations, which superficially give the appearance of threads, are interconnected to form a lacework or criss-cross pattern which seems clearly to represent distortion and disruption of the fundamental honeycomb type of structure seen in undistorted regions and still visible to some extent in parts of the regions designated a and f here. The fact that this criss-cross pattern is present at all optical levels shows that it is not due to overlapping of spirally disposed threads.

We have made a careful study of *Chironomus* salivary gland chromosomes in this laboratory and have found the type of organization comparable in all essentials with that in *Sciara* and *Drosophila*. Photographs of a portion of a stretched chromosome in *Chironomus* are represented in Fig. 22. The criss-cross type of structure here is present at all levels, as seen by focussing up and down through the chromosome, and hence cannot be considered to represent overlapping threads, or to be due to twisting a system of parallel threads. Likewise the striations are not independent, but interconnected, and in favorable regions in such cases the transition from the true honeycomb organization to the more definitely striated is clearly visible, as indicated to some

extent in Fig. 22 in the regions marked a and f.

A more extreme case of stretching is shown in Fig. 20, from *Sciara ocellaris*. Here the original organization has been almost completely disrupted, and the material drawn out so that it exhibits more or less parallel striations. Careful examination shows, however, that these are not continuous, but either interconnect or are staggered. In this chromosome the more delicate discs and granules have become obliterated by the distortion and most of the chromatic material has become incorporated into striations connecting the larger "granules," particularly in the region between h and m. In this region conspicuous individual lines may be traced for short distances, but then abrupt shifts occur. For example, the lines from i to j do not connect with those from j to k, but alternate with them in position. Similarly, those from j to k shift alignment at k, where they end in heavy "granules." The granules of the next row, at l, alternate with those of the latter row in position.

It should be recalled that the material in the chromosome here was coagulated by fixation (aceto-carmine method) before the stretching and distortion occurred. Hence the coarser structures, such as heavy droplets or granules, have tended to retain their relative positions while the finer ones have been shifted or disrupted. In the lower part of the figure, between a and h, the honeycomb structure has been less completely disrupted and more of the interconnections are visible. It will also be noted that the number of striations is considerably larger here than in the region just considered.

It seems clear that in extreme cases like this the number of striations in a region is determined by the number of *large* granules in the transverse rows. In other words, here, as in other forms of alveolar protoplasm, distortion produces striations resembling strands between the larger formed bodies or granules. In the region from b to h in the present case there are no rows of very large granules. All the granular "bands" or discs contain a relatively large number of small granules. Hence the number of striations is large. In the region from i to l, however, there are three transverse rows of large granules, at i, k and l respectively, and the number of striations corresponds to these. It is safe to say that in the original chromosome there were numerous rows of small granules in larger numbers in the intervening region, i to k, and that these have been obliterated in stretching. The one visible row, at j, has more granules than either that at i or at k.

The type of picture obtained by stretching depends to a considerable extent on the original condition of the chromosome. If the chromosome is "vesiculated," as in Fig. 18, and the chromatin largely distributed around the achromatic droplets, stretching produces the type of structure shown in Fig. 19. Here there are few conspicuous granules and the chromatin forms heavy striations which obviously are not threads, but optical sections of thin sheets or strips like the walls of minute, stretched bladders.

The evidence here agrees with that presented previously (Metz, 1936) in indicating that the chromatic material has a high degree of tenacity, may be greatly stretched, and in severely stretched chromosomes is nearly all drawn into striations. This is true in *Chironomus* as well as *Sciara*. In both cases striations may be produced in any direction within the chromosome by suitable stresses, and the chromatin of the discs may be pulled out laterally into delicate strands. An

example of the latter type of distortion in *Chironomus* is shown in Fig. 23. It is significant that here, as in *Sciara*, pulling the material out sidewise does not reveal any indication of longitudinal threads.

It is felt that the important consideration in the present connection is the mode of formation of the striations rather than their appearance in stretched chromosomes. Where the component structures are small, as in *Drosophila* and to a considerable extent in *Chironomus*, the finer organization is often obscure. In *Sciara*, however, details of structure are often so clear as to indicate clearly the nature of this organization (in the coagulated chromosome).

In some cases the granules in a small region of a chromosome may be aligned in apparently spiral or diagonal paths, as described by Koltzoff and by Bridges. Such an appearance is found when the structure is of the type shown in Figs. 7 and 8. Moderate twisting of the chromosome accentuates the spiral appearance. It seems clear that such alignment of the granules is of secondary importance and does not indicate the presence of chromonemata.

FIG. 23 PHOTOGRAPH OF A PORTION OF A CHROMOSOME GROUP FROM A SALIVARY GLAND NUCLEUS OF CHIRONOMUS (SP.), FROM AN ACETO-CARMINE PREPARATION. X1710

This illustrates the fact that lateral as well as longitudinal strands may readily be produced by suitable distortion, and that such lateral distortion does not reveal any indication of longitudinal threads (see text). Lateral strands extend from the region marked a, and also from the chromosome at the upper right.

DIFFERENTIATION OF THE GIANT CHROMOSOMES

Up to this point no special consideration has been given to the possibility that the giant chromosomes are not only enormously enlarged, but are highly specialized structures in which the component parts have undergone radical modification comparable to that exhibited by the cytoplasmic constituents in the same cells. Alverdes (*loc. cit.*) in his early work on the subject considered them to be products of a

process leading directly to disintegration, and interpreted the internal structures on this basis. Although his particular interpretation is not now acceptable his point of view may be correct, for the salivary glands under consideration are larval organs which undergo histolysis at pupation.

The opposite point of view has been maintained by Bauer (1935) who has claimed, on the basis of Holt's early evidence, that giant chromosomes of the type considered here give rise to the multiple chromosome complexes found in the alimentary canal of *Culex* and that this provides a demonstration of their compound (and hence essentially "normal") nature. Bauer's interpretation has been shown to be erroneous, however, by Mr C. A. Berger (*loc. cit.*) who has made a careful study of conditions in *Culex* in this laboratory and has found, as intimated above, that the multiple complexes do not arise from structures comparable to the giant chromosomes, and also that cells which do possess giant chromosomes here all degenerate at metamorphosis. These findings agree with all other observations on the subject, so far as we are aware. The evidence is strong, therefore, that giant chromosomes of this type are found only in cells which are incapable of further division. It would not be surprising if this specialization were reflected in a marked modification of internal structure.

It should also be noted in this connection that in *Sciara*, as shown by Doyle and Metz, the giant chromosomes in a normal living condition occupy most of the space in the nucleus and appear almost structureless, while on fixation or aging in physiological salt solution they undergo great shrinkage through loss of non-staining material and at the same time reveal the internal structural characteristics already considered. Observations made here in coöperation with Dr. D. F. Poulson indicate that comparable changes occur in *Chironomus* during fixation or aging. Such considerations, of course, do not imply that structural differentiation is lacking in the living chromosome and is produced by fixation, but they do raise a question as to how the fixation image should be interpreted, and also as to how far conditions in the giant chromosomes are comparable with those in "ordinary" chromosomes. Since much achromatic material is extruded by the giant chromosomes on fixation, it seems possible that much of what remains is not really chromosomal material but nucleoplasm.

At present there is no obvious way of making proper allowance for these features. They serve mainly to emphasize the need of caution in interpreting the evidence or in formulating any comprehensive hypothesis. What appears to be a significant line of evidence in this connection is provided by the chromosomes in some of the relatively small cells of the salivary gland. Like the cells, these chromosomes are many times their original size, although small as compared with the largest ones. They almost certainly represent pairs of homologues, and probably are at least quadripartite (evidence of Painter and Griffen, *loc. cit.*, and unpublished evidence of Dr. J. B. Buck), yet they appear single, each as a row of achromatic droplets held together by chromatic regions of about the same diameter as the droplets. So far as we have been able to observe there is no evidence of the four or more individual chromonemata presumably present in them.

DISCUSSION

In the considerations outlined above attention has been given to three different interpretations as to the organization of the giant chromosomes of dipteran larvae. A fourth interpretation, the oldest of all, should also be noted. It was advanced by Alverdes (1912) who considered the chromatic discs in the giant chromosomes to be disintegration products formed by breaking up of an original coiled chromonema. Each disc was thought to be one coil or part of one coil of the chromonema. The evidence presented in the papers noted above seem definitely to rule out this view, although it is apparently still supported by some investigators (Heitz, 1934; Sinoto and Yuasa, 1935).

The considerations of the present paper lead directly to the view that the visible striations in the giant chromosomes referred to by various authors as individual chromonemata are not chromonemata.

It seems clear that the striations in distorted chromosomes do not represent any definite units of organization, but are distortion products comparable to those produced under comparable conditions in other protoplasmic materials. The achromatic droplets are units of structure in a descriptive sense, at least, and form a natural basis for attempting to interpret the organization in terms of

chromonemata. The simplest method of doing this would be to assume that the droplets are aligned in a compact series to form chromonemata. As already noted one serious objection to this assumption is seen in the fact that the number of droplets is not uniform at the different loci in a chromosome. Another difficulty is the enormous size of the droplets as compared with any units in an ordinary chromonema. A third difficulty, according to our evidence, is the lack of any visible alignment or attachment or differentiation into such series. A fourth difficulty is seen in the lack of certainty as to whether the droplets have real structural significance and whether the actual numbers or constancy of numbers of droplets fulfills the requirements of such a view. It is by no means certain that the numbers are regularly multiples of two, or that a given locus will regularly have the same number under comparable conditions of size, etc.

Since the present paper was originally written Bauer (1936) and Painter and Griffen (1936) have called attention to conspicuous heavy-walled droplets and considered them as chromomeres aligned on threads. Painter and Griffen state that there are "two types of achromatic areas in the giant chromosomes; one is made up of chromomeric threads connecting the chromomeres (bands), the other formed by the achromatic vesicles." These areas are apparently considered to alternate. Although such an interpretation is suggested by the honeycomb type of structure in some regions our evidence does not support it as a conception of the real organization. Apparently they consider the delicate lines across one achromatic zone as threads and those across the next achromatic zone as interfaces or boundaries between appressed droplets. Our evidence does not indicate such a distinction (see, e.g. Figs. 5, 6, 13, 14, 15).

It might be suggested that the achromatic droplets do have significance as individual structures in the following manner: (1) that in some sense, as yet not understood, they represent chromomeres which have arisen by division of preexisting chromomeres which had themselves undergone great enlargement. (2) That the original chromomeres in one chromonema were intimately united to form a delicate cylinder of essentially uniform diameter—not separate and connected by threads like a string of beads. (3) That in the development of the giant chromosome no new true chromonemata are formed, but the chromomeres at different loci undergo multiplication more or less independently. (4) That growth continues fairly uniformly in all, so that if division does not occur the individual droplets are correspondingly enlarged (thus accounting for large droplets in small numbers in some regions).

Such an interpretation is far from satisfactory, and is opposed by certain lines of evidence (considered elsewhere). It is particularly unsatisfactory in that it necessitates the assumption of relatively enormous growth of the original chromomeres (genes?)—a growth without division, which has no counterpart in ordinary cells. As noted above, in the case of *Culex*, true, uniform multiplication of chromonemata does not give a structure resembling a giant gland chromosome.

As an alternative interpretation it might be suggested that two types of multiplication have occurred, in the following manner: It might be considered that the droplets multiply, as just intimated, but that each is itself a highly compound structure derived by multiplication of finer units which are themselves invisible. As already pointed out, however, such a view not only lacks observational support, but introduces a serious difficulty in that it assumes the presence of units of two

types, one a compound of the other, but nevertheless capable of division. Furthermore, since on this view the real units are invisible, the interpretation reveals little about the real nature of the structure.

Since much emphasis has been laid on the chromatic granules, by nearly all observers, a few words of further comment on them may be added here. The true granules (as distinguished from the small heavy-walled droplets which are only granules in a descriptive sense) seem best interpreted as merely relatively thick, as compared with relatively thin, regions in the discs, or as accumulations of material between the achromatic droplets separating the discs. In both cases they appear to owe their characteristics to the adjacent achromatic droplets, except as regards bulk, which depends also on the total amount of chromatic material present in a given region or disc. It is clear that the granules do not represent units of like characteristics in ordinary chromosomes. They could only be considered to have significance in relation to units in ordinary chromosomes by assuming a great enlargement of these original units. On any other basis they could only be given significance as real structural units of chromonemata by assuming that they represent definite multiples of the original units—i.e. are units of a higher order, but are themselves compound. As pointed out previously (Metz 1937) such an assumption not only seems to have no foundation in the evidence, but is difficult to justify on a theoretical basis.

On the present interpretation the giant chromosome may or may not owe its size mainly to multiplication of chromonemata which retain the size characteristics they exhibit in other cells. If it does, however, the evidence indicates that the unit chromonemata and chromomeres are not visible as such and must be present in very large numbers. Even in this case it seems necessary to assume that the chromonemata have increased in length, as intimated above, for, although theoretically possible, it seems very improbable that a chromonema in an ordinary cell in one of these flies could approximate the length of a giant chromosome.

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THE SURVIVAL OF TISSUES AFTER THE DEATH OF AN ANIMAL

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ONCE upon a time a person, evidently not a biologist, wrote a jingle to the effect that when his dog Rover died, "he died all over." The rhyme may have been correct but the facts were wrong, because certain it is that when Rover's heart stopped beating he didn't die all over; some of his tissues promptly ceased functioning but others did not.

This point is of interest to the human race for a number of reasons. Thus, when a man has hanged himself, or has been submerged in water, or exposed to carbon monoxide gas, or has apparently been killed by an electric shock or an overdose of chloroform, the question arises: How soon must the heart be put in motion again if resuscitation is to be effected and ultimate recovery is to be complete? Unfortunately, there have been many cases in which, after artificial respiration had been carried on for a time, the heart and lungs resumed their work but the blood returned too late to the brain, and the patient either remained comatose for several hours and then died or else woke to find later that his brain was badly injured. Much information on this point is to be found in articles by Shillito, Drinker and Shaughnessy, Salinger and Jacobsohn, Bruns, Pike, Guthrie and Stewart, Scheven and Boehm.

Some day, when we learn how to prevent the autolysis of transplanted organs, surgeons will be seeking for the best methods of preserving tissues removed from healthy persons who have met sudden death. Already the Russians are experimenting with red blood cells removed from the dead, such cells being kept on ice until needed for transfusion.

The subject of survival is of interest also to the histologist when he is securing bits of tissue for study; how long can he wait after the death of the organism before the cells will have changed in their appearance. Or the physiologist who is interested in keeping tissues alive and functioning normally outside the body may want to know how long he can wait before starting a tissue culture.

I became interested in survival times because I thought I could probably use differences in death rates of the different tissues of the bowel and perhaps parts of the neurones to analyze the structure and functions of the myenteric plexus. For instance, if it could be shown that synapses are unable to transmit impulses after a certain time, then any function that fails at that time might well be ascribed to conduction through synapses.

It was with this idea in mind that the writer searched through much literature for information as to the ways in which different parts of the nervous system die. As everyone knows, the first organ to suffer when the heart stops is the brain; and some parts of it are more sensitive and vulnerable than others. This difference in vulnerability may perhaps be

explained on the basis of local variations in metabolic rate. As one would expect, the tissues which have the fastest metabolism and therefore the greatest need for oxygen suffer first when the supply of this gas fails. One is reminded of Hippocrates' statement that "Old men endure fasting most easily, then men of middle-age, youths very badly, and worst of all children, especially those of a liveliness greater than ordinary."

Because so many of the studies on the survival of tissues are made incidentally in the course of other researches and are not mentioned in the title of the published report, it is difficult to review the subject completely, and I have not attempted to do so. As I said before, my main purpose in making this study was to gain some idea of the survival times of the different parts of neurones, and particularly of the peculiar types of hardy neurone that is found in the sympathetic nervou^ system and in the plexuses of the intestine.

Naturally, the rate at which a tissue dies depends not only on its own metabolic rate, but also on the metabolic rate of the animal of which it is a part. Thus it has been shown that the tissues of the quickly moving fishes die faster than do those of the more sluggish ones (Willer), and, as one would expect, the tissues of a cold-blooded animal usually survive much longer than do those of a warm-blooded animal. Jolly found that the blood cells of Triton kept forming new mitotic figures for fifteen days after removal from the animal, and the leukocytes of batrachians still showed ameboid movements after ten months in the ice box.

Differences in the metabolic rates of the several parts of the bodies of lower forms of life have been studied extensively by Child and his pupils. MacArthur and Jones showed that in the central nervous system the metabolic rate of the tissues decreases as one passes from the gray matter of the cerebrum to the white tracts at the lower end of the spinal cord, and a corresponding gradient in the vascularity of these parts was shown by Craigie.

As one might have expected, a number of studies showed that the speeding up of the metabolic rate of tissues by the giving of thyroxin impaired their ability to withstand anoxemia, and the slowing down of the metabolic rate effected by thyroidectomy lengthened the survival time in a poorly oxygenated medium (5, 20, 41, 98).

The survival time of a tissue depends also on the temperature at which the tissue is kept after death. Grodzínski, who kept bits of aorta taken from chick embryos at a temperature of 38°C. could not get them to grow in tissue cultures started after three hours. Bucciante found that moderate cooling down to 0°C., would lengthen the period of survival, but great cold, below 0°C., greatly shortened this period. When they had been cooled to −30°C., none of the bits of tissue removed from chick embryos grew. The survival time depends also on the amount of care that is taken to avoid contamination of the tissue with bacteria. Furthermore, men who kept tissues in oxygenated Locke's solution reported longer survival times than did those who first boiled and sealed the fluid, or who replaced its oxygen with nitrogen (Garry, p. 235). Nolf found that the highly sensitive ganglion cells in the intestine of the chicken survived better if some blood was added to the iced Locke's solution, and Cannon and Burket found that when they squeezed all the blood out of a short segment of bowel the survival time of the ganglion cells was about half of what it was when the mesenteric vessels were simply tied. The writer found that when the lumen of excised intestine was kept full of oxygen,

the functions of irritability, conduction of impulses, and rhythmicity were retained for many hours. Oxygen under increased pressure has been used by some investigators to keep excised tissues alive.

Because of these variables, one cannot always compare the figures given by one investigator with those of another. Thus, the men who, in studies on animals, tied both carotid and both vertebral arteries observed a longer survival of the reflexes about the snout than did those who used a guillotine. This was because even the small amount of blood that trickled into the brain through the spinal arteries served to prolong the life of the ganglion cells (Hill, Gildea and Cobb and others). Winkelbauer, who bled dogs until the electrocardiogram showed that the heart was no longer pumping, found that few animals would recover if the blood was not returned by rapid transfusion within five or six minutes. Curiously, an occasional animal could be revived after twenty minutes. On the other hand, Bruns was able to resuscitate dogs which a half hour previously had apparently been killed by the inhalation of carbon monoxide or carbon dioxide. In them, some circulation must have persisted for a while.

The length of time which may be allowed to elapse between apparent death and successful resuscitation will naturally be influenced by the length of the interval during which the heart keeps beating strongly enough to maintain a small flow of blood in the brain. Thus, in the cases of six dying men, Laubry and Degos found that some electrocardiographic signs of cardiac activity persisted for six, seven, ten, twelve, fifteen, and twenty minutes, respectively, after respiration had stopped and the radial pulse had disappeared (78). When men are officially hanged by being dropped through a trap, the heart continues to beat for about twelve minutes (94).

Remarkable experiments have been performed by a number of men who have markedly dehydrated certain tissues, or even entire small animals, and have later moistened these tissues and caused them to live again. This ability of lower forms of life to dry out and live through a period of drought was commented on years ago by Leeuwenhoek. According to Hall, some investigators have resuscitated small forms of life kept in a dry state for as long as five or six years. Even more remarkable is the report of Baker who, in 1764, moistened some nematodes that had been kept dry for twenty-seven years and found that they could be brought to life again (Hall).

Hall did a great deal of work to see how much dehydration would be tolerated especially by various lowly animals. According to him, man will tolerate the loss of only 10 per cent of his tissue fluids. More recently Krawkow, Morosow, and others have reported almost unbelievable survival times of bits of chick embryo, heart muscle and blood vessel wall kept in a desiccated condition for periods as long as six months. Morosow proved that his desiccated tissues were alive by culturing the cells. According to Lipschütz, partially dried ovaries kept for months and then transplanted into a suitable animal grew and produced recognizable hormonal effects.

THE BRAIN CELLS

The cells of the body least resistant to anoxemia seem to be those of the higher centers of the brain. They are so dependent on a good supply of blood that, in both man and animals, consciousness often fails the moment the circulation stops. The patient who suffers a large coronary thrombosis, or a temporary stop-

page of the heart beat such as is seen in coronary thrombosis or Adams-Stokes' disease, may topple over in the middle of a sentence, and the aviator, whizzing around a pylon in the international speed races, or straightening out after a long dive, "blacks out" or goes blind for a second as the centrifugal force pulls the blood out of his brain. Willius tells me that in the case of a man with Adams-Stokes' disease, reported by him and Yater, the stoppage of the heart beat was followed promptly by unconsciousness; the patient would stop in the middle of a sentence, only to go on a minute later as if nothing had happened to interrupt his flow of thought. In one such episode the electrocardiogram showed that there had been no heart beat for almost four minutes. Gallavardin and Bérard reported a similar case in which a man survived periods of asystole as long as two and a half minutes. The experiences of Nathanson, Weiss and Baker, Smith and Moersch and others who stopped the heart beat by pressure on the carotid body showed that some persons can remain conscious for from seven to twenty seconds after the circulatory pump ceases to function.

With lesser degrees of anoxemia, such as are met with at high altitudes or in cases of carbon monoxide poisoning, the higher faculties fail first and the victim does not recognize the fact that he is in danger. A beautiful example of this is to be found in Stefansson's vivid story of his difficulties in getting his fellow explorers out of an ice hut in which they were all being badly gassed by a defective stove.

In 1857 Brown-Séquard found that when rabbits were made unconscious by compressing for three minutes all four arterial trunks supplying the brain they could rarely be resuscitated. More recently, Hill found that the degree of cerebral anemia produced in a wild rabbit by holding it up by its ears was enough to paralyze the respiratory center in from eighty-five to 110 seconds. Complete anemia of the brain, if maintained for one minute, abolished all cortical activity.

After stoppage of the circulation the pupillary and ocular reflexes disappear almost immediately, but the respiratory and vasoconstrictor centers continue to show some signs of function for from thirty to 120 seconds (7, 9, 30, 39, 96). Pike, Guthrie and Stewart, Stewart and Pike, and others have found that the respiratory center is unusually resistant to anoxemia.

Laborde, experimenting with the head of a decapitated criminal, found that electrical stimulation of the cortex of the brain caused contractions of the facial muscles for a period of fifty minutes. In another such head the cortex lost excitability in twenty-five minutes, leaving only the facial nerves capable of function. Much of the old literature on this phase of the subject is given by Battelli, d'Halluin, Prus, and Hayem and Barrier.

The next question is: How long does it take for anoxemia to injure the brain cells so severely that they cannot recover? The best answers seem to range from five to ten minutes (30, 31, 39, 45, 93, 96). Reports of longer intervals indicate that the experimenter failed to shut off the circulation completely. Even when the anoxemia lasts only from five to ten minutes many of the animals that survive are demented.

NERVE CELLS IN THE SPINAL CORD

The nerve cells in the spinal cord are apparently a little more resistant to anoxemia than are those in the brain, but still they are so sensitive that closure of the aorta causes immediate insensitive-

ness and paralysis of the hinder parts of the body, together with a loss of reflexes (31, 93). Recovery is not likely to be complete if the obstruction is maintained for more than ten minutes. Some return of function can take place after thirty minutes, but after an hour the nerve cells are destroyed.

According to Ehrlich and Brieger, in rabbits one can destroy electively the gray substance of the cord if one shuts off the circulation for the right length of time. The conducting paths and the ganglions on the posterior roots are considerably more resistant than are the anterior horns.

SYNAPSES IN THE CENTRAL NERVOUS SYSTEM

Now that the synapse is coming to look more like a drugstore and less like two interdigitating sets of roots (6, 29, 84), it is not so easy to talk about its injury and "death"; all one can say is that it is less resistant to anoxemia and other deleterious influences than is the rest of the neurone (90).

Recently Heinbecker found that synapses can be blocked by a dose of carbon dioxide which does not extinguish the action potentials of normal nerve fibers, and he used this fact to help him in analyzing the structure of certain parts of the nervous system. The fact that reflexes disappear usually within a few seconds after the stoppage of the circulation indicates that synapses in the central nervous system are highly sensitive to anoxemia.

NERVE FIBERS AND ENDINGS IN THE CENTRAL NERVOUS SYSTEM

According to Forbes and Ray, if an animal is killed and the nerves are left in situ, action currents cannot be elicited after the passage of one or two hours. Curiously, if the nerve is removed after it has lost all signs of function and is placed in Locke's solution, it will recover and will then survive longer than if it had been removed promptly after the death of the animal. Nerves promptly removed and kept in Ringer's solution at a temperature of from 5 to 10°C. showed action currents as late as the fourth day. Symes, using cats and dogs, saw negative variations in excised sciatic nerves kept at 0°C. for from seven to nine days and then warmed. Gerard kept dog's nerves in an atmosphere of nitrogen and at a temperature of 25°C., and he found that the strength of the electric response was reduced by half in from ten to forty minutes. The survival time varied with the metabolic rate of the nerve (26, 46).

According to Hermann's old studies, the voluntary muscles can be stimulated through the nerves for about an hour after death. After tying the aorta of a rabbit, Fredericq stimulated the sciatic nerve and could obtain muscular responses from the muscles after half an hour. De Zilwa obtained contractions of the retractor penis of a dog on stimulating the pudic nerve two hours after death. Curiously, Parker found that the neurofibrils in nerves go to pieces a few seconds after the stoppage of the circulation.

In man, the sudden closure of the femoral artery by an embolus produces immediate anesthesia of the affected limb, anesthesia which appears to be due to an injury to the sensory nerve endings and not to the nerve trunks. The motor functions of the limb are not much disturbed, which again indicates the comparative hardiness of nerve fibers and motor nerve endings. Curiously, the limb which is anesthetic to a pin-prick will usually for some time be the seat of agonizing pain, showing that sensory nerve fibers are still conducting (88).

RESISTANCE TO ANOXEMIA IN PARTS OF THE AUTONOMIC NERVOUS SYSTEM

Available evidence points to a greater hardiness of autonomic neurones as compared with those in the central nervous system, and to a greater hardiness of the sympathetic neurones as compared with those in the parasympathetic division (42, 43, 55, 70, 89). Those in the wall of the intestine seem to be hardiest of all.

Elliott found that four hours of anoxemia due to clamping of the aorta did not seem to injure the function of the ganglion cells on the bladder of the cat. Years ago, Langendorff, and later Hering and Langley, showed that when a cat or rabbit is killed, the ocular reflexes that are dependent upon the functional integrity of synapses in the sympathetic system disappear in from ten to twenty minutes, together with the pilomotor effects of stimulating preganglionic fibers.

The normal effects of stimulating postganglionic fibers can be obtained for from thirty to forty-five, or perhaps even ninety minutes, after the death of the animal. In order to restore function in the cervical ganglions, Schröder had to perfuse the neck usually within fifteen or twenty minutes after the stoppage of the circulation. In only one experiment did he get a return of function after an hour.

In one experiment Hering found the cervical ganglions impermeable to stimuli within fifteen minutes after the stoppage of the circulation; the postganglionic fibers were still conducting after thirty-three minutes, and the vagus fibers to the heart were active for fifty-five minutes.

Pavloff cut a vagus nerve and left the end under the skin of the neck where he could stimulate it. The slowing effect on the heart was lost in from four to five days, but the effect on the pancreas persisted until the eighth day. This was confirmed by Tonkich. Using a similar technic, Arloing noted slowing of the heart up to the eighth day in some animals. In the dog all effects were gone by the fifth day. Curiously, in a donkey, long after the effect on the heart was gone, in fact thirty days later, there were good effects on the larynx and esophagus. All of this suggests that certain types of nerve endings in the tissues can degenerate before the fiber does.

According to Hering, Danilewsky and others, if a dead dog or monkey is kept almost frozen, and the heart is later revived by transfusion, a vagus effect can be demonstrated if the interval has not been more than one or two days; the accelerator nerves usually survived longer, in one instance up to fifty-three hours. If the body is not frozen immediately, the vagus effect on the heart seems to be lost in about forty minutes, and the accelerator effect a little later.

From work on guinea-pig bowel, Newman and Thienes came to the conclusion that the vagus nerve endings in Auerbach's plexus probably lose their function rapidly after the circulation stops. The nerve endings in the retractor penis of the dog were effective for at least two hours after the death of the animal, and they were immune to the largest doses of atropine and curare (107).

THE NERVES OF THE INTESTINE

Mall found, in studies on animals, that if the superior mesenteric artery was clamped for from twelve to twenty-four hours, reëstablishment of the circulation no longer started rhythmic contractions; but the bowel was not dead, as was shown by the fact that when he injected some hot saturated solution of mercuric chloride into the lumen he produced powerful rhythmic and peristaltic contractions. Cannon and Burket showed that segments of the small bowel of a cat or dog will

survive for about six hours even when all the blood supply is tied off. So far as they could see, the functions of such segments were normal after the restoration of the blood supply, and they could not find in microscopic sections any degeneration of the ganglion cells. But such tying of the mesenteric vessels leaves blood and hemoglobin close to the nerves, and since this almost certainly saves them from injury, Cannon and Burket devised other experiments in which they exsanguinated short segments of bowel by compressing them between two glass slides. By doing this they cut the survival time of the ganglion cells to about three and a quarter hours. They concluded that any period of exsanguination short of that which produces gangrene of all the tissues will leave the nerves capable of recovery, and this indicates that the nerve cells of Auerbach's plexus must be the hardiest in the body. Similarly, Scandola found that the intestine of the dog would survive six hours of anoxemia, and three weeks later the mistreated segment behaved normally.

Elliott clamped the aorta of a cat for four hours and failed to kill the ganglion cells that are to be found on the wall of the bladder. Macht found that the pigs' ureter will contract rhythmically after three days in the ice box.

According to Nolf, the neurones in the excised intestine of the chicken are so sensitive to anoxemia that the tissue must be kept very cold and in contact with blood if one wishes to revive the nerves later. Curiously, in the chicken, the nerves of the intestine seemed to be much less tolerant of anoxemia than was the sciatic nerve.

SURVIVAL OF MUSCLE

Hering, Danilewsky, Kuliabko, and others almost froze recently killed animals, such as dogs, rabbits and monkeys, and made the heart beat again after two or three days. Kuliabko and d'Halluin caused the hearts of infants to beat again twenty hours after death. Luciani mentions the case of a criminal whose heart was made to beat eleven hours after death.

Gunn and Underhill, Alvarez and others resuscitated intestinal muscle from cats, dogs and man, which was kept for about five days in cooled Locke's solution, and caused it to contract rhythmically. Overton and Mines preserved the irritability of excised sartorius muscle of frogs for from sixteen to twenty-one days by keeping it cool and under sterile conditions. Overton used a solution of 0.75 per cent boric acid to inhibit the growth of bacteria. Similarly I have kept rabbits' intestine irritable for a week in Locke's solution to which was added .05 per cent boric acid. A concentration of 0.5 per cent was injurious. The removal of the intestinal mucosa also served to prolong the life of the muscle.

Tennant ligated the descending branch of the left coronary artery of animals and found that the muscle lost its power to contract effectively after one minute. This change was irreversible after twenty-three minutes. Marked changes took place in the excitability, for electric currents, of the dog's sartorius after seventy minutes (8). Factors influencing the survival of intestinal muscle after the death of an animal were studied by Ascanio and me, by Garry, and by Gross and Clark.

HISTOLOGIC DETERMINATION OF SURVIVAL TIME

Besides the physiologic method of determining survival time there are histologic methods. Thus, Cannon and Burket found that in exsanguinated gut, the ganglion cells showed marked degenerative changes after three and a half hours.

Curiously, however, Müller found that such cells would stain well in intestine removed from ten to fifteen hours after death, and van Esveld obtained well-stained preparations from segments of bowel that had been kept well cooled for seven or eight days. Perhaps in such cases the stagnant blood in the bowel helped to preserve the tissues. In my own work I found it almost impossible to tell from histologic study whether or not ganglion cells are capable of function. When they were edematous and cloudy, and Kernohan, an experienced neuropathologist, was sure that they were decidedly abnormal, I had to admit that the intestine from which the sections had been taken had seemed to function normally.

Lewis and McCoy removed the various organs of rats aseptically, and in one group of experiments kept them in a moist chamber at 37°C. These experimenters used as a sign of dying the development of vacuoles and granules which had an affinity for neutral red. When the tissue died, the color left the granules and vacuoles and diffused throughout the cell. In the brain the granules and vacuoles seemed to form before the experimenters had time to get the tissue out of the dead animal, and a similar difficulty was encountered in the case of intestinal epithelium and heart and skeletal muscle. The principal cells of the liver and the pancreas showed degenerative changes after ninety-six hours, but in smooth muscle from the bladder and uterus they came only after 240 hours.

Gomez and Pike, and Gildea and Cobb found decided histologic changes in the ganglion cells of the brain within ten minutes after death. Cells in the medulla and in the anterior horns of the spinal cord lasted a little longer, and cells in the spinal ganglions lasted thirty minutes. According to Tuckett, sympathetic ganglion cells had to be deprived of their blood supply for five days before they showed histologic changes comparable with those produced in the spinal cord in one or two days.

Another way of determining whether a tissue is alive or dead is to culture the cells and see if they will grow. Thus Bucciante took bits of chick embryos and left them under sterile conditions at room temperature for varying periods of time. He found that skeletal muscle would grow after twenty days, cornea after eighteen days, skin after fourteen days, heart muscle after six days, and liver after two days. These periods of time could be somewhat lengthened by keeping the tissues in the ice box.

In a long series of experiments my associates and I found that anoxemia can be used much as nicotine is used to block synapses, and to throw light on the structure and functions of neuromuscular mechanisms like the heart and bowel. With the body of the dead animal or the excised bowel kept moist at 38°C., what were probable synapses between the vagal neurones and those of Auerbach's plexus failed to conduct from ten to twenty-seven minutes after the stoppage of the circulation. Another mechanism, perhaps a synapse, which is more or less essential in the production of peristaltic rushes, often failed to function after from thirty-five to forty minutes. Nervous paths, which in the intestine conduct an impulse about 15 cm. orad and caudad from a point stimulated electrically, often failed to function after 100 minutes; long distance rapid conduction from one end of the bowel to the other was retained for at least seven hours; nerve endings in the muscle also lasted seven hours or more, and the function of rhythmic contraction often persisted for several hours after the disappearance of conduction.

SUMMARY

The literature reviewed indicates that in mammals, including man, endings in the central part of the nervous system and perhaps some ganglion cells, cease functioning within a minute or two after the circulation stops. The susceptibility to anoxemia decreases somewhat as one passes from cortex through the medulla and to the spinal cord. Consciousness and most reflexes disappear immediately after stoppage of the circulation. After from five to ten minutes the damage done to the nerve cells of the brain is irreversible. The nerve fibers and the motor nerve endings of the central nervous system are much more hardy, and will function for an hour or two when left in the uncooled animal.

In the autonomic part of the nervous system, and particularly in the sympathetic division, all parts of the neurone are more resistant to anoxemia than are the corresponding parts of the neurones in the brain and spinal cord. Reflexes persist for from ten to twenty minutes after the stoppage of the circulation, and ganglion cells can be resuscitated for periods of time usually up to twenty minutes. Stimulation of the vagus nerves continues to slow the heart for about forty minutes, while stimulation of sympathetic nerves accelerates the beat for a few minutes longer.

What is probably a synapse at the end of the vagal fibers fails to function from ten to twenty-seven minutes after the stoppage of the circulation. What is perhaps a synapse, connecting neurones in Auerbach's plexus and helping in the production of peristaltic rushes, commonly fails to function after from thirty-five to forty minutes. Nervous conduction in the bowel lasts often for more than seven hours. Even then, if the blood is left in the vessels the changes in the nerve cells are not irreversible and several days after removal of the clamp on the mesenteric arteries all the motor functions of the bowel will seem to be normal. If all blood is removed from the vessels, irreversible changes appear after three hours and a half.

Because of their slower metabolic rate, the tissues of lower forms of life stand anoxemia and dehydration much better than do the tissues of higher forms.

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A CRITIQUE OF PLANT SEROLOGY (*Continued*)

PART II. APPLICATION OF SEROLOGY TO THE CLASSIFICATION OF PLANTS AND THE IDENTIFICATION OF PLANT PRODUCTS

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[*Editorial Note:* Exigencies of space make it necessary to publish this review in three parts. Part III will appear in the next number following this. The extensive bibliography will follow Part III.]

I. IDENTIFICATION OF PLANT PRODUCTS

SUITABILITY *of serology for purposes of identification.* The high degree of specificity of the serological reactions as familiar to students of the serology of bacteria and animal proteins, extends to the higher plants as well, although the relative complexity of plant extracts is such as frequently to obscure the specificity to a considerable degree. Thus one commonly finds in the phytoserological literature statements to the effect that animal proteins are more highly specific than plant proteins. We know from the works of Wells and Osborne that provided plant proteins are obtained in a state of comparative purity they exhibit a degree of specificity entirely comparable with that of egg albumin or the proteins of blood. Indeed familiar examples of the species-restricted character of hay-fever sensitivity afford excellent illustrations of the narrowness of the limits of serological affinities even among closely related species of plants.

The plant extracts commonly used in phytoserological practice, however, represent aggregations of many separate antigens, each presumably capable of evoking the production of antibodies. The serum prepared from such an antigenic extract is proportionately complex in its antibody constitution. We know further that in the animal kingdom the same protein may occur, particularly in a nutritional or other non-specific character, in more or less distantly related species (eye-lens protein, milk proteins), and so in plants one may perhaps find such non-specific components of the protein complex (369). We may think of the plant extract as comprising certain antigens so intimately connected with the individuality of the species that their species-specificity is complete, together with other antigens of a less individualistic character, shared in common with related species. As the degree of relationship between two species decreases, the number and quantity of common proteins decreases proportionately until serological inter-reactivity is no longer observed.

For the purposes of convenience, the present discussion arbitrarily falls into two divisions, concerned respectively with phytoserology based on identity reactions and phytoserology based on relationship reactions. However, it is probable that actually all true serological reactions are identity reactions, and that the observed

serological relationships of plants are due to comparisons of mosaics of antigens in which certain constituents are common to several species, others peculiar to a single species. For a more detailed development of this concept the reader is referred to the works of Moritz (233, 235).

The serological reactions have been found generally suitable for purposes of the identification of plant proteins and plant species. This is dependent on the fact that the proteins peculiar to the species are of sufficient antigenic importance in tissue extracts as to minimize the effect due to non-specific proteins. The utilization of such identity reactions for practical purposes has been attempted with success by a number of investigators, and the findings are considered below.

Detection of constituents and contaminants of flours, meals, fodders, drugs, etc. The possibility of identification of proteins by the precipitin and other serological tests has led to many attempts in the utilization of such tests for the determination of the presence and nature of biological materials. As regards substances of animal origin, it has been found not only possible but practical to utilize serological methods for the determination of the source and adulteration of blood, meats, bones, sausages, meat pastes, meat extracts, milk and milk products, eggs and egg products (e.g., caviar, roe, noodles, mayonnaise), honey, animal fats and oils (through their content of proteins as contaminants), and numerous other such substances (194, 260, 353, 216, 354). The routine use of such tests has become somewhat more familiar in Germany than in the United States, and serological evidence has frequently been introduced and accepted as conclusive in German legal cases dealing with pure food and drug laws.

Numerous similar studies have been made regarding plant products, and involve chiefly questions concerning the constituents of flours, meals, bran, animal fodders, and the like. The various plant fats and oils may also be studied from this viewpoint because of their customary content of specifically reacting protein contaminants (194).

As early as 1902 (25, 26, 27) Bertarelli had shown that with legume meals the nature of the gross constituents and the presence of practically important leguminous adulterants can be determined more accurately by means of the precipitin reaction than by the aid of the microscope. Using this method he was able to detect, for example, 2 per cent of *Vicia sativa* in wheat flour. At about the same time, Ottolenghi (267, 268) similarly succeeded in the detection of ergot as a contaminant of flour, although Schern's findings regarding detection of ergot by anaphylaxis tests were less satisfactory apparently because of the very small amount used (314, 315). The precipitin test has also proven satisfactory for the determination of the adulteration of wheat flour by *Vicia faba* (187), maize, rye, barley (189, 347), potato flour (313, 312), and the poisonous *Agrostemma Githago* (23). The toxic castor oil bean, *Ricinus communis*, is a not infrequent contaminant of certain types of European fodders, and the presence of the toxin can be satisfactorily determined by means of the precipitin (Kränich and Mooser in 312, 226, 243) and anaphylaxis (314) tests, and possibly also by the fact that ricin agglutinates normal red blood corpuscles, while the usual constituents of fodders do not (227). If the toxin is heated to 100° (moist heat) it is no longer toxic and fails to give the precipitin reaction, hence the test is indicative of the edibility of fodders (with respect to ricin) whether they once contained active ricin or not (226, 315).

The amounts of contaminants which can be detected serologically are often so small as to be negligible practically.

Thus Becker could safely detect .36 per cent of *Agrostemma Githago* in flour, .74 per cent in wheat bran, and .15 per cent in spelt, quantities which were no longer distinguishable chemically. Ricin can be safely detected in amounts too small to intoxicate cattle (315, 314), .1 per cent by the precipitin and complement fixation tests (229, 28, Kränich in 312). 4 per cent, a legally-permissible amount of potato flour adulteration can be detected in wheat flours and even cooked flour products if denatured potato protein is used in

immunisation (312). The gross-allergy test has also proved of service in the same connection, and the field mustard, another common contaminant of fodder, may be recognized by this method (315, 314), although results were less satisfactory with *Agrostemma* (314). Thöni and Thaysen have reported indifferent results in attempting to separate *Plantago* from wheat flour by the use of whole extracts (346), but on using antigens prepared by fractionation of the extracts with ammonium sulfate, they showed the possibility of detecting 3 per cent of such adulteration, which is an amount smaller by 3 times than the least amount which can be identified microscopically (347). Cao in 1904 (42) attempted to differentiate starches to the same end by the effect on the reduction of Fehling's solution of the addition of "starch-immune" serum to starch extracts. The results, however, are by no means convincing.

On the whole it may be said that the precipitin and other serological techniques have been shown to be of real value in the study of the composition of various plant products, the sensitivity and the specificity of the reactions exceeding those of other chemical and physical tests applied to the same materials, and hence justifying their use in a practical way as an aid in the solution of such problems as have been recounted above.

Application to seed testing. In 1911 Relander (291, 384) used the precipitin technique in an attempt to differentiate and identify the seeds of 27 varieties of barley, oats, *Vicia*, clover, and lupin. In all cases he succeeded in clearly separating the various species involved, and in some cases the varieties of a single species, but the relationship reactions between different varieties of the same species were often so strong as to prevent the possibility of their differentiation. Similarly, Schmidt in 1926 (316) tried to differentiate the seeds of *Pinus silvestris* of a number of varieties and proveniences, but here again, while the results gave some indication of a separation, the degree of differentiation was often so slight as to render the method insufficiently accurate for this purpose.

It has been the general experience of plant serologists that varieties of a single species are separated serologically only with great difficulty. Since in the problem of seed testing the matters of variety and provenience are of paramount importance while species differences are usually recognizable by gross morphology, it is doubtful whether serology can afford much aid to the other techniques available for the determination of the nature and origin of seed.

Identification of the plant viruses. Recent studies are showing that serology bids fair to become a useful technique in the identification of the plant viruses. Beale (283) has already shown that both ordinary and attenuate tobacco mosaic virus may be detected by this means, and Matsumoto and Somazawa (204, 205, 201) have reported at length experiments in which the invasion of plant tissues by this virus was determined serologically before symptoms appeared. In the writer's experience serological tests, particularly the precipitin reaction, have been repeatedly used with success in practical problems in connection with the plant viruses, and the following cases will afford illustrations of the utility of such tests (50, 54):

Masked tobacco mosaic virus in tobacco and mild strains of latent mosaic virus in tobacco, potato, and *Datura* were each identified repeatedly, although in every case the plants bore no recognizable symptoms of disease.

The following viruses in expressed sap were submitted as unknowns (although known to the donor), and each was correctly identified as to group by the precipitin test: tobacco mosaic, tobacco ring spot, cucumber mosaic, latent mosaic of potato, potato ring spot, potato vein banding, and mixtures of tobacco mosaic and potato mottle, of tobacco mosaic and cucumber mosaic, and of tobacco mosaic and potato ring spot. In each case the accuracy of the serological tests was confirmed by infection experiments. Manil (193b) and Matsumoto (201b) have also shown the possibility of identifying the components of virus mixtures in plants by means of precipitin tests. The precipitin reaction has also been used successfully in determining the viability of stored virus extracts and of samples of virus subjected to various physical and

chemical treatments and in studying the transmission of viruses through infected seeds (193c).

Some advantages of the serological tests in such cases are that the testing requires but an hour or less as compared with several days or much more required by the infection method, that one requires no plants for inoculation, which might occasion weeks or months of delay rather than days, that the test is perfectly objective, the symptoms in no way leading to false conclusions, that one may obtain not only a qualitative but to a certain extent a quantitative conclusion regarding the virus present, and that so far as tested virus always reacts with immune serum regardless of whether the virus has been purified by various techniques or preserved in various ways, including drying, so long as the virus is still viable, or whether it has been propagated in hosts different from that used in the preparation of the serum.

II. APPLICATION TO PLANT SYSTEMATICS

Early sero-systematic studies. The first impetus to the use of serology as an aid in plant systematics came as a consequence of the classic studies of Nuttall (1901–1904) demonstrating that by the use of the precipitin test it is possible to show clearly and objectively the relationships of the many hundreds of animal species tested (257–260). The Königsberg series of sero-diagnostic studies commenced with the work of Gohlke in 1913 (104). Between 1901 and 1914, however, there were a number of scattered sero-diagnostic researches, the findings of which are considered in the present section.

In 1901 Kowarski (157) found that the heat-resistant albumose of wheat induces in rabbits the production of precipitins which react strongly with wheat albumose extracts, and weakly or not at all with the albumoses of rye, barley, oat, and pea, although the reaction with pea extract was stronger than that with oat extract. Magnus and Friedenthal next showed by the precipitin test that there is a stronger serological affinity between yeast and the truffle, both of which are Ascomycetes, than between either and a representative Basidiomycete (188). They also confirmed Kowarski in finding that rye serum reacts more strongly with rye extracts than with those of other grains, and further that rye seed serum and rye pollen serum each reacts strongly with extracts of such diverse organs as rye seeds, pollen, roots, and sprouts (190). The same workers also investigated the discrepancy noted above in Kowarski's tests, and found that wheat serum reacted strongly with wheat extract but not with pea extract, and reciprocally. Strong homologous reactions and negative or weak heterologous reactions were also obtained with numerous other plant preparations, e.g. *Ustilago* sap, *Sauromatum* tuber sap, *Mucor* pulp, *Cocos* endosperm milk, and seed extracts of numerous Gramineae (189).

Using Kowarski's techniques, Bertarelli in 1903 (25, 26, 27) performed a number of cross-precipitin reactions with legumes and found that in each case tested the reactions of a serum were much stronger with its homologous extract than with various heterologous leguminous extracts. This work was continued in 1908 (290) by Relander who likewise reported success in the differentiation of species and varieties of barley and *Vicia* by means of the precipitin reaction. Similar results were obtained with species of legumes and grains by Gasis (100) although Gasis observed weak reactions with somewhat distantly related antigens, which according to Magnus (187) were non-specific, normal serum reactions, removable by absorbing the aberrant extracts with normal serum before testing. Such absorption completely restored the specificity of the tests. Magnus also was the first to point out that in the progressive immunisation of an animal, the serum first shows reactivity only with closely related antigens, later with more and more distantly related ones, the homologous reaction always remaining strongest.

Schütze in 1901 and later (321–324) was able to demonstrate both agglutinins and precipitins with yeast preparations as antigens, but was unable to differentiate to satisfaction the various types of yeast used. This may in large part be due to the fact, as pointed out by Defalle in 1902 (64), that the antigens involved are wall substances and not the specific protoplasts or their constituents. Axamit (11) also report positive anaphylactic tests using yeasts as inoculum, although his data on intervals of inoculation indicate that his reactions were atypical if they were truly anaphylactic. Rosenau and Anderson

(297), however, reported reliable positive anaphylaxis tests with yeasts.

In the determination and differentiation of a number of legumes and Gramineae the gross anaphylaxis reaction was used with some success by Rosenau and Anderson (297, 298), Azuma (12), and Karasawa (143), and the precipitin test by Kanahara (142) and Hiki (125).

In 1910, Ballner and Burow (14, 15) applied the complement fixation technique to preparations of legumes and grains, obtaining homologous reactions with titers to 1:20,000, and showing by this method that rye is serologically very close to wheat, with barley and oats less closely related to rye, and rice and maize still more distant although showing some relationship as contrasted with the legumes. The legumes tested were found to form a compact serological group separating into subgroups about *Pisum*, *Phaseolus*, *Vicia*, and *Lens*, respectively. The same year Chapman (46) immunized rabbits to *Acacia* seed extracts, and after discarding all extracts which reacted with normal serum showed that by the precipitin test *Acacia* has stronger affinities with *Pisum* than with *Phaseolus*, *Vicia*, and numerous non-leguminous species.

Wendelstadt and Fellmer (370) applied both the precipitin and complement fixation tests to grains and legumes, and the results obtained confirmed those of the previous workers. Normal serum was found in some cases to introduce artefact reactions, but these were eliminated by dilution. Relationships among the legumes were also indicated by gross anaphylaxis tests, and Inomata (132) used this reaction with success in showing the close relationship between lentils of various species. At about the same time, Sturm (343) found that serum immune to pea extract reacts strongly with pea but not at all with *Sambucus* and *Adoxa*.

Magnus and Friedenthal in 1910 published a further study (191) contending that there is a definite serological similarity between the sexual cells (pollen) and the somatic cells of a given plant species. The "conglutination" reaction (Mez' reaction) gave favorable results in the experience of Sauli (309) in showing the affinities of species of Cruciferae and Leguminosae. Precipitating sera for the sunflower were found by Galli-Valerio and Bornand (98) to react most strongly with sunflower extracts, less strongly with *Aster*, *Cynara*, artichoke, and non-Compositae in the order named. The same workers also showed (99) that precipitating sera for the poisonous *Amanita muscaria* react positively with *Amanita* extracts but not with those of species of various other fungus genera.

The agglutination test was used by Rosenblat-Lichtenstein (300, 301) in the hope of differentiating unicellular algae, and while her relationship tests are not of much significance because of the doubtful systematics of the forms used, her work is of interest in showing that green and colorless cultures of the same alga are serologically distinct, a finding which was later confirmed by Lieske (176), who further eliminated the weaknesses in Rosenblat-Lichtenstein's work, and showed that the agglutination reaction may be used with success in the differentiation of algae, since a given anti-alga serum agglutinates its homologous alga most strongly, closely related species less strongly, and widely separated species not at all.

Finally, in 1914 Zade (385) succeeded by means of the precipitin test in plotting a genealogical scheme of numerous species and varieties of *Avena* and of *Triticum*, which relationships were found to agree well with the existing views with regard to the systematics of the forms treated, both from the standpoint of their morphology and from that of their susceptibilities to certain parasitic fungi (Vavilov, 355). These early sero-systematic studies were reviewed by Janchen in two historical accounts in 1912 and 1913 (136, 137).

We thus see that prior to the studies of the Königsberg school abundant evidence had been brought forward that the serological tests, particularly the precipitin test, afford results entirely in keeping with the earlier systematics of the isolated plant groups tested, that immune sera in general react strongly with homologous plant antigens, proportionately less strongly with antigens from more or less closely related species, and not at all with very distantly related ones. Up to this time, however, the work had been limited to very few plant groups, and it accordingly remained for Mez and his associates in Königsberg to attempt an extension of this work covering the whole plant kingdom, correlating the results previously obtained, and greatly extending them to include many plant groups heretofore not investigated.

The Königsberg serological "Stammbaum" of plants. Under the leadership of Mez the precipitin and "conglutinin" (Mez'

TABLE 1

Serodiagnostic publications of the Königsberg school, and subsequent study by Berlin workers

	KÖNIGSBERG	BERLIN
I. *Sero-systematic studies*		
Plant kingdom, main outlines	Gohlke 1913 (104)	
	Mez and Gohlke 1914 (217)	
Archegoniates, main outlines	Guttmann 1924 (117)	
Algae	Steinecke 1925 (333)	
	Wilke 1929 (378)	
	Landfester 1933 (169)	
Fungi	Neuhoff and Ziegenspeck 1925 (254)	
Bryophytes	Mielinski 1926 (225)	
	Stepputat 1929 (335)	
Pteridophytes	Conradi 1926 (58)	
	Grenda 1926 (115)	
	Wilkoewitz 1929 (379)	
Gymnosperms	Mez and Kirstein 1920 (218)	Eisenträger 1928 (74)
	Kirstein 1922 (146)	
	Mischke 1925 (228)	
Monocotyledons	Worseck 1922 (383)	Franz 1928 (96)
	Ankermann 1927 (6)	
Dicotyledons		
Ranales branch	Mez and Lange 1914 (219)	Zarnack 1927 (387)
	Lange 1924 (172)	
	Saltzmann 1924 (304)	
Rosales branch	Kohz 1923 (151)	Nay 1927 (248)
	Saltzmann 1924 (304)	Schwandt 1930 (325)
	Raeder 1930 (296)	
Centrosperm branch	Malligson 1922 (192)	Helwig 1927 (123)
	Bitzek 1928 (31)	Müller 1932 (242)
Parietales branch	Mez and Preuss 1914 (220)	Wermund 1928 (371)
	Reuter 1926 (292)	Görner 1929 (105)
Columnifer branch	Hoeffgen 1922 (129)	Bärner 1927 (17)
	Ruff 1930 (302)	Bry 1930 (38)
		Hoepner 1930 (130)
Sympetalae branch	Alexnat 1922 (4)	Huhn 1927 (131)
		Blass 1930 (35)
		Scholz 1930 (318)
Miscellaneous dicotyledons	Raeder 1924 (286)	Zalkowitz 1932 (386)
	(Contested positions of such forms as: *Adoxa*, *Polygala*, *Arctostaphylos*, *Empetrum*, *Hypericum*, *Viscum*)	(Sarraceniaceae, Nepenthaceae, Droseraceae)
II. *Published editions of the Königsberg Stammbaum*	Gohlke 1913 (104)	
	Mez 1922 (210)	
	Mez and Ziegenspeck 1926 (222)	
	Gortner, R. A. "Outlines of Biochemistry" (N. Y.)	

TABLE 1—*Concluded*

	KÖNIGSBERG	BERLIN
III. *Works correlating the Stammbaum with other systematic disciplines*	Ziegenspeck 1925 (389) (With anatomy) Ziegenspeck 1925 (388) (With palaeontology) Ziegenspeck 1927 (391) Mez 1936 (261b) (With morphology) Mez 1928 (216a) (With cytology)	
IV. *Techniques and theories*		
General	Mez 1922 (210), 1924 (211), 1926 (213, 214, 215, 216) Mez and Ziegenspeck 1925 (221), 1926 (222) Ziegenspeck 1926 (390) Steinecke (- Mez) 1925 (212) Steinecke 1925 (334)	Gilg and Schürhoff 1926 (101) Gilg and Schürhoff 1927 (102, 103)
Use of various tissues	Wilkoewitz and Ziegenspeck 1928 (380)	Burger 1929 (39) Wartenberg 1930 (359)
Artificial sera	Mez and Ziegenspeck 1925 (221)	Sasse 1928 (308) Nahmmacher 1929 (245)
Function of lipoids, and other artefact reactions	Becker 1932 (22)	Meyer 1929 (209)
Use of purified proteins		Arms 1928 (7)

reaction) reactions have been applied to a great many species of plants representing most of the plant families from bacteria upwards. The results have been gathered in the form of a genealogical tree of plant relationships, or serological "Stammbaum." The work commenced with a blocking out of the main serological subdivisions of the plant kingdom by Gohlke (104), and then each subdivision was made the subject of intensive research by one or more collaborators. For the purpose of ready reference to an otherwise rather complex literature, Table 1 provides references to studies of the various taxonomic units. It is to be regretted that the complete Stammbaum has been patented and hence cannot be reproduced here in its entirety, but Table 1 includes references to its published editions.

In an earlier part of the present paper the techniques of the Königsberg investigators have been dealt with in detail, and it is hence unnecessary to consider them here. A word should be said, however, as to the method of construction of Stammbaum branches.

The customary procedure in Königsberg consists in the selection of a number of representatives of the group to be studied, an attempt being made to select forms representing as many important subdivisions of the taxonomic scheme as possible. Each of these is used for immunisation, and the reactions obtained when such sera are tested against many kindred species are plotted in the form of circles of greater or less radius according to strength of reaction, and concentric to the serum, which represents a "reaction centrum". By observing the degree of intersection of circles from two centra, it then becomes possible to orient a test species with regard to the two centra involved. More centra and more test species are added until the whole forms a logical system of coordinated relationships.

During the development of the Königsberg work new techniques came to replace

older ones, and hence it became desirable to re-test the groups studied earlier; an examination of the table will show that as a rule the relationships of each group have been confirmed at least once in comparatively recent times (the Königsberg techniques reached essentially their present form in 1924–1926). It will be recalled that Mez considered as significant only results which have been reciprocally tested by both precipitin and Mez' reactions, in which the controls are faultless, and in which the investigator has performed and evaluated all reactions without knowing the identity of his test extracts.

It is highly significant that, although the studies of the Königsberg workers have frequently overlapped, the cases of disagreement are comparatively rare. One such case was that of *Ginkgo* which was erroneously misplaced in an early study. Mez' critics have made much of this relatively isolated case, often losing sight of the fact that the error was discovered and rectified in Mez' laboratory before it had been commented on by outside investigators.

Mez lays considerable stress on the qualification that, besides being purely objective and faultless in execution, the method must produce results which form a logical systematic arrangement. There is no question that the Stammbaum as published in 1926 (222) represents such an achievement. The work has excited both favorable and adverse criticism, the critics forming three groups, viz. plant systematists such as von Wettstein, Diels, Stolley, and Heintze who have viewed the work purely from its systematic results, the Berlin-Dahlem workers associated with Gilg and Schürhoff who have attempted a re-testing of the Königsberg Stammbaum by the use of modified serological methods, and a number of independent critics, as for example Boom, Moritz, Kōketsu and Kojima, Krohn, and Hannig and Slatmann, who have also separately re-tested taxonomic units of plants either by the Königsberg techniques or by modifications of or supplements to these.

As regards the criticisms of plant systematists, there is on the whole a tendency for these to regard the Königsberg system as a stimulating attack on many old controversial taxonomic problems. It is not unnatural for the serological Stammbaum to be looked on with some suspicion wherever the results do not strictly conform with the opinions of the critic in question and for it to be accepted uncritically as an excellent confirmation whenever it does substantiate the opinions of systematists. Systematics is so frequently based on opinion rather than on objective demonstration, that it is to be expected that different systematists should differ in their taxonomic alignments, and under the circumstances no such system as the Königsberg tree could be hoped to satisfy all opinions, many of which are conflicting or diametrically opposed. In all the important relationships of the Königsberg tree which have been criticized by certain systematists, Mez has shown that other systematists agree with the relationships detected serologically. Moreover, in each of the recent Königsberg studies, the group investigated has been worked over from the standpoint of morphology as well as of serology, and the same logical sequences have arisen from both disciplines. The attack on the Königsberg work by the Berlin investigators also has had its effect on the acceptance of the Königsberg Stammbaum by systematists, the latter being unversed in serological techniques and often accepting as just criticism the not wholly unprejudiced and sometimes

intemperate contradictions of the Berlin school. A consequence of the unfortunate controversy between the two schools has been to impair the repute of the sero-systematic method, although the more impartial and soundly scientific attitude of such recent workers as Boom in Holland, Moritz in Kiel, and Krohn in Finland have fortunately had a stabilizing effect on the disturbed state of sero-systematics in Europe. The reception of the Königsberg Stammbaum by plant systematists of the older school may be illustrated by the following representative cases.

Von Wettstein (372) has expressed the view that serology gives systematic results well worthy of consideration by systematists. The methods are held to be still in a relatively primitive state, and accordingly von Wettstein felt that sero-diagnosis by itself cannot be considered decisive, but must be controlled by comparative morphology. The chemist, Molisch (228a), and the animal serologist, Boyden (36a), have come to similar conclusions. Stolley (337, 338, 339) in a series of rather vituperative notes has bitterly criticized the Stammbaum which in certain cases fails to accord with Stolley's views as to the phylogeny of the forms in question. Mez has shown, however, that with regard to the systematics of the disputed forms Stolley stands alone in his opinions, opposed not only to the Königsberg findings but also to the contentions of systematists in general. For example, the Stammbaum, in accordance with the views of nearly all systematists, indicates that the mosses preceded the Pteridophytes in origin, while Stolley objects to the Stammbaum because of this. Heintze in Sweden finds that "serodiagnostic investigations have hardly contributed to a clearing up of the relationships within the Cormophytes. By and large, they only 'confirm' the errors of Engler and other authors" (121). But this serves to illustrate again that the discrepancies in opinion among the systematists themselves are such that it becomes impossible for any system to meet with the approval of all. Diels in Berlin (67) feels that the techniques at present are relatively inadequate but that it is entirely possible that the serological methods may in time come to afford very considerable aid to systematics.

Thus, on the one hand, we find the Königsberg Stammbaum accepted only with considerable reservation by many systematists. On the other hand, Mez maintains that the serological system is more fundamental and reliable than others based on less empirical criteria. He bases his position on his contentions that: (1) serology is empirical while most of the other approaches to systematics are necessarily based on opinion, (2) serology deals with the relationships of the species-specific proteins, presumably of the proteins of the chromatin, while morphology deals with far removed consequences of chromatin activity, and (3) morphological systems show frequent unnatural "convergences of evolution" while these have never been seen in serological studies. However, Mez' original position has been conservatively stated:

> "With emphasis I take Janchen's standpoint that the serum reaction in itself asserts nothing regarding relationships but only teaches one regarding protein similarities, that is, physiological-chemical agreements and discrepancies. Under no conditions have we in the serum reaction a systematic panacea, but we see a new method of investigation open, the results of which must be compared with other systematic methods, and not considered as more valuable, but only as equally deserving of consideration in the balance." (Transl. from 217.)

With the more complete development of the method, Mez has come to place

more and more reliance on it because of its value as seen in an empirical way. Many doubtful and controversial relationships have yielded satisfactorily to the serological method (222).

On the whole, the results of the Königsberg investigations seem to be highly deserving of consideration from a systematic viewpoint. Theoretically the method appears more fundamental than any other systematic approach; practically this is limited by the complexity of the technique and the not infrequent occurrence of artefact disturbances. Yet with the advance of phytoserological techniques these artefacts are coming more and more under control, and it is noteworthy that even the results of the antagonistic Berlin school, in spite of numerous reported systematic discrepancies, nevertheless include many findings wholly in support of the Königsberg Stammbaum (e.g. 17).

The Berlin "refutation" of the Königsberg "Stammbaum." In 1926 a group of students working in association with Gilg and Schürhoff in Berlin-Dahlem commenced a series of studies ostensibly for the purpose of re-testing the correctness and value of the Königsberg Stammbaum. Each of the main branches of the Spermatophytes was re-worked by one or more investigators, and in addition a number of studies were undertaken for the purpose of testing techniques as well as results. Table I shows in concise form the scope of these studies.

It is indeed remarkable that, while thirty investigators at Königsberg all found the serological methods suitable for the study of plant relationships, a score of workers at Berlin have nearly all come to opposite conclusions, stated in the following terms of Gilg and Schürhoff:

"Die Serodiagnostic ist für die botanische Verwandtschaftsforschung völlig unbrauchbar. Wir sind der Meinung, dass schon diese einzige Ueberlegung vollkommen genügt, um zu zeigen, das man an den Ausfall der unspezifischen Reaktionen keine weitgehenden Deduktionen anschliessen darf" (102). To be sure, this dogmatic position is somewhat qualified by certain of the Berlin workers. Blass, for example, while recording a number of apparent contradictions in his tests, still considers the method helpful in phylogeny, although he does not feel that a Stammbaum can be erected on such reactions alone. He, like certain other Berlin workers, has drawn rather far-reaching conclusions from his serological tests in spite of his refutation of the method. ("Out of the totality of my relationship reactions, especially of the behavior of the Cucurbitaceae and Umbelliferae, I believe it very apparently probable that the division of the Dicotyledons into the two classes Choripetalae and Sympetalae no longer corresponds to the true genetic relationship of the families classified thus.") (Transl. from 35.)

All of the Berlin investigators find serology satisfactory for the identification of plant antigens, while doubtful of its value in relationship study.

That two large groups of investigators working in the same field could come to such contradictory conclusions is a paradox demanding interpretation. Gilg and Schürhoff contend that the Königsberg Stammbaum is not purely objective but that it has been influenced by the preconceived opinions of the Königsberg workers. Mez and in addition certain independent workers, notably Boom, maintain that the Berlin work is not free from prejudice. Assuming that neither contention is correct, that the work of both schools is purely objective, there still remain differences in the methods of the two groups sufficient, in the writer's opinion, to account for this discrepancy. These differences in methods concern the execution of the tests and the evaluation of the results, the latter probably being most important in the present connection.

The techniques of testing differ materially in the two schools.

In Königsberg the precipitin flocculation test and Mez' reaction are used in parallel, while in Berlin neither has been adopted, and instead the precipitin ring test is the chief reaction. In Königsberg it is customary to preëxtract all antigenic materials with alcohol, while in a large part of the Berlin work this preëxtraction has been avoided because of an assumption that alcohol preëxtraction will denaturate the antigens. The period of incubation of precipitin mixtures is 12 hours at 37°C. in Königsberg, and from 10 minutes to about 1 hour at room temperature in Berlin. The serum titers in Königsberg are relatively high, of the order of 1:25,000 or 1:50,000 in many cases, while in Berlin they are more frequently of the order of 1:1000.

Mez' rigid system of controls has been described in a previous section. Emphasis should be laid on the requirement that reactions must be reciprocally confirmed before they are incorporated into the relationship studies, and that any experiment must be rejected in which the controls, particularly the tubes containing antigen + normal serum, are faulty.

The Berlin objections to the Königsberg results are based chiefly on the observations in Berlin that normal serum controls are often faulty, that non-specific positive reactions frequently appear, connecting distinctly unrelated species, while closely related forms are not infrequently negative with one another, and that the reciprocal reactions often do not confirm one another.

According to the Königsberg incubation schedule, normal serum frequently precipitates in the presence of various antigenic extracts. This is particularly true if the antigens are not preëxtracted with alcohol. The occurrence of such normal serum precipitates, which can be greatly lessened by adequate preëxtraction and a shortened incubation time, and which can be entirely eliminated by absorbing all extracts with normal serum (152, 153, 154) or in certain cases at least by extracting in phosphate buffers (118, 119, 330), in no case serves as a valid objection to the Königsberg work if for no other reason than that all Königsberg experiments in which normal serum cloudings appear are rejected. That the Berlin workers often obtain negative results with closely related forms is doubtless due to the low titer reactions, the ring test observed after 10 to 60 minutes not being as sensitive as the flocculation test, even though the incubation time for the latter is materially shortened. The positive non-specific reactions of the Berlin workers in many cases are due to an insufficient preëxtraction of the antigens thus failing to remove such non-specific precipitating substances as the lipoids. Mez also uses a variety of techniques for removing tannins, glucosides, alkaloids, organic acids, gums, resins, and other substances which may produce non-specific results, and such techniques have rarely been incorporated into the Berlin work.

Furthermore, the occurrence of an isolated non-specific reaction, even if due to an unrecognized variable, need not prove detrimental to serosystematic work since its presence will usually be detected at once by the fact that the reciprocal tests involving it will not harmonize, and hence, according to Mez' requirements, reactions with such an antigen will be automatically discarded.

We have, then, two distinct types of reactions to consider, the specific protein reactions which are found by both schools to harmonize with systematic conceptions, and non-specific reactions which lead to impossible relationships. Both types of reactions have been obtained by both schools of workers. But here lies the essential difference in viewpoint. The non-specific reactions in large part are removed by the Königsberg techniques and those which remain become eliminated under the rigid qualifications of the Königsberg control system. There remains a residuum of reactions which form a

logical, coördinated taxonomic system. In Berlin, on the other hand, the non-specific reactions are more frequently obtained than in Königsberg because of the excessive caution in preëxtraction technique; in general they are not automatically eliminated by reciprocity requirements; and finally the emphasis on them in interpretation tends to overshadow the truly specific relationship reactions obtained so frequently in Berlin. Herein, in the writer's opinion, lies the basis for the misunderstanding between the two schools, a misunderstanding not necessarily resting on a subjective coloring of the results in either case, but upon such differences in technique and in control requirements that the works of the two are hardly comparable. In no case is it to be concluded that the Berlin work constitutes a decisive refutation of that of the Königsberg school.

Re-testing of the Königsberg studies by independent investigators. The Königsberg work excited immediate attention among systematists and as early as 1917 Kōketsu 153, 154) and Kojima (152) in Japan proceeded to test the value of the serological techniques on a broad scale. The precipitin tests showed the Dicotyledons to be widely separated from the Gymnosperms in systematic position with the Cycads connecting the Gymnosperms to the Dicotyledons through *Magnolia*. Within the Gymnosperms the results in general agree with those of modern systematics, although with a few deviations. The work of Kōketsu and Kojima is of particular interest inasmuch as they completely eliminated the normal serum artefact reactions by absorbing all extracts with normal serum before testing. The isolated non-specific reactions obtained may well be due to the excessive presence of tannins in the Gymnosperms, no mention being made of their removal, as well as to the fact that reciprocal confirmation was not held to be an essential qualification for the acceptance of a result as valid.

Arzt in Leipzig (8) applied the precipitin technique to a study of the relationships of the Gramineae. While his work in some respects seems to lack sufficient background in the essentials for serological research, the results appear to be valid. His work indicated that barley is nearer to oats than to wheat, and that among the barleys there are minor serological differences, the latter, however, being rather weak and not entirely convincing, in accordance with the failure of most other investigators to separate varieties of a species by the precipitin test.

The greatest weakness of the Königsberg methods appears to be the reliance on the value of Mez' and Ziegenspeck's artificial sera in the most recent Königsberg work.

Sasse (308) and Nahmmacher (245) in Berlin pointed this out in 1928 and 1929, respectively, and in 1928 Grijns (116) in Holland and Eisler (75) in Germany also came to the conclusion that this feature of the Königsberg technique is not reliable. Boom in Holland (36) has likewise arrived at the same conclusion. Boom's work represents one of the most satisfactory attempts at a re-testing of the serodiagnostic method. He conservatively accredits the value of the serological Stammbaum while emphasizing the need for morphological control of its results. So many factors influence the strength of serological tests that in general they are considered of value chiefly from their qualitative rather than their quantitative features. This fact has been emphasized by Boom in connection with his sero-systematic studies, and serves as a major limitation to the serological approach to plant systematics.

An interesting contribution was made in 1928 by Hannig and Slatmann (330, 118, 119). They found that the addition of phosphate to the extractant serves to eliminate the normal serum artefact reactions so frequently observed especially in Berlin, although there may be some question whether this technique does not in itself introduce artefact reactions of another sort. Thus, although they obtained some results bearing out systematic concepts, numerous non-specific reactions were seen, and from their tables of reactions it appears that in many cases these excep-

tions were of combinations in which the reaction was properly negative until phosphate was added.

In 1931 Silberschmidt (327) analyzed the Berlin-Königsberg controversy and pointed out in particular the necessity for adequate preëxtraction of extracts, constancy of electrolyte constitution of extract dilutions, a complete control system, the avoidance of the zone phenomenon by the proper adjustment of proportions of antigen and serum, and a continuation of the observation time beyond the interval usually used in Berlin. Jaretzki (138) in a review of the subject the same year concluded that the value of the serological method in systematics consists chiefly in a confirmation of the deductions from other disciplines and in a stimulus to re-investigation in those cases in which serology fails to accord with morphological findings.

Moritz in Kiel has made a valuable contribution in his analysis of the value and limitations of the Königsberg work (230, 231, 232, 235) and in his development, with vom Berg, of the anaphylaxis (Schultz-Dale) technique as an aid in sero-systematics (233, 234, 236, 237, 239, 24). His criticism particularly regards the facts as pointed out that the Berlin technique is much less sensitive than that in Königsberg, and that the Berlin preëxtraction is inadequate and leads to artefact reactions. The Königsberg results are considered error-free provided that experiments in which the normal serum controls are faulty are rejected (a Königsberg requirement). His experiments with Mez' artificial sera, like those of Sasse, Nahmmacher, Boom, Eisler, and Grijns, were entirely unsuccessful.

An important feature of Moritz' contribution lies in his analysis of the plant antigen in terms of complexes of single antigen units or partigens. The relationship reaction is interpreted as a mosaic of identity reactions of such partigens, the strength of the total reaction depending on the frequency and concentration of common partigens and partigen-antibodies. This conception is elaborated and illustrated by means of Moritz' and vom Berg's anaphylaxis tests which will be mentioned subsequently.

Krohn in Finland (162) has recently published an excellent study on the serological affinities of the Sympetalae branch of the Königsberg Stammbaum. This author first shows that the Berlin repetitions of the Königsberg work in this group are supported by data which are entirely inadequate and in which, because of numerous faulty control reactions, the results obtained are neither significant nor comparable with those of Gohlke and Alexnat in Königsberg. He then proceeds to investigate the group serologically in elaborate detail and following precisely the techniques of Mez. The results obtained offer a detailed confirmation of the Königsberg results with this group and show at once the objectivity of the Königsberg approach and the inadequacy of the Berlin "refutation". An important contribution is Krohn's extensive data showing that a single species may be taken as representative of its family, serologically.

Sero-systematics of animals. Studies analogous to those of Mez have been executed for the purpose of determining the serological relationships of animals. The classic introductory work of Nuttall has already been mentioned. Subsequent workers have published accounts of the serological relationships of crustacea, lepidoptera, helminths, amphibia, birds, cattle, and other mammals. The literature concerned has been adequately reviewed by Boyden (36a).

Isolated sero-systematic investigations. In 1923 Rives (294, 295) in France applied the precipitin technique to a number of varieties of the grape and found that a failure to graft successfully was accompanied by a marked serological difference, while varieties which grafted successfully were similar according to the test. A similar type of work was undertaken by Green in 1926 (114), in an attempt to study the possible correlation between serological affinity and grafting compatibility in *Citrus*, Rosaceae, and Solanaceae, and the precipitin test showed that such forms as grafted readily reacted positively, and that poor graft combinations were more distantly related serologically.

Maschmeier in 1927 (196) was unable to separate varieties of the potato by precipitin reactions according to the Berlin methods, in accordance with the failure experienced by other workers in the separation of such closely related forms. It is now known, however, that the latent mosaic virus of potato is present in practically all potatoes in the field and that potatoes infected with this virus

(which may cause no symptoms of disease) give a strong precipitin test specific for the virus. Hence, Maschmeier's failure to separate potato viruses by the precipitin test may be due in part at least to the strong virus reaction, which would tend to obscure specific potato protein reactions. Kato and Maruyama (144) likewise failed in the separation of rice varieties by ordinary methods, although a saturation technique afforded a clear cut differentiation of Japanese, Chinese, Korean, and Formosan rices. Moreover, Nelson and Dworak (249, 251) have reported experiments in which it was possible to separate wilt-resistant from wilt-susceptible flax varieties by using the seed globulins in precipitin tests, although it was impossible to do so if extracts of the whole seeds were used, implying that the varietal-specificity of the precipitin reaction with these seeds is confined to the seed globulins and masked by reactions of other proteins common to all varieties of the species. The writer, in attempting to differentiate genera and species of the Solanaceae by the Schultz-Dale anaphylactic reaction, has also found that the globulins of the leaves of these plants show much more specificity serologically than do the albumins or the total-protein extracts (unpublished).

A very interesting aspect of serosystematics is brought out in the work of Baldwin, Fred, and Hastings who in 1927 (13) showed that the serological reactions of numerous legumes bear a strong and consistent relationship to the differential susceptibilities of these legumes to various strains of nodule bacteria. Precipitin and anaphylaxis reactions were used with comparable results, and it was found that all groups of legumes inoculable with the same nodule bacteria were serologically homogeneous, and that serologically similar groups were nearly always cross-inoculable with the same bacteria. No serological affinity was found between the legume seed-proteins and the proteins of the bacteria infecting the same legumes.

According to Elmore (80, 81), the complement fixation test satisfactorily distinguishes *Euglena*, *Chlorella*, and *Chlamydomonas*, and, while different green strains of *Euglena* were not separable, green and colorless forms of the same strain were distinguishable (cf. Rosenblat-Lichtenstein, 301, and Lieske, 176). The complement fixation test also serves to differentiate *Aspergillus niger* from *A. fumigatus* in the experience of Corpaci (61), although the agglutination and precipitin tests proved unsatisfactory. Matsumoto (197, 198) had a similar experience using 23 species and strains of *Aspergillus*, the homologous reactions always being strongest, and heterologous combinations reacting more weakly. *Gloeosporium* and *Penicillium* were found to be too distantly related to react with *Aspergillus*. The method was accordingly recommended for the differentiation and identification of fungi. Similarly, the precipitin test served for a satisfactory differentiation of some yeast species, although the complement fixation test is felt desirable in this connection (266). Using her "cytotoxin" test (see p. 42), Sauer has recently shown that *Euglena gracilis* strains may be serologically separated into a group showing luxuriant growth and a group with delicate growth (308a). Green and colorless forms of the same alga were inseparable by this method, although Sauer earlier reported (80, 81) a serological difference between such strains.

In 1928 the complement fixation technique was used by Coons and Strong (60, 59) in a study of *Fusarium* species, and while the method was considered too laborious and exacting for general appli-

cation, its value in special investigations was indicated. Thus, *F. radicicola* and *F. Martii* were readily distinguished from each other and both from *F. conglutinans*, and in one case even a varietal distinction was possible. Link and his associates likewise have been carrying on work in the serological differentiation of *Fusarium* and other fungi (180, 181). While the *Fusarium* species showed a strong common group reaction, various genera of the Pezizales were separated from the Hypocreales, and certain of the latter from one another. A number of difficulties were encountered, some homologous tests being constantly negative and some group reactions being so extensive as to preclude satisfactory genus and species differentiation, and Link's recent work on the serology of the fungus carbohydrates (376) indicates that the serological activities of these substances may be a cause of this latter difficulty. Beck in Canada (21) also had certain difficulties in the separation of Ustilaginaceae by the precipitin test, but both Link and Beck indicate that the method, although limited with respect to the fungi, offers possibilities in the determination of affinities among species and genera of doubtful systematic relationships. Canonici (41) did not find the serological tests as delicate as biochemical tests in the differentiation of 209 strains of *Aspergillus niger*, although positive group reactions were obtained with both the precipitin and agglutination methods.

In America the higher plants have also been studied serologically. In 1929 Nelson and Birkeland (250), using Mez' techniques and absorption precipitin tests on the globulins of wheats, found the serological characterization of the wheats studied to be correlated to a certain extent with such genetic characteristics as resistance to stem rust and yield, so that within limits the serological technique may be used as an aid to the plant breeder in the early orientation of genetic variants with respect to their genetic characters. In a similar but more extensive study in 1931, Edgecombe (71) found a close parallel between the wheat globulin precipitin test and relationships of wheats as measured by their resistance or susceptibility to *Puccinia*, although the chromosome numbers did not agree with either of the other criteria.

Thus, taken as a whole the serological approach to plant systematics as first elaborated in Königsberg has been supported, although qualified to some extent, by numerous independent investigations, the inadequacies of the Berlin "refutation" have been made evident, and the net result indicates that serosystematics is on a sound basis provided that rigorous care be taken to exclude non-specific reactions. Some methods of such exclusion have been pointed out. The whole subject of serology is still in an imperfect state and this applies in particular to that of the complex plant antigens. But even with this qualification, emphasizing as it does the need for a further development of the phytoserological techniques, from a purely empirical standpoint serology has proven itself of value not merely as an adjunct to other systematic procedures, but as an approach with advantages appertaining to no other systematic method, and with disadvantages which are rapidly becoming minimized with the advance of technical knowledge.

The sero-systematics of the plant viruses. Beale has shown (279, 280, 281) that tobacco mosaic plants possess precipitinogenic properties apart from those of healthy host plants, and this has been repeatedly confirmed. Dvorak (70) noted that a similar situation exists with regard to mosaic potato plants, a fact which has been confirmed by Gratia (106, 108–110) and others. Beale likewise observed that tobacco mosaic immune serum fails to precipitate

in the presence of extracts of plants containing Sudan grass mosaic, Hippeastrum mosaic, lily mosaic, Abutilon mosaic, peach yellows (281), and mosaics of *Asclepias*, *Datura*, and *Solanum* (283). In none of these cases, however, were immune sera prepared with respect to these heterologous viruses in order to demonstrate a true cross-specificity of the tobacco mosaic reaction. Gratia (106, 108–110, 112), however, showed that tobacco mosaic serum precipitates tobacco mosaic extract but not a potato mosaic extract, and that, reciprocally, potato mosaic serum precipitates potato mosaic extracts but not tobacco mosaic extracts. He was unable to prepare a serum reactive with the veinbanding virus of potato.

The question of specificity was carried somewhat farther by Birkeland (29) who found that tobacco mosaic extract, cucumber mosaic extract, and "spot necrosis" extract are each precipitated by its specific antiserum but not by the other two heterologous antisera. Precipitating sera for Wingard's tobacco ring spot were not obtained. Spooner and Bawden have recently confirmed Birkeland's work in part (331). Manil in a survey of tobacco leaf necroses (193a) found that several types of such disease were serologically related to tobacco mosaic, although the majority of plants tested failed to react with either tobacco mosaic or latent potato mosaic virus sera. Recently, Verplancke in Belgium (356, 356a) has claimed to have prepared a precipitating serum for a sugar beet mosaic which was negative against various other virus materials, and Matsumoto (201a) has been able to distinguish two similar-appearing *Petunia* mosaics serologically, since saps of one type showed a tobacco mosaic reaction, while the other type was serologically inactive.

The writer has immunized rabbits with extracts of tobacco mosaic, cucumber mosaic, and tobacco ring spot, and obtained sera from each which specifically neutralized its homologous virus but neither of the two heterologous viruses in each case (47). Continuing and extending this earlier work, attempts were next made to utilize the serological tests for the purpose of aiding in the study of the classification of the plant viruses. The results have been encouraging and may be briefly summarized as follows (49, 50, 54):

Up to the present 60 viruses and virus strains have been tested, and about half of these give specific serological tests. The reactive viruses may be classified in 8 groups. It may be stated parenthetically that this step toward classification rests in largest part on precipitin tests, and that in nearly every case the precipitin tests have been reciprocally confirmed by using each virus both as antigen for serum preparation and for testing, respectively.

1. *Tobacco mosaic group*. This group comprises field-type tobacco mosaic, Holmes' attenuated and symptomless strains of tobacco mosaic, Jensen's yellow and necrotic tobacco mosaic isolants, aucuba mosaic of tobacco, and Johnson's tobacco virus VI. It may be noted here that the serological evidence regarding this large but uniform group of tobacco mosaic segregates is in entire conformity with their homogeneity as shown by thermal death point, infectivity, host range, and other characters so far as these have been studied. According to the tests of Manil (193a) and Matsumoto (201a), certain Belgian necrotic diseases of tobacco and a Japanese *Petunia* mosaic or components of these also belong in this group.
2. *Latent potato mosaic group*. This group includes the potato ring spot and spot necrosis (or a constituent of the latter if it is a mixture) used by Birkeland, as well as the attenuated spot necrosis employed by him. It includes also the typical latent virus of potato (X-virus and D-virus (331) of potato in the European literature), Hyoscyamus IV virus (112), potato mottle, and British Queen streak. All of these viruses show a greater or less necrosis of the ring spot type, but at least one, the potato mottle, represents a mixture of two or more latent virus strains, from which has been segregated one, a relatively non-necrotic, almost symptomless type, and another of the tobacco ring spot type, each of which behaves exactly like the others serologically. The viruses in the tobacco mosaic group are so closely related serologically that there is very good ground for considering all of the viruses of the group as strains of the same virus type, and the same may be said for the viruses of each of the other groups, respectively. Even the complement fixation reaction shows them to be remarkably uniform as groups. Spooner and Bawden (331) have recently found a number of strains of this group to be indis-

tinguishable by precipitin, complement fixation, and neutralization tests, the reactions being independent of solanaceous host species. Rugose mosaic of potato consists of a mixture of a strain of the veinbanding virus + any strain of latent mosaic.

3. *Potato veinbanding virus* (Y-virus of potato in the European literature) and cucumber mosaic. These two viruses have shown an unexpectedly close serological relationship, so close that they are considered strains of the same virus type. This finding is corroborated by their similarity in infection of the cowpea (*Vigna sinensis*). Valleau's delphinium virus 10729 also belongs in this group.
4. *Aucuba mosaic of potato.*
5. *Mild mosaic of potato.* (Reaction weak.)
6. *Tomato etch and severe etch.* These two viruses are evidently strains of the same virus type as shown by their cross precipitin tests, and are distinct from the other groups here listed. On one or two occasions cross-reactions with tobacco mosaic virus juice were obtained, but these were probably due to a contamination of the material, as later tests have shown the etches to be entirely distinct from tobacco mosaic.
7. *Wingard's tobacco ring spot* (green and yellow strains).
8. *Osborn's pea mosaic viruses # 2 and # 3*
9. Verplancke (356) has recently reported positive precipitin tests with serum and extracts of *beet mosaic.* His serum was negative toward extracts of beets with yellows, of mosaic *Dahlia*, of potato with mottle, crinkle mosaic, mild mosaic, streak, streak mosaic, and leaf roll, of mosaic *Pelargonium*, or of *Monstera* with *Anthurium* mosaic. It cannot be determined from Verplancke's scanty data whether or not this represents a distinct virus type from those enumerated above (beet mosaic is sometimes associated with cucumber mosaic in the literature). The question seems justified whether this worker actually was dealing with a virus reaction, since the data are meagre, no mention is made of healthy beet or normal serum controls, the incubation period was excessive (24 hours at 37°), dilution series were not employed, and the serum precipitated beet mosaic extract which had been heated nearly to boiling, in contrast with the findings regarding all other viruses heretofore tested (51). Gratia and Manil (113) were unable to confirm this work of Verplancke's.

Each of these 8 groups of viruses is distinct serologically from the other groups. Within the tobacco mosaic group, the ordinary precipitin technique fails to show differences among the various strains of this virus, although recently it has been shown that a technique of precipitin absorption serves to reveal minor serological differences among the strains of tobacco mosaic virus (54). The same has been shown for the latent potato mosaic group.

We thus see that a fair start has been made toward a study of the relationships of the plant viruses by serological methods; the results obtained are in harmony with the results of other approaches to the question of virus classification, and serology accordingly bids fair to be a useful and impartial aid in this difficult field. As has been pointed out, however, (50) certain viruses, notably those transmitted with difficulty or not transmissible mechanically, have not yet proved good material for serological study. Thus Gratia and Manil were unable to prepare satisfactory antisera for the viruses of potato veinbanding, beet mosaic, and sweet clover mosaic (113), Beale failed to obtain sera for a number of viruses, and the writer could not obtain precipitating sera for the viruses of peach yellows, aster yellows, and certain other diseases. In general, the viruses which have thus far proved amenable to serological examination have been those which are readily transmissible by mechanical means, and which are relatively resistant to physical and chemical agencies. It is to be hoped, however, that improvement of the techniques of handling viruses *in vitro* will extend the present limit of application of serological methods to the plant viruses.

III. SEROLOGY OF PURIFIED OR ALTERED PLANT PROTEINS

Investigations of Wells, Osborne, and their associates. Osborne's notable success in

the purification and crystallization of many plant proteins at a time when relatively few animal proteins were obtainable in a comparable state of purity (263) quite naturally led to a particular interest in the serology of such preparations. In coöperation with Osborne, Wells, Jones, White, Avery, Lewis, and others have carried out intensive studies in this field, combining a study of the serology of the purified proteins with investigations of their chemical nature and differentiation.

The various techniques employed consisted of the gross-allergy test, precipitin reaction, complement fixation reaction, Schultz-Dale anaphylaxis test, bronchospasm and skin anaphylaxis reactions, and the Abderhalden reaction. The results of all these tests were in general agreement, the Schultz-Dale and bronchospasm tests proving most sensitive and specific, the precipitin, complement fixation, and gross-allergy tests satisfactory but less delicate, and the Abderhalden reaction on the whole in conformity with the other reactions but more subject to aspecific, distorting results.

In general, all of the purified plant proteins are antigenic, but they differ considerably in their antigenic power. Those which are relatively insoluble in the body fluids (e.g. the alcohol-soluble zein), show a tendency to be least active antigenically (361). The plant proteins show a degree of reactivity entirely comparable with animal antigens, and the titers obtained are often relatively high, e.g. complement fixation to 1:1,000,000 with hordein, to 1:100,000 with wheat gliadin and squash globulin, precipitin reaction to 1:100,000 with edestin (168). In immunisation the serum first shows a positive complement fixation test, then in addition a precipitin reaction, and finally also the anaphylaxis reaction. Likewise, each reaction is sharply specific when it first appears, but as the titer increases reactions occur which involve proteins from species more or less closely related to that providing the immunisation antigen (168).

With regard to the serological relationships of the plant proteins, the following facts have been determined. Zein and gliadin are entirely distinct (361); the gliadins of wheat and rye are similar or identical, as are the legumins of pea and vetch, while vicilin is relatively close to the latter two (365). Gliadin of wheat and rye and hordein of barley are closely related but distinguishable; glutenin shows affinities to gliadin although serologically and chemically distinct (366). Edestin is entirely distinct from gliadin and both from the globulins (374). Within the wheat seed there are at least 5 proteins, globulin, gliadin, glutenin, leucosin, and natural proteose, all distinct serologically (382). The globulins from squash and canteloupe are serologically and chemically identical (140). The alcohol-soluble prolamines of wheat and its relatives all show serological relationship, as do those of corn and its relatives, although a relationship of the wheat-group prolamines with the corn-group prolamines is lacking (174, 175). It would thus seem that the Königsberg technique of alcohol preëxtraction may in some cases remove proteins of value in the relationship reaction. In the legumes also the serological reactions follow closely both the chemical constitution of the proteins and their botanical relationships (364).

We thus see that, while a single species of plant may contain several serologically and chemically distinct antigens, and while also two closely related species may contain chemically and serologically indistinguishable antigens, nevertheless, considering either the protein complex of the plant as a whole or single protein constituents of the complex, the serological reactions obtained by precipitin, anaphylaxis, and complement fixation techniques correspond closely to the results of chemical analysis so far as the latter is possible, and to the botanical relationships of the plants yielding such proteins (cf. 263). The often-repeated statement of Wells and Osborne that "the anaphylaxis reaction depends on the chemical structure of the protein molecule and not on its biological origin" (369) implies a contradiction which actually does not exist, since their own later work has shown adequately that serological relationships, chemical relationships, and botanical relationships of the purified plant proteins all go hand in hand. The plant sero-systematists have shown the correlation between serological affinity and botanical affinity of plants, and Wells and Osborne and their colleagues

have supplied the connecting link between these two: chemical relationship.

Isolated researches with purified or altered plant proteins. Previous to the first researches of Wells and Osborne on the serology of purified plant proteins, a few scattered attempts had been made to study the antigenicity of such proteins.

As early as 1901 and 1902, Schütze (320) and Castellani (43) independently showed that "Roborat", a hot-water-soluble derivative of legume protein (*Pharm. Ztg.* Oct. 2, 1900: 770), induces the production of precipitins which are specific in the sense that Roborat sera react against Roborat extract but not against various animal protein preparations.

In 1904 and 1906 (Obermeyer and Pick (261, 262) observed that an iodized or nitrified animal protein loses ability to react serologically with natural protein of the same source, but does react specifically with unrelated iodized or nitrified protein respectively. Tests with the plant protein edestin gave similar results. This work, which has since been repeatedly confirmed, gives evidence that the specificity of the protein molecule rests in relatively minor radicals of the molecule.

Rosenau and Anderson in 1908 (299) successfully employed crystalline edestin and excelsin in the anaphylactic sensitization of guinea pigs, although the results obtained were by no means as clear as those obtained by Wells and Osborne. This type of work was successfully repeated in Germany by Bürger in 1914 (40), who obtained good anaphylaxis reactions with native gliadin, zein, edestin, amandin, and excelsin, but no cross-specificity with acid digests of the same proteins. The serology of the crystalline protein associated with the mosaic disease of tobacco will be considered subsequently.

Nitzescu in 1914 (256), attributing pellagra to a maize intoxication, claimed that the Abderhalden reaction was regularly positive between pellagra sera and zein, but negative between healthy sera and zein. Subsequent investigations have largely discounted this work, however, and Herzfeld in particular (124) has shown that zein is regularly broken down by many types of sera, healthy, gravid, psychotic, etc.

IV. SEROLOGY OF THE PLANT TOXALBUMINS AND OF NON-TOXIC SUBSTANCES AFFECTING BLOOD

An extensive number of toxic plant substances have been detected, purified to a greater or less extent, and studied from their serological aspects. The list includes the following:

Abrin (*Abrus precatorius*), agrostemmin (*Agrostemma Githago*), two or more substances found in *Amanita* species, condurangin (*Condurango blanco*), crepitin (*Hura crepitans*), crotin (*Croton tiglium*), cyclamin (*Cyclamen europaeum*), cytisin (*Cytisus* spp.), digitalin, digitonein, and digitonin (*Digitalis purpurea*), *Herniaria* sapotoxin, papain (*Carica papaya*), phasin (*Phaseolus*, *Vicia*, *Pisum*, *Ervum*, etc.), *Quillaja* sapotoxin (*Quillaja saponaria*), ricin (*Ricinus communis*), robin (*Robinia Pseudacacia*), *Sapindus* sapotoxin, saponin (*Saponaria officinalis*), senegin (*Polygala senega*), smilacin (*Smilax* spp.), solanidin and solanin (*Solanum* spp.), and *Yucca* saponin. Toxicodendrin (*Rhus toxicodendron* and other spp.) and the toxin of the "tutu" (*Coriaria thymifolia* and *C. ruscifolia*), both evidently glucosides, and the toxic alkaloids are considered in the following section.

Although the substances enumerated above are generally referred to as "toxalbumins," in many cases a critical demonstration of their albuminous or even of their protein nature is lacking. However, the term "toxalbumin" is retained in the present discussion, for the sake of convenient grouping and in uniformity with the usage of the earlier literature. Much of the work in this field originated in the laboratory of Kobert, and an extensive paper by him in 1913 (150) affords a comprehensive review of this work.

The plant toxins enumerated above very frequently show characteristic effects on normal blood. The normal washed blood corpuscles of various species are hemolyzed, often at very high titers, by agrostemmin (148), a constituent of *Amanita* (148, 85, 86, 1, 88, 91, 94, 284, 97), crotin (79, 135, 377), cyclamin (350, 148), digitonein and digitonin (148), *Quillaja* sapotoxin (269, 350, 148), ricin (148, 73), *Sapindus* sapotoxin (148), saponin (269, 149), senegin (10, 148), smilacin (148), solanidin (271), solanin (271, 273, 18, 148, 149, 20), and *Yucca* saponin (148), while papain has a lytic effect on serum proteins (65, 66). Hemolysis also occurs with such toxic alkaloids as cocain, atropin, strychnin, and pilocarpin, but not with a number of non-toxic alkaloids. Condurangin and cytisin lack such hemolytic power. Instead of hemolyzing normal blood corpuscles, or often as a

preliminary to hemolysis, the plant toxalbumins very frequently have the power of agglutinating washed corpuscles or of precipitating normal serum, or both. This is true of abrin (57, 287, 147, 122, 72, 79, 173, 149, 135, 171, 377), constituents of *Amanita* and other fungi (88, 91, 93, 94, 95, 284, 97), crotin (79, 173, 149, 171, 377, 57), phasin (170, 287, 375, 28), *Quillaja* sapotoxin (269), ricin (20, 223, 173, 287, 336, 72, 73, 79, 62, 173, 149, 158, 135, 9, 77, 171, 134, 227, 289, 377, 28, 57), robin (149, 173), and crepitin (293, 272), but not of condurangin (141).

The immunization of animals with the plant toxalbumins by feeding or injection of sublethal doses, is customarily followed by a greater or less resistance to or toleration of the poison in normally lethal amounts.

This has been shown in particular for abrin (72, 135, 57), *Amanita* (86, 92, 93, 284), crotin (57), papain (277), ricin (72, 135, 134, 226, 243, 57), robin (72), toxicodendrin (272), and crepitin (293, 272). Such acquired immunity is usually accompanied by the appearance in the serum of neutralizing antibodies, e.g. in the cases of abrin (72, 135, 57), *Amanita* toxin (85, 86, 284), crotin (57), ricin (72, 73, 183, 135, 134, 127, 57), toxicodendrin (272), and crepitin (272, 293). Solanin is claimed by Pohl (273, 274) to induce antihemolysis in the rabbit, but this has been contested by Bashford (18, 19). The ricin antitoxin may have a relatively simple structure since it is resistant to heating for 2 hours at 60°C. or for 1 hour with peptic acid at 35°C. (but not at 60°C.), and is not destroyed by incubating 1/2 hour at 37° in the presence of M/10 H_2SO_4 or M/10 NaOH (134).

With the immunization of animals to the plant toxalbumins and the production of resistance and neutralizing antibodies in the serum, there also frequently appear various other types of serological reactivity.

Thus acquired precipitins for the toxic material have been demonstrated in serum immune to crotin (20), papain (277), and ricin (183, 223, 226, 243, 28, 127, 314, 315), complement fixing antibodies in sera immune to papain (277) and ricin (377), and anaphylactins in the case of papain (276). Furthermore, such immune sera often specifically prevent the hemolysis or hemagglutination of normal bloods by the toxalbumins, as is true for antihemagglutination with abrin (287), *Amanita* (93, 97), phasin (287), and ricin (134, 135, 227; contested in 183), and for anti-hemolysis with *Amanita* hemolysin (86, 93), crotin (135), ricin (134), and solanin (272). The hemagglutinating and hemolytic actions of the phytotoxalbumins are demonstrated with washed red corpuscles. If whole or defibrinated blood is used, the serum present not infrequently prevents hemolysis or hemagglutination, even though the animal has not been previously immunized (20, 223, 134, 227, 173, 97). Peptone also has such an inhibitory effect (287).

The plant hemagglutinins are not necessarily the same as the hemolysins, the normal serum precipitants, or the toxins present in the same extracts (77, 94, 377); in fact each of these manifestations may be caused by a different factor in the plant extract, although in the case of ricin the amount of ricin neutralized by immune serum is equivalent to the amount which is precipitated by the same serum, which suggests that the two reactions may be homologous. Yet many extracts which agglutinate do not cause serum precipitation or hemolysis or are non-toxic, toxic extracts may be non-agglutinative, hemolyzing extracts may not cause agglutination, etc. The normal agglutination cannot be due merely to a difference in isoelectric points of the active substances, because of the peculiar elective characteristic of various agglutinins toward the same and different bloods. Neuberg (253) held that hemagglutination by plant extracts is associated with lipolysis, basing this contention on his and Rosenberg's observations on the lipolytic activity of commercial ricin and crotin, but Mendel (208) had questioned this view since Osborne's highly purified ricin showed high agglutinative power with little or no lipolysis, while certain highly lipolytic plant extracts were non-agglutinative.

The evidence is strong that the essential elements of most of the plant toxins considered here are protein.

Thus the blood effects of abrin are destroyed by heating at 85° (122), crotin at 69–70° (79), phallin at 65° (85, 86, 88, 94), papain at 100° (285), ricin at 90–100° (226, 227) or less (377, 264), and phasin at 90° (375). Similarly, these substances do not readily pass through dialyzing membranes, e.g. ricin (72), crotin (79), phasin (170), and *Amanita* toxin (148), and they are customarily precipitated by ammonium sulfate, alcohol, and other protein precipitants (crotin, 79; *Amanita* hemolysin, 86, 88; phasin, 170). Crotin has been shown to be a mixture of a globulin and an albumin and is destroyed by alkali and acid (79). Ricin appears to be a coagulable albumin, digestible by trypsin (264). The hemolysin of tomato which is active at dilutions of expressed sap up to 1:30 (50) does not pass through membranes which are permeable to the larger non-protein molecules but impermeable to protein. It is inactive at pH 4.7 but becomes active above pH 4.9 and below pH 4.5, and is hence probably a protein with isoelectric point near pH 4.7.

In spite of these ordinary properties of proteins, however, a number of workers have felt that there was some doubt as to the protein nature of the toxalbumins because they are often highly resistant to the action of proteolytic enzymes, as has been shown for ricin (241, but digested by trypsin according to 264), robin (173), abrin (173), *Amanita* toxin (85), and phasin (375). But, although the bacterial toxins are usually destroyed by such enzymes as pepsin and trypsin, many plant proteins are highly resistant to their action, and such a resistance is not incompatible with the view that the plant toxins considered here are protein in nature (272).

The evidence points to the view that the toxic principle of a plant toxin may be entirely distinct from the principle or principles affecting normal blood. Müller (241) and Jacoby (134) have both shown that the hemagglutinin of ricin is destroyed by pepsin, while the toxin is unaffected by this enzyme. Furthermore, in the case of *Amanita* the agglutinin is thermostable and non-antigenic, the lysin thermolabile and antigenic (94, 284), and anti-hemolytic sera do not protect against the toxin (93). The case of *Amanita* deserves special mention. Ford maintains that there are present in *Amanita* two substances, one, the *Amanita hemolysin* of Ford or *phallin* of Kobert which is a N- and S-containing glucoside, alcohol-precipitable, thermolabile (65°), and antigenic, leading to the production of anti-hemolytic sera, and a second, the *Amanita toxin* which is an indol or pyrrol derivative or an aromatic phenol so combined with an amine group that it gives an indol or pyrrol ring on fusion. It is thermostable and antigenic (94, 85, 1, 88, 89, 93). Kobert believes the *Amanita* toxin of Ford to be a non-antigenic alkaloid, the lysin a protein, not a glucoside (272). The anti-hemolytic sera do not protect against the toxin, although animals may be immunized against the toxin by using whole *Amanita* extracts as immunization antigens. Because the lysin is thermolabile, alcohol precipitable, and antigenic, Kobert (284) maintains that it must be proteinaceous, while he relegates the toxin to the class of toxic alkaloids. This *Amanita* toxin-lysin-agglutinin complex has never been satisfactorily unravelled.

The acquired serological reactions to the phytotoxalbumins show a well-defined specificity. Thus anti-ricin sera do not affect abrin, and reciprocally (72, 73), the two *Amanita* constituents are serologically distinct (85) in the sense that anti-hemolytic sera are not antitoxic, and the bean agglutinin is serologically different from those of ricin and abrin although there is cross-reactivity between the bean and lentil hemagglutinins (287). The complement fixation reaction with ricin sera is positive for ricin but negative for abrin and crotin (28). On the other hand, the plant toxalbumins do not have a high degree of elective capacity as regards the species of animal supplying

the agglutinated or hemolyzed blood, although all bloods are not affected equally, and many are unaffected by toxins agglutinating or hemolyzing other bloods.

The plant toxalbumins appear to illustrate the fact that the *in vivo* and *in vitro* effects of an antigen may be different. In the case of the *Amanita* toxin, to be sure, hemolysis takes place *in vivo* as well as *in vitro*, since injected animals are characterized by the appearance of red urine and hemolyzed serum (148). Crotin, however, shows that the acquired precipitin reaction against an antigen may be very different *in vivo* and *in vitro* (20). Thus if an animal is immunized to crotin its serum will give a highly active precipitin reaction with crotin. If the *precipitate* resulting is washed and injected into the circulatory system of an animal, death follows almost at once due to embolism. If on the other hand the crotin-immune animal is injected with *crotin*, embolism does not occur. If the *in vitro* effect were duplicated *in vivo*, the occurrence of the precipitate would cause death by embolism, and hence one may conclude that the two effects are different. The same difference has also been shown for peptone (20).

While the plant substances considered above are characterized by their toxicity, their blood effects, and their protein nature, there are also certain other, non-toxic protein substances with similar blood effects. Thus hemagglutinins for various species of blood have been found in *Salpiglossis* (76), *Datura* (9, 76, 77), *Phaseolus* (76, 28, 170, 287, 77, 208, 317, 155), *Pisum* (76, 170, 9, 208, 155), potato (195), *Ervum* (76), *Vicia* (76, 208, 155), *Lathyrus* (208, 9), *Soya* (9), *Canavalia* (9), *Glycine* (155), *Vigna* (155), and according to Mendel (208) also in the following: *Caragana*, *Cassia*, *Lens*, *Wistaria*, *Hesperis*, *Robinia*. They have been searched for but not found by Assmann (9), in henbane, by von Eisler and von Portheim (76) in species of *Solanum*, *Nicotiana*, *Capsicum*, *Physalis*, *Hyoscyamus*, and numerous other Solanacea, and by Mendel (208) in *Arachis*, *Baptisia*, *Cladrastis*, *Coronilla*, *Gleditsia*, *Gymnocladus*, *Lupinus*, *Mimosa* (?), *Psoralea*, *Thermopsis*, *Trifolium*, *Vigna*, *Brassica*, *Ipomea*, *Tropaeolum*, *Linum*, *Fagopyrum*, *Zea*, *Avena*, and *Canna*.

Besides the property of agglutinating blood corpuscles, non-toxic extracts of plants sometimes agglutinate bacteria or precipitate bacterial extracts, e.g. *Cotyledo* extracts (160, 161) and *Phaseolus* extracts (76), but not extracts of *Datura* (76) and agglutinating extracts of many other species of plants (208), or they may have the power of hemolyzing blood corpuscles, e.g. *Phaseolus* extracts (77), but not *Datura* extracts (76). Furthermore, as has been pointed out earlier in this paper, the extracts of a great many species of plants precipitate in the presence of normal blood serum when the extracts are at relatively high concentration.

It is evident that the non-toxic hemagglutinins of different plant species represent different substances, as is indicated by their elective capacity toward various bloods. Thus the extract of one plant species may agglutinate rabbit blood corpuscles but not human corpuscles, while the agglutinin of a second plant species may agglutinate human corpuscles while not affecting rabbit blood (208, 76). Furthermore the non-toxic plant agglutinins appear to be quite different from the agglutinins of the toxalbumins. Thus (76) anti-ricin serum inhibits ricin agglutination but does not affect *Datura* agglutination. It was not possible to prepare an anti-agglutinative serum for the *Datura* agglutinin (76), although this did succeed in the case of the bean (287, 155). Moreover some evidence has been adduced to show that the hemagglutinin and the hemoprecipitin of the bean are distinct entities since they have distinct thermal inactivation points (92° and 83°C. respectively, 317).

The plant agglutinins give evidence of being of the nature of resistant plant proteids. Thus they are destroyed in the

vicinity of 80°–90°C. of heat (77, 195, 155), are imperfectly dialyzable (161), are precipitable by ammonium sulfate and alcohol, are insoluble in fat solvents, and in the case of the bean at least are to be found only in the proteose fraction (317). On the other hand, as is often true of plant proteins, they are highly resistant to digestion by pepsin and trypsin (161, 317, 76). They are moderately resistant to ageing *in vitro* (161, 195). Activity titers range from the order of 1:20 to as high as 1:100,000 extract dilution (*Canavalia* extract, 9). The bean agglutinin appears to be associated with reserve food substances in the plant, since it is absent from the green seed, present in the ripe seed, and again disappears on about the 8th day after germination commences. It is absent from other tissues of the plant at all times (77, 217, 155). Normal serum has an inhibitory action on the agglutinin of the potato (195). It appears that the substance in the blood corpuscles responsible for the binding of the agglutinin is the same in different animal bloods with respect to the same plant agglutinin (195), and that the union of agglutinin and corpuscles may be dispersed by warming. This binding property resides in the stroma of the corpuscle, since plasmolyzed, washed stromata are still agglutinated, and it appears to be exceedingly resistant to chemical and physical treatments (288).

V. SEROLOGY OF PLANT NON-PROTEINS

To speak of the serology of non-proteinaceous substances appears to be paradoxical since antigens are almost invariably proteinaceous (362, 363). Certain significant exceptions obtain to this rule, however, and furthermore a certain amount of evidence that the antigen *par excellence* is proteid rests on negative results with plant non-proteins. A word regarding serological tests of the plant non-proteins is hence not amiss.

Carbohydrates. From the work of Heidelberger and Avery it is known that a highly purified carbohydrate prepared from types II and III pneumococcus will precipitate at very high titers in the presence of sera prepared from the whole pneumococci, although in many cases such carbohydrates will not stimulate the production of antibodies.

Heidelberger, Avery, and Goebel (120) have observed that anti-pneumococcus sera also precipitate in the presence of various plant gums, and that purified gum arabic in particular shows this reaction at titers of 1:8,000,000 (anti-type II serum) and 1:2,000,000 (anti-type III serum). The absorption of the anti-pneumococcus sera with pneumococcus carbohydrate removes their activity against the gum arabic. Furthermore, Mueller and Tomcsik (240) have prepared from yeast a purified carbohydrate of yeast gum which precipitates in the presence of whole-yeast serum, but will not by itself stimulate the production of antibodies. Here the reaction occurs at a titer of 1:400,000. Similarly Tomcsik and Kurotchkin (348) were able to demonstrate anaphylaxis using animals sensitized passively with serum immune to whole yeast and shocking with the purified yeast-gum carbohydrate. The phenomenon in question has not been satisfactorily explained. Acting on the theory that certain lipoids (see below), while non-antigenic, will elicit antibodies if the lipoids are first linked to protein molecules, Mueller and Tomcsik attempted to link their yeast carbohydrate to horse serum protein and then immunize, but the attempts did not succeed. The carbohydrate technique was applied to species of *Monilia*, *Willia*, *Saccharomyces*, and *Trichophyton* by Kesten, Cook, Mott, and Jobling (145) who found that the agglutination and precipitin reactions thus obtained with these fungi showed only a low grade of cross specificity. When, however, an absorption technique was applied to this material, a high degree of specificity was observed among the forms studied. Similar results were obtained by Kurotchkin, Lim, and Chu (166, 167, 178) utilizing H_2O-soluble carbohydrate fractions in attempts to differentiate pathogenic species of *Trichophyton*, *Monilia*, and *Saccharomyces*. Injecting macerations of the whole fungi in immunisation, Kurotchkin and his associates obtained high-titer reactions with the carbohydrate fractions as test antigens (precipitation at 1:400,000, complement fixation at 1:25,000,000), but little

specific differentiation was observed among the various species of pathogens studied. At times heterologous reactions were even stronger than homologous ones. Wilcox and Link (376) found a common carbohydrate antigen in each of the 8 strains of *Neurospora sitophila* isolated from a single ascus, and in two inter-fertile, intra-sterile genotypes of *N. tetrasperma*.

It is difficult to interpret the specificity of these carbohydrate reactions. The low-grade specificity with the zoö-pathogenic yeast-like fungi may not be a function of the method but of the material, since numerous tests with protein fractions from such fungi also give a low order of specificity. The far-removed heterogenetic reactions between pneumococcus and yeast, coupled with the type differentiations in pneumococcus, indicate that the distribution of the water-soluble carbohydrate antigens has little bearing on the systematic relationships of the forms tested, representing an evolutionary specificity of an entirely different character from that of the proteins. The utility of the method has been shown in the differentiation of the strains of pneumococcus, and this may extend to strains or sexual types among the fungi. Wilcox and Link are continuing their studies in this direction. But from the standpoint of the use of serological reactions in plant systematics on a broader scale, the carbohydrate reactions, in the state of present knowledge, present such difficulties in interpretation as to limit their use to the narrowest differences in genetic constitution, a field where protein serology is as yet of little value.

Certain plant *glucosides* have also been studied serologically, and one in particular, a constituent of *Amanita*, has been discussed above. Similarly, the "tutu" plant (*Coriaria thymifolia* and *C. ruscifolia* of New Zealand) yields a crystalline glucoside which is highly toxic to livestock, thermostable, resistant to ageing and acids, and which shows no latent period in toxic action. Animals recovered from non-fatal toxic effects show no resistance to subsequent inoculation of lethal doses (92). It is not improbable that crepitin is also a glucoside (272, 293).

The toxicodendrin of the poison ivy (*Rhus toxicodendron*, *R. diversiloba*, and *R. venenata*) may be another glucoside of the same type (89, 90), although this is contested by Pick (272). Here a low order of immunity in man may at times exist naturally or may be obtained by enteral administration of the toxin, and rabbits, guinea pigs, goats, and horses if immunized with sublethal doses yield sera which neutralize 5–6 lethal doses of the toxin for guinea pigs (87, 89, 90). Straus (339a) has shown that the natural immunity in man depends upon an early exposure to the poison, as unexposed infants are regularly susceptible. Strickler's work in 1918 (340) indicated that *Rhus* poisoning may be effectively cured by the intramuscular injection of alcohol extracts of *Rhus*, and since then numerous cases of such effective treatment have been reported (e.g. Alderson, 2, Sayer, 310, Strickler, 341, 342, Bivings, 32, Williams and MacGregor, 381). Recently the less irritating almond oil has been substituted for alcohol as a vehicle for the toxin. Theimmunity resulting from such treatment is apparently a tissue immunity, transient in character (340). Serological tests as indices of immunity have proven unsuccessful (340, 342).

According to Porges (275), active unheated normal serum stimulates the phagocytosis of *starch* by homologous phagocytes, but this property of normal serum is not increased by immunization. Nozu (in 103a) claimed to have demonstrated an acquired complement deviation

reaction in rabbits inoculated with starch, but Giovanardi (103a) was unable to confirm this.

The toxic plant *alkaloids*, as strychnin, atropin, and pilocarpin, are hemolytic to normal blood, although this property was lacking in the non-toxic alkaloids tested (272). On the other hand, however, such alkaloids have not been found to induce in animals the production of antibodies or to react serologically with sera prepared from whole extracts of the plants yielding the alkaloids. Thus Lusini (184) obtained a precipitating serum for whole opium extract, but this serum did not react with the opium alkaloids morphin, narcein, codein, and papaverin. Similarly, a precipitating serum for whole belladonna extract failed to react with the belladonna alkaloids, and with morphin chlorhydrate and neutral atropin sulfate no precipitating sera could be prepared (186). Nicoletti (255) attempted to bind morphin to serum protein and produce sera specific for the alkaloid, but his attempts were unsuccessful, as were those in which he tried to produce morphin-precipitating sera by inoculation of morphin chlorhydrate.

Chlorophyll and *xanthophyll* extracts are hemolytic, the reaction of chlorophyll but not that of xanthophyll requiring sunlight, while normal serum of various species inhibits this action (246). It has been claimed by Hôdyô (128) that rabbits may be immunized with alcoholic chlorophyll extracts intracutaneously, and that the sera of such animals precipitate chlorophyll extracts with titers of 1:2000, while normal serum fails to give any reaction with chlorophyll. Although the related hemoglobin is known to be antigenic, these isolated findings of Hôdyô require further confirmation. If chlorophyll and xanthophyll are precipitinogenic in general, it is very surprising that one does not obtain common reactive factors in all green plants, a situation which assuredly does not obtain. Sherwood (326) has described a peculiar effect of normal serum in the presence of plantain extracts in liberating chlorophyll from chloroplasts, but the phenomenon is not at all understood. Elmore (80, 308b) has observed the liberation of chlorophyll from plant cells by the action of normal serum, a phenomenon which is not increased by immunization and which appears to be lipoid in nature.

Hematoxylin, a dye prepared from the wood of *Hematoxylon*, was claimed by de Angelis (5) to produce sera which precipitated and decolorized hematoxylin solutions, but Takemura (345) was unable to confirm this.

It is known from the work of Sachs and others (303) that while *lipoids* are unable by themselves to produce antibodies in animals, if the lipoid be first bound to a non-specific protein and then injected, the serum resulting is serologically active against the pure lipoid, a situation perhaps comparable to the action of the pneumococcus carbohydrate as detailed above. In such cases the lipoids are referred to as haptenes or half-antigens. Little has been done with the plant lipoids in this connection, however. Londini (182) was unable to immunize animals with either pure olive oil or pure cottonseed oil. Uhlenhuth and Händel (351), however, by immunizing guinea pigs with flax-seed oil later were able to obtain anaphylactic reactions by administering a second dose, not of the oil alone, but of an extract of the whole flax-seed. They also obtained similar results with rapeseed oil and almond oil. It is unlikely that the oil itself was the sensitizing substance. Almost incredibly small amounts of protein will sensitize a guinea pig, and it would be extremely difficult to

prepare a plant oil entirely free from such slight contaminations. The second or shock dose must be relatively larger, however, which might well explain why anaphylaxis was obtained by Uhlenhuth and Händel only when the second dose was of whole seed extract, not of the "pure" oil. Uhlenhuth and Jung (352) have described a reaction resembling agglutination of suspensions of olive oil and gum arabic in the presence of normal horse serum. Normal rabbit serum did not show such an action, but the inoculation of rabbits with the oil resulted in the production of sera which caused a similar flocculation. The "agglutinin" obtained was destroyed at 67° and was not specific for the olive oil, as other oils could be substituted in the reaction. It is possible that this phenomenon is interpretable in terms of the haptene theory.

Organic acids. Tannins have often been recognized as productive of non-specific artefact reactions with blood sera. The tannins in trade are frequently dispensed in the form of tannin-albuminates, but Schenk (311) has shown that, while such substances serve for immunization, a resultant reaction occurs only with the albumin, indicating that the tannin radical plays no part serologically in the reactions observed.

(To be concluded)

MORPHOGENESIS OF THE SHOULDER ARCHITECTURE
PART V. MONOTREMATA

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IN THE present series of papers concerning the shoulder architecture in vertebrates the Monotremata naturally follow the Reptilia. The former cannot be regarded as in any way representing the "prototherian" conditions in this feature of their anatomy, however, as one of their group names would imply. Rather must their shoulder be considered as a development of the reptilian type, highly specialized, and culminating, as well as terminating, in this group.

For more than 50 years the work of Westling on the anatomy of *Echidna*, and for 40, that of McKay on *Ornithorhynchus*, have been standard, and have been quoted far and wide. Both are excellent papers, indicating unusual care in dissecting, and the latter is further useful in that the author quotes extensively from previous reports on the subject. Westling's figures are excellent, but insufficiently diversified, while those of McKay are rather poor. If one desires purely topographical detail these papers are entirely adequate, but they are too old to reflect modern concepts of morphology, and present-day reports upon the anatomy of this group are highly desirable.

The shoulder region of a specimen of *Ornithorhynchus* was dissected, through the generosity of the U. S. National Museum, and the undertaking involved not a little difficulty. The muscles were particularly robust and closely crowded, the connective tissue about the nerves aggravatingly tough, and the smaller arteries so difficult to distinguish from nerves, that I suspect that the last feature accounts for some of the questionable innervations reported in the literature. Hence, in the present study the innervations often were verified microscopically.

SKELETON

One is struck by the fact that in monotremes the shoulder girdle is definitely reptilian rather than mammalian in character. Not only is independent movement of the two sides so circumscribed as to be practically nonexistent, but the whole girdle is incapable of more movement than the sternum (and consequently the ribs) permits.

The membranous girdle is represented by clavicle and interclavicle only. The latter is broad posteriorly, where it articulates with the episternal element, moderately constricted in the middle, and expanded into a long, slender process on either side anteriorly. The latter extend to, and articulate with, the acromial processes of the scapula. Fused in the adult with the lateral interclavicular processes are clavicles, the whole being termed the clavo-interclavicular bars. To this bar are attached trapezius and claviculate deltoid elements as expected. In addition the superficial coracohumeral

has this origin, having relinquished original attachment to the anterior coracoid, as in placentals. The remainder of the interclavicle provides origin for a part of the pectoralis.

The dorsal division of the cartilaginous girdle may be viewed as consisting of suprascapula and scapula. The former, as a discernible element, has been reduced almost to the point of absence, except at one spot near the middle of the vertebral border, where there is a distinct cartilagi-

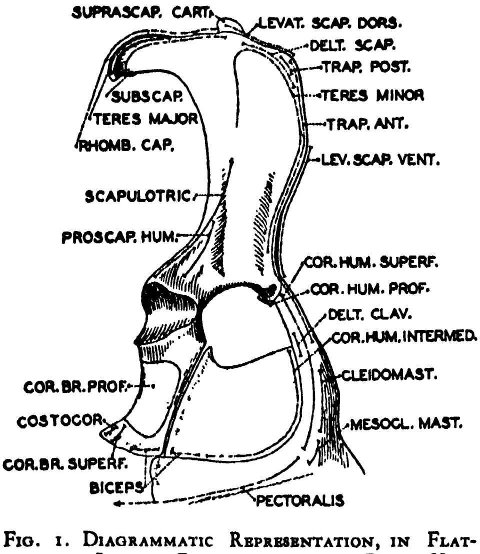

Fig. 1. Diagrammatic Representation, in Flattened, Lateral Perspective, of the Right Half of the Shoulder Girdle of the Platypus, with Muscle Attachments Shown by Broken Lines

nous process giving rise to a part of the levator scapulae dorsalis. The remainder of the suprazonal muscle matrix, consisting of serrati scapulae, rhomboideus, and levator scapulae (pars dorsalis in part, and pars ventralis in whole), has thus settled upon the scapula. As in reptiles mm. trapezius and teres minor (dorsalis scapulae), originally concerned with the cleithrum, also are attached to the scapula, while the scapular deltoid has migrated from the clavicle to this situation.

As compared with the scapula of *Iguana*, that of the platypus is noteworthy in having the vertebral part prolonged caudally (instead of cranially) in the styliform process, the cranial border everted laterally, a process upon the medial surface, greater definition of the acromion and its separation from the coracoid. The reason for the caudal process is muscular, and it is difficult to see, in view of the immobility of the girdle, how the serrati could have been instrumental in this. It therefore must be due to mm. teres major and subscapularis, both of which elevate the caudal border of the humerus and thus rotate this segment.

For 85 years it has been believed (Owen, Mivart, Wilson and McKay, and others) that the anterior border of the scapula in monotremes, as in reptiles, represents the scapular spine of therian mammals, and that the part of the monotreme girdle representing the anterior border of the scapula in therians is the process upon the medial surface of the bone upon which is attached the omohyoid. The part between this process and the anterior border in platypus thus would represent the supraspinous fossa of therians, an area invaded by m. supraspinatus. The eversion of this representative of spina scapulae is quite marked. There is no violence done to sound morphology by this reasoning, for an exactly similar procedure has been followed within the class Mammalia by the development of the human crista iliaca.

Carrying this line of reasoning farther, Wilson and McKay advanced the hypothesis that the posterior border of the therian scapula is represented in monotremes by a ridge upon the lateral scapula between mm. teres minor (McKay's infraspinatus) and subscapularis. This is possible, but a needless complication for which there is no good evidence.

The question of the coracoids of monotremes needs no lengthy discussion here, for the situation is exactly the same as in reptiles. In primitive reptiles, as in lizards, an anterior and a posterior coracoid area give rise to distinctive muscle matrices, and the same muscles arise respectively from separate anterior and posterior coracoids in monotremes. For the former bone the term epicoracoid is permissible, but I prefer for the present to designate it by the English appellation.

In monotremes the anterior coracoid has made considerable progress in retreating from the scapula, an extensive hiatus separating the two, and articulation is solely with the posterior coracoid. It has a cartilaginous medial border that overlaps its antimere. Reduction in the extent of this bone evidently was permitted by the fact that a part of the coracohumeral matrix already has left it in favor of attachment to the clavo-interclavicular bar, and a lesser part to the acromion. Of its original muscular equipment it retains only the intermediate part of the coracohumeralis, but to it has migrated the origin of anterior biceps and the extension, upon its dorsal surface, of the coracobrachialis profundus, both of which originally were concerned with the posterior coracoid. The latter muscle particularly may well have terminated a reduction of the bone that otherwise would be more pronounced. The character of this muscle, however, is doubtless a monotreme specialization, for it would be useless, in such form, in a mammal in which the arm is held in the typical therian position.

The posterior coracoid is the heavier but less extensive of the two ventral bones, and is not separated in adults from the scapula by suture. It provides attachment for mm. costocoracoideus, coracobrachiales, and biceps posterior. Noteworthy are the facts that it is relatively much longer in the platypus than in *Iguana*, giving a greater leverage to the muscles concerned, and that it articulates so definitely with the episternum.

Examination of the glenoid cavity of *Iguana* indicates that the humerus is, in mean position, held at an angle of about 30 degrees with the body axis, and essentially parallel with the ground. The comparable figure for *Ornithorhynchus* is about 70 degrees, with the humeral axis likewise horizontal. Indeed, the coracoid border of the glenoid projects to such a degree that the humerus cannot be lowered. In regard to brachial posture, then, the platypus is less "mammalian" than *Iguana* even, and less capable of bringing the arm beneath the body.

In certain respects the humerus of *Ornithorhynchus* is very remarkable. As it is held horizontally—a position which is very wasteful of effort when the animal is on land—the angles of leverage for the muscles must be great. This phenomenon is exhibited in the breadth of bone, arranged essentially in a tetrahedral pattern, reminiscent of *Eryops* and some of the earlier reptiles, but very different from living reptiles. Particularly noteworthy is the shape of the humeral head and its significance. The shape of the articular surface is not oval, as in *Iguana*, nor spherical as in so many mammals, but resembles an interrogation mark (Fig. 2A). The way in which this operates is at once clear if one manipulates the head within the glenoid cavity. It is then seen, with the humerus horizontal, that when the anterior part of the articular surface is within the glenoid, the manus is advanced and slightly abducted. As one rolls the articular surface through the glenoid the humerus is rotated, the distal end slightly retarded, and the manus

brought downward, backward, and finally against the body with the palm upward.

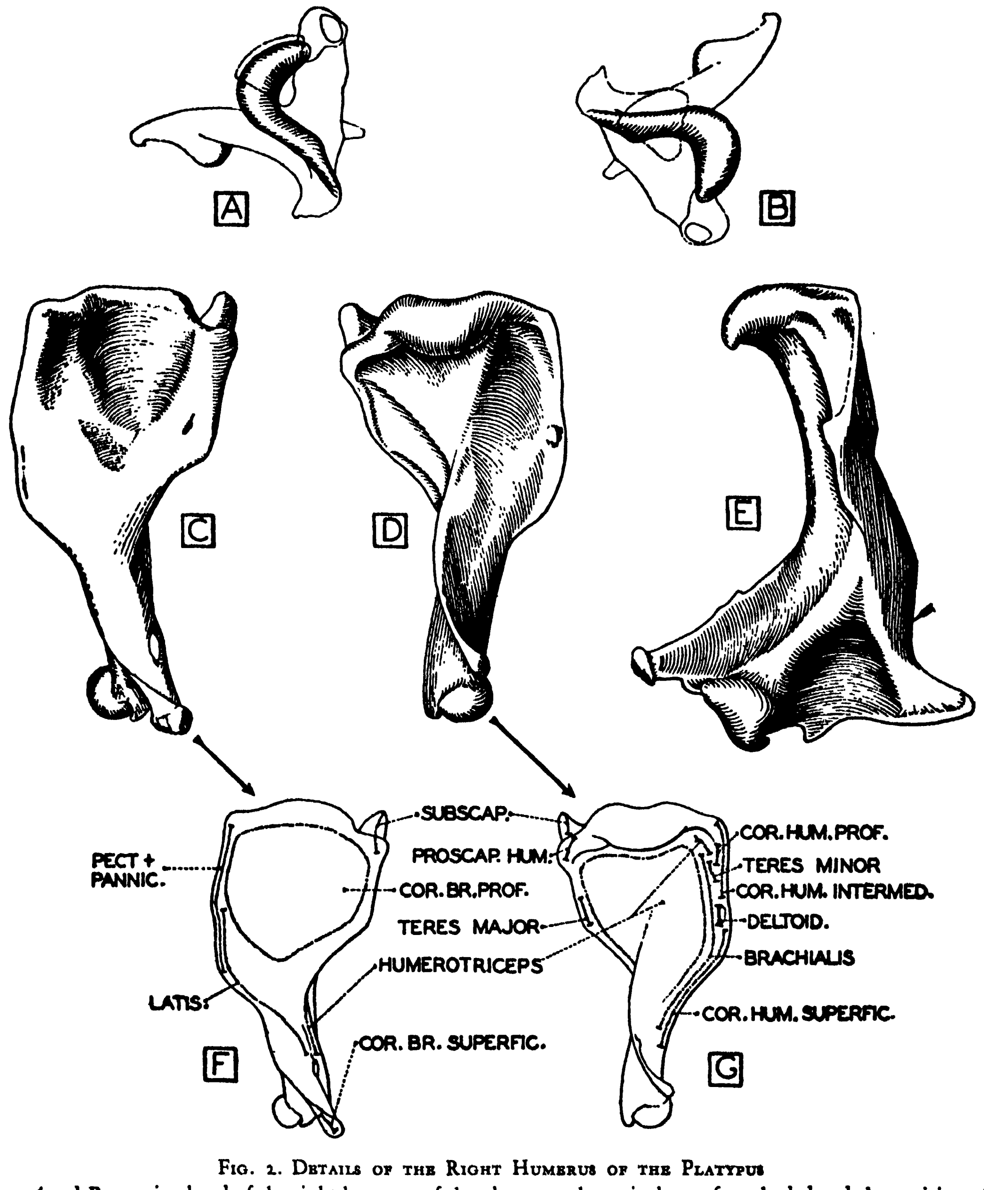

FIG. 2. DETAILS OF THE RIGHT HUMERUS OF THE PLATYPUS

A and *B*, proximal end of the right humerus of the platypus, the articular surface shaded and the position of the glenoid shown by a dotted line; to show degree of rotation possible: *A*, with anterior border of the humerus elevated, and *B*, with same depressed. *C*, the right humerus from ventral; *D*, from dorsal; and *E*, from cranial aspects. In the last the arrow pierces the entepicondylar foramen. *F*, ventral aspect of the humerus showing muscle attachments. *G*, the same from the dorsal aspect.

This is the stroke movement which the animal uses in swimming, and by which it may walk. Elevation and depression of the distal humerus is possible to a limited extent but only within a very restricted arc.

The proximal humerus is very broad (51 per cent of the length) and thin. The cranioventral margin may be termed the cranial border, and the caudodorsal margin the caudal border. Both represent processes lengthened to form crests. The dorsal surface of the proximal expansion is occupied by the humerotriceps origin, and the ventral surface by the insertion of the coracobrachialis profundus. The latissimus has shifted distally, as also has the coracobrachialis superficialis insertion.

Because the main action of the humerus is that of rotation the muscle attachments (except of humerotriceps and coracobrachialis superficialis) exhibit a tendency to congregate upon the bordering crests.

The plane of the distal part of the humerus is at a right angle to that of the proximal part, and is even more expanded, its width being 78 per cent of the humeral length. Most of this width is attributable to the medial epicondyle, which gives great strength to the forearm flexors, while the lateral epicondyle is shorter. The trochlea is offset considerably lateral to the axis of the bone and is spherical, allowing much rotation (supination and pronation) of the forearm as a whole. An entepicondylar foramen is present.

Recent morphologists mainly agree in the opinion that monotremes constitute one mammalian stock, while marsupials and placentals represent another; but seldom or never does one encounter in the literature an opinion regarding the sort of reptile from which monotremes may have arisen. Both the myology and osteology of this group exhibit a high degree of specialization, in many respects away from the direction taken by therian mammals. It is always a question in such cases which characters may be the result of adaptation and which may be basic; but there is one point, covered by the present paper, of particular interest in this regard. The theory of irreversibility of evolution is accepted by me only with extensive reservations, but it is difficult to see how the anterior coracoid of a tetrapod could become severely reduced, following the shifting of its musculature to other bones, as was clearly the case with the theriodonts of the Triassic, and could later take the reverse step to conform to monotreme conditions. The obvious conclusion is that monotremes could not have been derived from any of the "mammal-like reptiles" of therapsid affinity of the types now known.

NERVES

The brachial plexus of *Ornithorhynchus* is built upon the usual principle comprising three trunks, derived mainly from five roots. The first trunk is formed by C5 and C6, the second by C7, and the third by C8 and T1; hence the axis is through C7. In addition the plexus may receive a fine filament from C4, but at least in some cases it is not unlikely that this carries solely phrenic fibers. Westling and McKay found that T2 also may contribute to the plexus, but I was unable to demonstrate this in my specimen.

The nerves of the plexus are even more readily separable into dorsal and ventral groups than in most mammals. The definition of the usual mammalian condition, involving three cords, is obscured; or if one prefer it can be stated that there is a cranial dorsal, a middle ventral, and a caudal mixed cord.

The brachial plexuses of all tetrapods have many points that are fundamentally very similar, but no one who has completely dissected the appendage can consider that the plexus of the platypus is fundamentally mammalian (therian). It is different in a very significant manner, and its most unique feature, involving

the double radial nerve, is quite comparable to the situation encountered in both Amphibia and lacertilian reptiles. Neither do the trunks of C8 and T1 supply only ventral components as stated by Miller. Other interesting features comprise the facts that n. thoracodorsalis has relinquished close association with the axillary group and arises from the second rather than the first cord, and the arrangement of the cord chiefly supplying n. medianus.

The illustration of the plexus is shown from the dorsal aspect, contrary to the

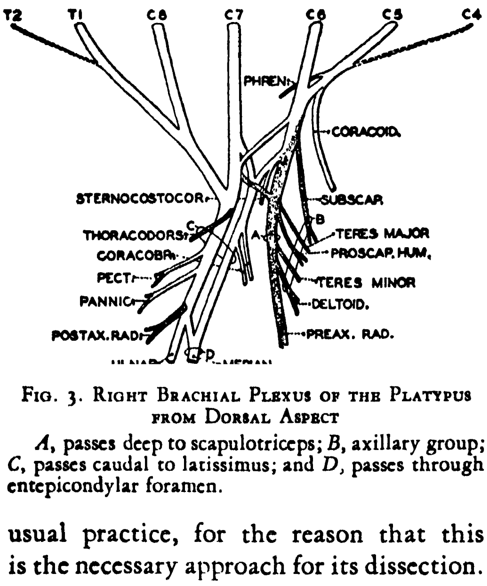

FIG. 3. RIGHT BRACHIAL PLEXUS OF THE PLATYPUS FROM DORSAL ASPECT

A, passes deep to scapulotriceps; *B*, axillary group; *C*, passes caudal to latissimus; and *D*, passes through entepicondylar foramen.

usual practice, for the reason that this is the necessary approach for its dissection.

N. *vago-accessorius* supplies mm. trapezius, a part of the cutaneous muscle field, and the two divisions of "sternocleidomastoideus." At least C2, 3, and possibly also 4, enter this field.

Nn. infrahyoidei, innervating the infrahyoid musculature and representing nn. hypobranchiales of amphibians and reptiles, are derived from C1 and 2.

Dorsal (extensor) division

Suprazonal group

The nerves representing levator scapulae branches and nn. thoracales posteriores of placentals (not illustrated), arise from C3 to 7, and innervate mm. serrati anteriores, rhomboideus, and levatores scapulae.

Shoulder group

N. *thoracodorsalis*, supplying m. latissimus dorsi, is derived at least from C7 and possibly more posterior roots. In the platypus it has relinquished close association with the axillary group.

Nn. axillares communes consist of the remaining dorsal divisions of the shoulder proper, and in the platypus they diverge from the trunk of the preaxial radial, the highest component being one or more branches to m. subscapularis, and next, n. teres major. The former, and at times the latter, are so situated as to receive no fibers from nerve roots caudal to C6, while C7 also can contribute to the remainder of the axillary group. The main trunk then passes caudal to m. teres major and thence cranially deep to the origin of m. scapulotriceps. In consecutive order branches are supplied to mm. proscapulohumeralis, teres minor, and both divisions of deltoideus.

Brachio-antibrachial group

Nn. radiales. The dorsal fibers to the extremity proper follow two pathways in an interesting way. The first (a) comprises a continuation of the cranial cord from which arise the axillary branches, and hence may be considered as preaxial in that it is derived from C5, 6, and possibly partially from 7. After passing in a cranial direction deep to the original end of the scapulotriceps it follows a superficial course, lateral to m. humerotriceps, to the forearm, where it divides into rami profundi (muscular) et cutanei. The second part (b), which may be designated as postaxial, separates near the elbow from the third or caudal cord of the

plexus (whose course is described under the ventral nerves), enters between scapulo- and humerotriceps and is distributed to these muscles. This arrangement accordingly is comparable to conditions in some Amphibia and in lacertilians, but not in therian mammals.

Ventral (flexor) division

Infrazonal group

These arise either from C5 or 6, or both, and take the form of fine filaments from the middle cord of the plexus, innervating mm. sterno- and costocoracoidei.

Shoulder group

Nn. pectoralis et panniculus carnosus are two stout branches of the caudal cord of the plexus and are distributed to the muscles of the same name. Upon one side of the animal dissected m. pectoralis appeared to receive a branch also from the middle cord, but it was cut in skinning and I could not be certain. This was not the case upon the other side.

N. coracoideus (anterior coracoid nerve) is derived from C5 alone. It passes to the lateral aspect of the girdle through the fenestration bounded by scapula, coracoid, and clavicle, and is distributed to the three divisions of the coracohumeral musculature, and, by a fine filament, to a part of m. biceps anterior.

Nn. coracobrachiales in the specimen dissected occurred as more than one branch of the middle cord of the plexus. Mm. coracobrachiales, biceps posterior, and a portion of biceps anterior are so supplied.

Nn. flexores antibrachii (flexor brachii, brachialis longus inferior) comprise median and ulnar components. The former constitutes a direct continuation of the middle, and the latter of the caudal cord of the plexus. Both together pass caudal to the latissimus, between the latter and the coracobrachialis superficialis, and in the distal brachium lie superficial. After the posterior trunk gives off the postaxial branch of the radial it makes an anastomosing contribution to the median, and then continues around the elbow as n. ulnaris. After receiving the anastomosis above mentioned the continuation of the middle cord pierces the entepicondylar foramen and becomes n. medianus. It then innervates m. brachialis.

MUSCLES

McKay has given not only a full description of the myological details, but extensive digests of the opinions of previous workers in this field. Muscle descriptions in the present paper, therefore, are made as concise as feasible, while reference will be made to previous reports only when these appear to be called for.

Branchiomeric division

M. trapezius. This occurs in two divergent divisions: one (a) arises from the dorsal fascia as far caudally as the latissimus origin, and narrows to insert upon the anterior angle of the scapula. The other (b) has fascial origin over almost the entire braincase and from the anterior part of the nuchal line. It converges to insert upon the anterior scapular border at the anterior angle and ventrally, upon the acromion and almost the entire clavicle. The anterior border of this muscle is quite thick. Berry Campbell (personal communication) found that the cutaneous muscle field has had increment from the accessory field, but my specimen was in such condition that the cutaneous muscles were not fully dissected.

M. mesocleidomastoideus (episternocleidomastoideus part McKay) arises under cover of the anterior trapezius from the squamosal. More ventrally it emerges from beneath the border of the trapezius

as a narrow band and inserts upon the interclavicle near the midline.

M. cleidomastoideus (episternocleidomastoideus part McKay) arises from the squamosal also, immediately posterior to the last division, than which it is narrower. It inserts upon the middle portion of the clavicle.

Innervation: N. accessorius, C2, 3, and probably 4. With particular care I demonstrated that C2 and 3 ramify into the anterior trapezius and extend to the mastoid divisions. C4 also enters the trapezius but I was less certain of its ramifications.

Infrahyoid division

There are two muscles in this group that may receive the names employed. They may or may not be exactly homologous with the corresponding two divisions of placentals.

M. omohyoideus arises from the process upon the medial side of the scapula, as described under the skeleton, and inserts upon the hyoid and mylohyoid raphe.

M. sternohyoideus arises from a midline raphe dorsal to the coracoids as far caudally as the sternum, and inserts upon the hyoid.

Innervation; Branches of C1 and 2, the former of which receives also a contribution from XII.

Dorsal (extensor) division

Suprazonal (or serratus) matrix

M. rhomboideus arises broadly from the cranium and slightly from the ligamentum nuchae, and inserts upon the caudal half of the vertebral border of the scapula.

M. levator scapulae dorsalis arises robustly from the cervical transverse processes (2–7, McKay), and inserts upon the anterior half of the vertebral border of the scapula, including the small suprascapular cartilage.

M. levator scapulae ventralis (acromiotrachelien McKay) arises from the cervical vertebrae and inserts upon the ventral two-thirds of the anterior surface of the reflected part of the craniomedial border of the scapula, including the acromion, encroaching slightly upon the clavicle.

Mm. serrati scapulae. The origin of these slips was not checked accurately, as this could not be done without interfering with more important features. There is a separate posterior slip (superficialis) (Fig. 4B) to the caudal half of the vertebral border, a largely horizontal division, unmentioned by McKay, the most lateral of all, from a costal origin to insertion upon the ridge upon the medial scapula, and the most medial division, comprising a sheet of slips from the ribs to the entire vertebral border of the scapula.

Innervation: C6 to 7, the last supplying the most caudal (superficialis) slip.

SHOULDER GROUP

Thoracodorsal (latissimus) matrix

M. latissimus dorsi. I found this to be somewhat different than described by McKay. Like his specimen mine showed a dorsal portion, arising from the vertebral spines, and a costal portion from the ribs. The former, however, is separable into three layers (not shown in Fig. 4) which partly overlap. The deepest is continuous with the costal portion. Many of the fibers of the most superficial layer disappear in fascia, and the others join a common insertion, upon the ridge extending from the entepicondylar foramen toward the extensor process of the proximal humerus. The insertion of the ventral part of the muscle is more tendinous. The whole constitutes a very powerful mechanism for retarding and then elevating the arm. This doubtless is an aquatic specialization. It should be noted that

the latissimus insertion, unlike conditions in reptiles, is not straddled by any part of the triceps but is located medial to the latter complex. On the other hand it is situated between the two divisions of the extensor nerve (n. radialis).

Innervation: N. thoracodorsalis, from C7, 8, and probably T1.

M. deltoideus clavicularis. This is the deeper division of McKay's deltoideus acromioclavicularis. It arises from the clavointerclavicular bar deep to the coracohumeralis intermedius, in intimate association with the origin of the latter, and inserts just deep to the insertion of the scapular deltoid.

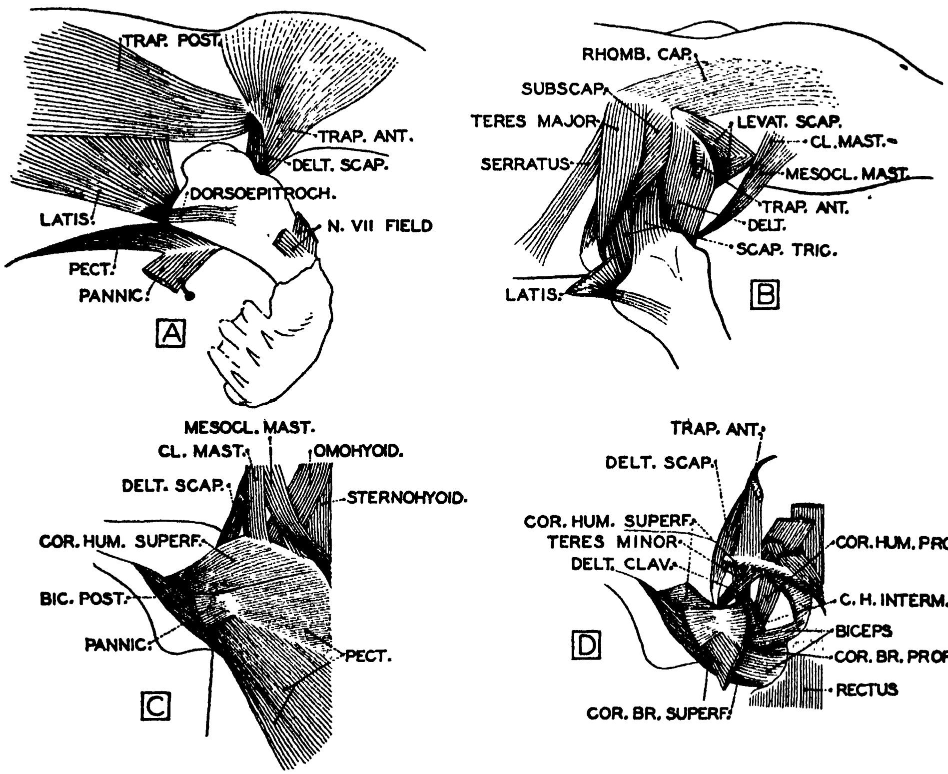

FIG. 4. RIGHT SHOULDER MUSCLES OF THE PLATYPUS
A, lateral view of superficial muscles; *B*, lateral view of deeper details; *C*, ventral view of superficial muscles; and *D*, ventral view of deeper details.

Axillary matrix

M. deltoideus scapularis (all authors) arises from the anterior angle of the scapula adjacent to the insertion of the trapezius posterior. It passes between the brachial musculature and m. coracohumeralis superficialis to insert by a constricted tendon upon a well defined tubercle upon the cranial border of the humerus.

M. proscapulohumeralis (teres minor McKay; subscapularis part, Coues, Westling). This term, although to some extent a misnomer, is applied for the reason that the muscle appears to be the homologue of the shorter division of the same name in caudate amphibians. It is a short, thick, deep muscle arising from the ventral third of the glenoid border of the scapula, between the teres minor and scapular

triceps, anteriorly overlapping in part the origin of the latter muscle. Insertion is upon the tip of the caudal border of the humerus.

M. teres minor (infraspinatus McKay, Owen, Coues, Cuvier and Laurillard) arises deep to the deltoid from the anterior part of the lateral scapula and ventrally almost to the acromion. It inserts upon the cranial border of the humerus deep to the coracohumeralis intermedius.

M. teres major (most authors) arises from the dorsal part of the caudal process of the scapula. It passes caudomedial to the triceps complex and inserts by tendon upon a well marked process of the caudal border of the humerus.

M. subscapularis (teres major part, Coues, Meckel) arises from the caudal half of the lateral scapula, deep to the teres major, and from the entire medial surface of the scapula. The insertional end contains a large sesamoid bone, and attachment is upon the extensor tuberosity of the humerus.

Innervation: M. subscapularis situated caudal to the scapulotriceps, is innervated by branches whose fibers can come from no roots posterior to C6, while the branches to the remaining five muscles may be derived also from C7.

There can be no question regarding the identity of the deltoideus scapularis. It carries to an extreme the tendency for dorsal migration exhibited throughout its phylogeny, and here represents a spinodeltoid. McKay included the deltoideus clavicularis with the superficial coracohumeral, terming the two together deltoideus acromioclavicularis. This, in view of the distinctiveness of the innervation, is difficult to understand. That it is probably phylogenetically distinct from the coracohumeral is first suggested by the separation of the two insertions by the scapular deltoid, and that it belongs with the latter is shown by the common nerve supply. McKay may have been led astray about the innervation by the fact that branches of the n. coracoideus lie in contact with it. The muscle evidently represents the deeper layer of the deltoid (scapulohumeralis brevis of Caudata), over which the coracohumeralis has extended.

The proscapulohumeralis is so termed for the reason that if it has a homologue in other tetrapods it must be the muscle of the same name in Caudata. If this be the case the origin has changed from the anterior to the posterior border of the scapula, to which migration there would be no obstacle. On the other hand, there are insuperable obstacles, because of the insertion, to McKay's interpretation of it as the teres minor. The muscle is absent in placentals.

The teres minor clearly is the representative of the corresponding muscle in placentals, and of the dorsalis scapulae in lower forms, because of nerve supply, insertion, and position. True, it is located on the anterior instead of the posterior part of the scapula, but the development of suprascapular musculature would force it caudally. McKay did not specify the grounds upon which he considered this to be an infraspinatus, and his statement that the nerve supply is by n. coracoideus as well as n. axillaris is probably attributable to the fact that the muscle receives blood from a small artery accompanying n. coracoideus and this vessel easily is mistaken for a nerve.

M. teres major is even more distinctive a muscle than in placentals, and if it has been derived from the latissimus dorsi, as somewhat reluctantly I have come to believe, neither its conformation nor nerve supply here indicate the fact; yet there appears to be no other disposition to make of it. In mammals its insertion

is always in intimate relation with that of the latissimus, and there is no muscle in anamniotes that could correspond to it. Hence it is assumed that a part of the latissimus became attached to the gleno-vertebral angle of the scapula, and that this part then split from the remainder, as it very easily could do. The subscapularis is essentially the same as in reptiles, and calls for no further remarks.

Brachio-antibrachial group

M. *dorso-epitrochlearis* originates as a slender but very distinct muscle from the ventrolateral border of the latissimus near its insertion. It passes over the elbow and ends in the superficial fascia over the flexor carpi ulnaris.

M. *scapulotriceps*. In my specimen this was not separable into three layers, as in McKay's, but into two. Origin was from the lateral surface of the scapula on a line from the ventral part of the glenoid border extending half way to the suprascapular cartilage. The more caudal fibers separated into a superficial layer that inserted into the loose fascia over the olecranon, while the deeper part inserted strongly upon the olecranon.

M. *humerotriceps*. As both branches of the radial nerve are entirely superficial to this division, caput laterale, characteristically present in placentals, should be considered, strictly speaking, as undeveloped in monotremes. In the latter there are three divisions, however; (a) a more lateral part, arising by a thin tendon from the cranial portion of the dorsal surface of the proximal humerus, (b) a main portion, from the entire dorsal expansion, and (c) a deep, short division from the caudal surface of the distal expansion of the humerus. All insert upon the olecranon.

Innervation: The nerve supply of m. dorso-epitrochlearis was not determined by me. Westling, and McKay, indicated that the nerve is given off n. radialis. The triceps is supplied by the postaxial radial.

As m. dorso-epitrochlearis is reputedly innervated by a branch of n. radialis it is placed with the triceps. This branch, however, very readily could have shifted from association with n. thoracodorsalis, so the actual derivation of the muscle cannot be established.

As in reptiles, the scapulotriceps of monotremes appears to have reached the scapula by migration along a fascial plane in order to bridge the axillary nerves. Because of the extent of the origin, robustness, and the extreme dorsal position in which the elbow is habitually held, it is clearly a specialized and very powerful muscle. It should be noted that in monotremes, as in other mammals, there is no corocotriceps. It is thought that the latter is present only when the latissimus tendon is so situated as to require its straddling by the long head of the triceps in order to render the latter efficient. In the platypus the latissimus is medial to the entire triceps complex; and besides, the arm is held in such a position that a coracotriceps could not function.

It may be noted that the humerotriceps is a closely knit entity. In tetrapods different parts of the complex may diverge proximally in various ways. Thus in *Iguana* a lateral and medial head straddle the insertions of proscapulohumeralis and latissimus. In *Ornithorhynchus* both these insertions are situated more caudally, so that the entire humerotriceps is located cranial to them.

Ventral (flexor) division

Infrazonal matrix

M. *sternocoracoideus* (sterno-epicoracoid McKay) is the more dorsal and narrower of these two muscles. It arises from the

dorsal aspect of the sternum and the medial part of rib I. It then becomes styliform, inserting upon the extreme craniomedial part of the posterior coracoid, and the medial cartilaginous border of the anterior coracoid.

M. costocoracoideus is fan-shaped, arising from more laterally on rib I, and converging to a restricted insertion upon the caudomedial part of the posterior coracoid.

Innervation: C(4), 5, and 6.

Owen, and Coues, with reservations, considered these muscles to represent m. pectoralis minor, but the former division usually is regarded as representing m. subclavius of placentals. The latter supposition is plausible, although it cannot be proven that the subclavius has not arisen from the anterior coracoid matrix.

SHOULDER GROUP

Pectoral matrix

M. panniculus carnosus. The older concept held that all dermal muscles posterior to the head constituted panniculus elements, but it appears that a considerable part of this muscle complex has been developed by posterior migration of the facial field, and hence, is innervated by cranial n. VII (see Huber, 1930). Unfortunately, the condition of the specimen at my disposal is such as to preclude the possibility of satisfactorily determining the innervation of each of the layers of such a complicated mass. The significant facts in the present connection are that the facial field has invaded the thoracic area and that slips of it insert not only upon the fascia of the trunk, but upon the antibrachium. Thus a more superficial part inserts into the fascia of the ulnar border of the wrist, and a deeper, more robust part, firmly upon the ulnar border of the distal ulna. These advance and elevate the manus. The panniculus itself, in several layers, is situated mostly ventrallv and fuses at insertion with the pectoralis, the common attachment being to the flexor tuberosity of the humerus, with an inclination that retracts the arm.

M. pectoralis (pectoralis major part, all authors) arises from the midline ventral to the interclavicle and the entire sternal complex. The central fibers insert upon a raphe as illustrated. A small portion upon the posterior border is separable (McKay's pectoralis quartus), this being entirely comparable to the pectoralis abdominis of many placentals, but not of man. It inserts, with the panniculus, upon the entire crest of the cranial border of the humerus.

Innervation: Ventral branches derived from the more posterior part of the plexus and diverging from the caudal cord. Upon one side of the animal dissected the pectoral may have received a branch also from the middle cord. McKay stated that the panniculus is supplied as well by the lateral cutaneous nerve of the thorax, branches from C1–4, and from the posterior divisions of the spinal nerves. That these branches as listed by McKay are not cutaneous nerves piercing the panniculus requires proof.

Apparently tetrapods did not develop a panniculus until the pliable integument of the Mammalia was attained. There is every reason for believing, and none for doubting, that it was derived from the matrix of the primitive pectoralis. That the mammal lowest in the scale has one of the most complicated systems of dermal trunk musculature is of no concern to us here. There is nothing noteworthy about m. pectoralis except that its nerve supply exhibits a tendency to shift cranially—a phenomenon not in accord with the placental condition. Thus in some instances it receives in monotremes no increment from C8 to T1, as I found, although in other individuals of platypus it does so

(McKay). This matrix is certainly the representative of the pectoralis minor layer of higher mammals, this including mm. pectoralis minor, pectoralis abdominis, and panniculus carnosus where they occur. In this respect monotremes thus are intermediate between reptiles, in which the true pectoralis has over-ridden the coracohumeral, and placentals, in which the superficial coracohumeral (pectoralis major) has over-ridden the true pectoral layer.

Anterior coracoid matrix

M. *coracohumeralis superficialis* (deltoideus acromioclavicularis McKay, Westling; pectoralis major Meckel, Coues, Cuvier and Laurillard) is the superficial muscle adjoining the pectoral that arises from the entire clavo-interclavicular bar. It inserts upon a line on the anterior humerus extending from the deltoid process almost to the trochlea.

M. *coracohumeralis intermedius* (epicoracohumeralis McKay; deltoideus part, Owen, Coues; pectoralis major part, Cuvier and Laurillard; supracoracoideus part) lies immediately deep to the last, and is partially covered also by the pectoralis. It arises from the cranial two-thirds of the border of the anterior coracoid, and narrows to an insertion upon the cranial border of the proximal humerus.

M. *coracohumeralis profundus* (supraspinatus of authors) is in the same layer as, and adjacent to, the insertional end of the teres minor. It arises just medial to the ventral border of the acromion and inserts upon the more capsular part of the cranial border of the proximal humerus.

Innervation: N. coracoideus, from C5.

Meckel, Cuvier and Laurillard, and Coues correctly interpreted the superficial division of this matrix as representing m. pectoralis major of placentals, Westling indicated doubt, while McKay considered it to be a part of the deltoid. McKay gave the innervation as n. axillaris and possibly a minute twig from n. supracoracoideus (coracoideus). I found it very plainly innervated by the latter alone, placing its homology beyond question. In reptiles it lies deep to m. pectoralis, arising from the anterior coracoid, while in placentals it is entirely superficial to the true pectoral. In monotremes it is fairly intermediate in this respect, lying in the same layer with, and adjoining, the true pectoral. In this group, however, origin of the superficial portion has shifted from the anterior coracoid to the clavo-interclavicular bar, in this respect being suggestive of placentals.

The intermediate and deep layers of the anterior coracoid matrix may be discussed together. Owen, and Coues, considered the former to represent a part of the deltoid, and Cuvier and Laurillard, and Meckel, a part of the pectoral, while McKay advanced no opinion. Authors are pretty much in agreement, however, in considering the deepest division to represent m. supraspinatus.

It is extremely likely that in the broad sense these two muscles of monotremes represent mm. supraspinatus, infraspinatus, and possibly the anterior part of subscapularis of placentals, but it cannot be stated without reservation that the deepest division of the former sort of mammal represents any particular one of the three muscles in the latter sort. The deepest slip is excellently situated to invade either the anterior aspect of the scapula as a supraspinatus, to broaden and become both supra- and infraspinatus, or to be crowded caudally to become an infraspinatus only. On the other hand, the intermediate division might very easily migrate dorsally to become either one or both of the suprascapular

muscles. It either did this or was absent in the placental ancestor.

In this connection it must be remembered that the monotremes are very much specialized, and that in them suprascapular musculature, although definitely suggested, apparently has been arrested far short of the conditions indicated in Permian dicynodonts, or even some of the cotylosaurs. Accordingly the precise arrangement of the deeper coracoid musculature that finally gave rise to true suprascapular musculature may have been quite different from that now encountered in monotremes. All that can be said with conviction is that the superficial part of the matrix became m. pectoralis major, and the deeper part the suprascapular musculature, possibly including the anterior portion of the m. subscapularis. The last point will be discussed in conjunction with placentals.

Posterior coracoid matrix and brachio-antibrachial group

As in placental mammals, but in a different fashion, these two groups of muscles in monotremes are closely associated, and they present as many difficulties as in lower tetrapods.

M. *coracobrachialis superficialis* (c. longus McKay) is located upon, and is partially hidden by, the caudal border of the posterior biceps. It arises from the caudomedial part of the posterior coracoid and inserts upon the medial epicondyle of the humerus.

M. *coracobrachialis profundus* (c. brevis plus epicoracobrachialis McKay). The more ventral part of this lies deep to the posterior biceps. Origin is from most of the ventral surface of the posterior coracoid, and in addition, from the dorsal surface of both this bone and the anterior coracoid. Insertion is upon the entire dorsal surface of the proximal expansion of the humerus.

M. *biceps anterior* (caput epicoracoideus McKay) is a slender muscle caudal to m. coracohumeralis intermedius. It arises from the caudomedial part of the anterior coracoid and forms a slender tendon that partially fuses with the next.

M. *biceps posterior* (caput coracoideus McKay) caudally adjoins the last, just deep to the pectoralis. It arises from the medial part of the posterior coracoid and forms a rather broad tendon that inserts upon the radius.

Innervation: Nn. coracobrachiales, from the middle or flexor cord of the plexus, derived from C7 *et ante:* m. biceps anterior also by a filament from n. coracoideus.

M. *brachialis* is a stout muscle arising from most of the length of the humerus, between the humerotriceps on the one hand and the insertion of the deltoid and associated muscles on the other. It passes to the forearm with the biceps and is inserted upon the ulna.

Innervation: A branch of the median nerve that diverges distal to the entepicondylar foramen.

M. coracobrachialis superficialis calls for no comment. The deeper division is not only indivisible, in contrast to the situation in reptiles, but has spread around the posterior coracoid border and over the dorsal surface of both posterior and anterior coracoids in a quite remarkable manner, so that this dorsal part bears a considerable resemblance to m. subcoracoideus of reptiles, with which it frequently has been considered as homologous. The resemblance, however, is only superficial. In reptiles m. subcoracoideus is closely associated, both in conformation and nerve supply, with the dorsal subscapularis, and there is no reasonable doubt that it was derived from the latter. It is separated from the coracobrachial field by the strong posterior process of the coracoid. In the

platypus, on the contrary, the muscle occupying this situation not only is continuous, at origin and insertion, with the deep coracobrachial, but the nerve supply is the same, both very definitely by ventral components.

Any flexor muscle arising from the shoulder and inserting upon the forearm is here termed biceps. As explained in previous papers the earliest tetrapods evidently had no such muscle, but its need can be inferred from the fact that all living forms have endeavored to manufacture a biceps from such material as was at hand. The results are extremely diverse and different in Caudata, Salientia, Reptilia, and Monotremata. In reptiles conditions suggest that the distal part of the two divisions of the biceps have split from the brachialis component, while the proximal part of the anterior coracoid head has been derived from the coracobrachial matrix, as its innervation shows. In the platypus the biceps may have arisen in the same way, although it alone, without any reference to other tetrapods, shows no indication of it. If this were the case, then in some ancestor the entepicondylar foramen surrounded n. medianus at a point between the nerve branches to mm. brachialis and biceps respectively. The former thus was prevented from migrating in a proximal direction, while the latter could, and did, so migrate in conformity with any dictates of fasciculation.

As in reptiles the origins of the biceps could reach their present positions by migrating along fascial planes.

As in reptiles, also, one division of the biceps arises from the posterior coracoid, while the other has left its logical position by migrating to the anterior coracoid. Perhaps playing some part in the latter circumstance may be the fact that a slip from the anterior coracoid matrix has fused with the longer biceps, as a portion of the coracobrachial matrix has so fused in reptiles. In the platypus a nerve filament from branches supplying mm. coracobrachiales was found to enter the biceps anterior on one border, and a similar filament from n. coracoideus from the opposite direction. Histological examination showed that neither was a blood vessel, but that both were nerves.

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A SPURIOUS PORTRAIT OF SWAMMERDAM

By G. H. PARKER

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IT DOES not seem to be generally known in America that a lithographic portrait designated as that of Jan Swammerdam and seen on the walls of many of our biological laboratories is spurious. This lithograph which is reproduced in Figure 1 of this article is marked "Jan Swammerdam geboren 12 February 1637 overleden 17 February 1680 te Amsterdam." Next the lower edge of the lithograph centrally placed and in small letters is the inscription "J. P. Berghaus lith Leyden 18 $\frac{22}{\text{III}}$ 51 en gedr."; at the right hand "J. Stolker del." and at the left "Rembrandt pinxt." On a sheet of paper projecting from under a book on the table within the picture itself occur the letters "Rembr," the inference being that the portrait was painted by Rembrandt. This portrait is reproduced in W. A. Locy's *Biology and Its Makers*, page 69 (1908) and in D. C. Peattie's *Green Laurels* opposite page 38 (1936), both books much too good to carry a misleading illustration. Locy refers to it in his text with the comment "although its authenticity has been questioned, it is the only known portrait of Swammerdam." Peattie apparently accepts it as genuine. It is against unwitting appropriations of this assumed representation of the celebrated Dutch micro-anatomist that this article is written.

To anyone familiar with the works of Rembrandt the background of the Swammerdam picture shows no sign of the master's style. If the portrait came from Rembrandt its delineator certainly took the greatest liberties with the possible original. But from several Dutch correspondents I have complete assurance that there never was a portrait of Swammerdam. Certainly the circumstances of his short, bitter, and impoverished life make it highly improbable that his face should ever have been put upon canvas. A careful survey of the illustrated catalogues of Rembrandt's works gave no record of such a painting, but in the course of this search the Keeper of Prints in the Fogg Art Museum, Miss Laura H. Dudley, called my attention to the resemblance between one of the heads in the Anatomy Lesson of Dr. Tulp by Rembrandt and the so-called head of Swammerdam. The suspected head in the Rembrandt painting is that nearest the head of Dr. Tulp himself (Figure 2). The resemblance of the figure in the assumed portrait of Swammerdam to the one just pointed out is striking (Figure 3). The neck dresses in the two are not the same, being a ruff in the Rembrandt head and a smoother neck circle in the lithograph, a form of neck dress more like that worn by the other auditors in the Lesson. The paper held in the left hand of Rembrandt's auditor has large lettering upon it which though illegible for the most

part gives no sign of having to do with the natural history of the dayflies as is clearly seen in the Swammerdam picture. The poses of the two figures are in general nized that this so-called portrait of Swammerdam was a fraud and had been described as such by J. F. van Someren in the third volume of his *Catalogus van*

FIG. 1. ASSUMED PORTRAIT OF SWAMMERDAM AS DRAWN BY J. STOLKER

so unmistakably similar that I was led to ask of one of my Dutch correspondents if this resemblance had ever been noted. To this question I got an immediate reply that in Holland it had long been recog- *Portretten van Nederlanders* (1891). Here on page 611 this author remarks that the so-called Swammerdam portrait is one of the numerous frauds perpetrated by Jan Stolker who took for it the portrait of

FIG. 2. THE ANATOMY LESSON OF DR TULP BY REMBRANDT

FIG 3 THE TWO HEADS, ONE THE SO-CALLED PORTRAIT OF SWAMMERDAM (LEFT) AND THE OTHER THE HEAD FROM THE ANATOMY LESSON (RIGHT), ARRANGED FOR COMPARISON

Dr. Matthijs Kalkoen in Tulp's Anatomy Lesson by Rembrandt and by changing slightly the surroundings passed it off for one of Swammerdam. The only additional information on this matter that may be mentioned is that the face in Rembrandt's picture is believed by some not to be that of Dr. Kalkoen but of Dr. Hartman Hartmanszoon. Although the identification of this head may thus not be wholly certain those who have looked into the matter have no hesitancy in pronouncing against Swammerdam as the possible original. It seems extremely probable then that there is no authentic portrait of this highly original but unhappy man and that the so-called picture of him herein reproduced is spurious.

NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

EVOLUTION. *Fact or Fiction?*
By E. C. Wren. *Thynne and Co., London.* 2s. 6d. $7\frac{1}{4} \times 5$; 107; 1936.

A long time has passed since we have made an addition to our *Fundamentalist Portrait Gallery*. In fact we had about despaired of ever finding another literate fundamentalist. The species seems to be disappearing in America, or perhaps it is only that the New Deal's "more abundant life" has so seduced them that they have

MAJOR E. C. WREN

transferred their major allegiance from Jahweh to Santa Claus. But in England the good old bulldog breed stands firm and solid as Gibraltar in its convictions that the Bible said the last important word about biology, and that biologists are not quite bright.

It is with great pleasure that we present to our readers the portrait of Major E. C. Wren whose *Evolution—Fact or Fiction?* should be in every biological laboratory so that students may learn at first hand the fundamentalist's technique.

Major Wren's little book exemplifies this technique about as well as any anti-evolutionary effort we have ever seen. Its basis is, of course, quotation. You pick out from the writings of eminent scientific persons fragments that, when isolated, appear more than commonly idiotic. Then with righteous complacency you say: "Now then take a look at *that*." As an example which will particularly entertain our American readers for several reasons, we take the following passage from p. 8 of Major Wren's *opus* (the slight inadvertence in spelling is his, not ours):

> There would appear to be a great number of scientists who are against evolution but whose voice is seldom heard, and whose opinions seldom appear in print. Dr. R. A. Millican, the eminent physicist, said, recently, at a meeting of the American Chemical Society, that: "The pathetic thing about it is that many scientists are trying to prove the truth of evolution, which no scientist can do." It is *evidence* the public has a right to demand and should demand before accepting evolution as a science. It is evidence that is so entirely lacking. This lack of evidence is recognized by many evolutionists who only cling to evolution as an act of faith, hoping that some day some evidence may be forthcoming.

Indubitably our author has done a great deal of reading over a wide range, and has missed few morsels suitable for the nourishment of his literary baby. We were strongly tempted to make a statistical table showing the names of the persons he quotes and the frequency of occurrence of each name. The table would be impressive but the labor involved appeared too great for the result to be achieved. It would, however, make clear the range of Major Wren's preparation of his case.

One more quotation will illustrate his agility as a debater.

> It is well to know that Sir Arthur Keith has said: "as our knowledge increases the vestigial organs decrease," and "we find the same vestigial structures—the same evolutionary postmarks—in the body of man and anthropoid." The "vestigial hairs" of the human embryo are supposed to be proof of his anthropoid ancestry. The monkey embryo passes through a hairless stage, and it is only later that it grows the hairy coat with which it enters the light of day. We do not hear that this hairless stage is a "vestigial" stage proving the monkey's human ancestry. The tissues which produce the "hormones," those remarkable, mysterious, and little-understood substances, have been called "vestigial" and useless by Darwinians. They are now known to be indispensable to life. Sir Arthur Keith says of these "hormones," the product of those "vestigial" organs, that they are "Chemical substances in ultra-microscopic amounts, dispatched from one community to another in the circulating blood. Clearly a discovery of this ancient and intricate system opens up fresh vistas to the student of man's evolution. How Darwin would have welcomed this discovery." Not long ago when the tissues producing hormones were called vestigial they were claimed as good evidence in favour of evolution; to-day when they are found, after all, not to be vestigial but, on the contrary, essential to life they are just as good evidence for evolution as they were before.

We think it is a mistake to suppose, as too many biologists are apt to do, that fundamentalists are intellectually dishonest when they use this sort of argument. Some of them doubtless are, but equally some are not. We are convinced in this particular case that Major Wren is a thoroughly honest man, but terribly misguided because of his profound ignorance of biology. We feel sure that he sincerely regards such points as those we have quoted as real and significant evidence against the occurrence of organic evolution as a fact of nature. Which brings us to the reiteration of the statement made at the beginning: namely that this book should be in every biological laboratory so that students by reading it can really understand, in the light of their first-hand knowledge of the data of biology, how intrinsically feeble and far removed from reality are the arguments brought against the idea of evolution, and what a difference it makes on one's outlook really to have some precise and ordered knowledge of biology and the facts of nature generally.

PROBOSCIDEA. *A Monograph of the Discovery, Evolution, Migration and Extinction of*

the Mastodonts and Elephants of the World. Volume I: Moeritherioidea, Deinotherioidea, Mastodontoidea.

By Henry Fairfield Osborn. Edited by Mabel R. Percy. The American Museum Press, New York. $20. $12\frac{1}{2} \times 10\frac{3}{4}$; xl + 802 + 2 folding charts; 1936.

This magnificent and fundamental contribution to the facts and theory of organic evolution will stand as the final and fitting monument to the genius of Henry Fairfield Osborn. This first volume only half completes the work, but the second and concluding volume is promised for the current calendar year.

The most striking characteristic of the monograph is its thoroughness—a feature that marked all of Professor Osborn's paleontological work. Next is the wealth, precision, and beauty of the illustrations. Here, once more, is biology in the grand manner that we see so little of today in a world lavishly extravagant in wasting money on ephemeral enterprises, and excessively parsimonious in supporting enterprises of permanent worth to mankind. Great sections of biology still are, and will long remain, necessarily descriptive in their technique of investigation and for these regions adequate illustration is an absolute and ineluctable necessity. Precise and comprehensive descriptions of the world of living things—past and present—still constitute one of the major needs of biology as a science.

The significance of this monograph for students of evolution cannot be better indicated than by quoting Professor Osborn's own estimate of a part of it.

> Not revolutionary, but in accord with the clocking of geologic time by similar stages of evolution (homotaxis, Huxley) long in use by invertebrate palaeontologists, is the evidence yielded by intensive examination which the Proboscidea afford for Tertiary geologic correlation of stages in adaptive progression and retrogression in the widely distant distribution centers of mastodonts and elephants which migrated into all the continents except Australia. The continuous evolution of far-separated adaptations of the grinding teeth, in northern and southern Africa, in Eurasia and in North and South America, can now be coördinated, with unexpected precision, as in the outstanding metamorphosis of *Archidiskodon proplanifrons* of South Africa into the gigantic *Archidiskodon maibeni* of Nebraska. In this correlation the intensive study of the grinding teeth in the mastodontoid and elephantoid divisions plays a leading part.
>
> Another result which will prove to be revolutionary in anthropology is the new means afforded of dating precisely the main periods of the prehistoric evolution of Man, by intensive measurement of the length, width and thickness of the enamel layers in the grinding teeth of the elephantoid division in successive stages from Upper Pliocene to closing Pleistocene time. Man was a mastodon and elephant hunter from early times. Remains of fossil elephants are occasionally found embedded in the same strata with remains of fossil man, and the total length of the enamel foldings in proboscidean grinding teeth enables us to date relatively the successive phases in the evolution of man.
>
> From the author's prolonged thirty-five-year research on Titanotheres and Proboscidea there issue not only the principles governing the classic modifying modes of evolution known to Lamarck and Darwin (variation, development, degeneration) but also the newly discovered and hitherto unrecognized principle and modes of the origin of new characters through aristogenesis or creative biomechanical rectigradation.
>
> Ascending mutations, species and genera are principally defined by continuously progressive changes in the aristogenes and proportions of the grinding teeth and in the upper or lower incisive tusks. Although the rate of proportional adaptation varies enormously, for the first time we know approximately how long a period of geologic time it takes to produce a full-fledged and highly efficient adaptation, as in the metamorphosis of the lower incisive tusks of Oligocene *Phiomia* into the dominant shovel-tusks of Pliocene *Amebelodon*, or in the metamorphosis of the posterior grinders of *Trilophodon* from Lower Miocene into Mio-Pliocene.
>
> Through the clear distinction between change of proportion (alloiometry) and the origin (aristogenesis) of new parts, also through the newly discovered multiple lines (forty-one or more) of ancestry and ascent technically known as phyla, the Proboscidea afford a complete revolution in our biological philosophy and concepts of the nature and causes of evolution.
>
> Never before has it been possible to follow many lines of phylogenetic ascent over extremely long periods of geologic time, noting the progressive adaptive changes in each organ in each phylum to gain the perfection of certain mechanisms at the expense of other mechanisms. In general, the specialization of certain organs becomes more intense, while closely contiguous organs remain absolutely stationary. For example, among the shovel-tuskers, *Phiomia osborni* of the Oligocene of Egypt gives rise to the incredibly specialized *Amebelodon fricki* of Nebraska, with a relatively similar and unchanging skeleton and limbs. *Amebelodon* is paralleled by the flat-tusker *Platybelodon* of the Gobi Desert and of Nebraska in which the whole jaw becomes an enormous shovel, the upper jaw and skull being sacrificed and thus greatly reduced in size.

It gives us great pleasure that an accident of editorial timing makes it possible for us to place side by side this and the preceding review. So long as biologists continue to pile up the kind of real

evidence for organic evolution that Osborn's Proboscidea does, the world has no reason to worry over the activities of the fundamentalists.

GENETICS

EUGENICAL STERILIZATION. *A Reorientation of the Problem.*

By The Committee of the American Neurological Association for the Investigation of Eugenical Sterilization: Abraham Myerson, James B. Ayer, Tracy J. Putnam, Clyde E. Keeler and Leo Alexander. The Macmillan Co., New York. $3.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; xii + 211; 1936.

By an ample margin this volume stands in the front rank in the eugenic field of discourse for its scientific soundness, unemotional sanity, and plain common sense. After pointing out that the "most of the legislation [for eugenic sterilization] which has been enacted so far is based more upon a desire to elevate the human race than upon proven facts," the Committee makes the following recommendations:

First. Our knowledge of human genetics has not the precision nor amplitude which would warrant the sterilization of people *who themselves are normal* in order to prevent the appearance, in their descendants, of manic-depressive psychosis, dementia praecox, feeblemindedness, epilepsy, criminal conduct or any of the conditions which we have had under consideration. An exception may exist in the case of normal parents of one or more children suffering from certain familial diseases, such as Tay-Sachs' amaurotic idiocy.

Second. Particularly do we wish to emphasize that there is at present no sound scientific basis for sterilization on account of immorality or character defect. Human conduct and character are matters of too complex a nature, too interwoven with social conditions, such as traditions, economics, education, training, opportunity and even prejudice, especially when these factors operate in the earlier years of life, to permit any definite conclusions to be drawn concerning the part which heredity plays in their genesis. Until and unless heredity can be shown to have an overwhelming importance in the causation of dangerous anti-social behavior, sterilization merely on the basis of conduct must continue to be regarded as a "cruel and unusual punishment."

Third. Nothing in the acceptance of heredity as a factor in the genesis of any condition considered by this report excludes the environmental agencies of life as equally potent and, in many instances, as even more effective. That scientific day is past when the germplasm and the environment are to be considered as separate agencies or as opposing forces. Both operate in the production of any character, though in different degrees, but the degree in which each operates is, at present, mostly in the field of the unknown.

The book records an important job of work well done. We congratulate the American Neurological Association on their acumen in promoting the inquiry and picking the men to carry it out.

The book is well planned and simply written without undue technical rumble-bumble. The bibliographic documentation runs to 20 pages. There are author and subject indexes.

YEARBOOK OF AGRICULTURE, 1936.

U. S. Department of Agriculture. Government Printing Office, Washington. $1.25. 9 x $5\frac{3}{4}$; 1189; 1936.

AGRICULTURAL STATISTICS, 1936.

U. S. Department of Agriculture. Government Printing Office, Washington. 50 cents. $9\frac{1}{4}$ x $5\frac{3}{4}$; 421; 1936 (paper).

This year, instead of the usual "brief summaries of miscellaneous new developments in agriculture," the *Yearbook* presents a single subject: "the creative development of new forms of life through plant and animal breeding." The material contained in the volume is the result of a survey made by the Committee on Genetics. It consists chiefly of a discussion of specific improvements and general progress made in 19 major crops and types of livestock (wheat, cotton, sugar beets, cattle, swine, etc.). In addition, however, the reader is introduced into the general field of genetics and made aware of the importance of this subject in sections on Better Plants and Animals; Heredity Under the Microscope; and Unusual Possibilities in Breeding.

Technical terms have been eliminated wherever possible, but since in some cases it was necessary to use them, a glossary is provided. Each section has been written by a specialist and each represents a comprehensive survey of its subject. The text is illustrated with tables and photographs and at the end of each section there is a list of the literature cited. Anyone interested in breeding, whether

commercial or experimental, will find much interesting material in this volume.

The volume of *Agricultural Statistics* contains information which, until this year, was published in the statistical section of the *Yearbook of Agriculture*. The usual "most important agricultural statistics of the United States and of the world so far as the agriculture of this country is concerned" are presented.

GENERAL BIOLOGY

EXPERIMENTELLE BEITRÄGE ZU EINER THEORIE DER ENTWICKLUNG. *Deutsche Ausgabe der Silliman Lectures Gehalten an der Yale University im Spätjahr 1933.*

By Hans Spemann. Julius Springer, Berlin. RM. 27 (paper); RM. 29.60 (cloth). 9¾ x 6¼; viii + 296; 1936.

This monograph, a German translation of a series of lectures delivered at Yale University in 1933, contains an excellent summary of many experimental embryological studies including remarks on the technique involved; a lengthy discussion of the relation of these studies to the problems of ontogeny, and a splendid bibliography of both recent and classical titles. The volume is especially noteworthy in its attempt to weave a great mass of isolated facts into a general picture of development the central key of which is the conception of embryonic induction. The author emphasizes primarily experiments in which a controlling region or "organizator" in the embryo is shown to play a definite rôle in shaping differentiation by guiding the pattern of growth.

This book by a distinguished author is a tribute to its field and merits the serious attention of all biologists. A number of illustrations supplement the text. There is no index.

Организация Клетки

Н. К. Кольцов. Государственное Издателство биологической и медицінской литературы, Москва-Ленинград. 14. руб.; 652; 1936 [THE ORGANIZATION OF CELL.

By N. K. Koltzoff. State Publishing House for Biological and Medical Literature, Moscow-Leningrad. 14 roubles; 652; 1936 (cloth)].

In this volume, dedicated to the coming centenary of the cell theory, are collected papers published by Professor Koltzoff in the course of the last thirty-three years. Practically all of them were published in the past in the two languages, Russian and German, and in this way made available to the biological world.

These collected papers, representing the life-work of one of the leading Russian biologists, give an impressive account of fruitful attempts to apply the methods and modes of thought of physics and chemistry to cytology and biology in general. Having started his career as a comparative anatomist, Professor Koltzoff became soon interested in experimental cytology and published a number of interesting papers on cellular morphology from a physico-chemical viewpoint. In the Institute of Experimental Biology, organized by him in Moscow, he has for many years cultivated cytology, physiology of development, and genetics. Many excellent articles have issued from this Institute, and many of his students and associates, particularly in genetics, have now become leading Russian scientists with world-wide reputations.

OCTOBER FARM. *From the Concord Journals and Diaries of William Brewster.*

Edited by Smith O. Dexter. Introduction by Daniel C. French. Harvard University Press, Cambridge. $2.50. 7½ x 5¼; xv + 285 + 5 plates; 1936.

This book consists of excerpts from Brewster's voluminous diaries, kept during practically his whole life. The material extracted deals particularly with observations about Concord where the author lived on his home, October Farm. There is a forword by T. Barbour and a partly biographical introduction by Brewster's life long friend, Daniel Chester French. For once we feel that we have read a diary that was not written with an eye to publication. In a simple, unaffected and quite unexciting style the author describes the country scenes and wild animals he observed over a period of forty-seven

years in his daily wanderings about his farm.

Studien über das Zusammenspiel von Hypophysen- und Ovarialhormonen, insbesondere im Lichte von Parabiose-versuchen.

By Ejnar Møller-Christensen. Levin and Munksgaard, Copenhagen. 10 x 6½; 157 + 3 plates; 1935 (paper).

The first part of this book is a brief résumé of earlier work on the effects of hypophysectomy on the sexual apparatus and function. In the experiments discussed in detail in the second part it was demonstrated that the oestrus cycles of two normal, adult female rats ran independent courses during parabiosis, and likewise that such union between normal males and normal females effected no changes in the organs or functions of either. However, in parabioses between castrated and normal animals, the hypophysis of the normal animal exercised a retarding effect on the oestrin activity and ovarian cycle of that animal. The ovaries, uterus and oestrus cycle remained normal in the normal rat during parabioses of hypophysectomized and normal female rats.

Each part is followed by a bibliography. The illustrations include text figures and three colored plates.

Our Natural Resources and Their Conservation.

Edited by A. E. Parkins and J. R. Whitaker. Contributions by various authors. John Wiley and Sons, New York; Chapman and Hall, London. $4.00. 9 x 5¾; xii + 650; 1936.

A general survey of the entire field of our natural resources is presented here through the coöperation of twenty-two authors. The major topics selected for discussion are soils, forests, water and minerals and the various problems relating to these resources and their conservation. The extent and distribution of each resource and its service in national and regional development is discussed and the degree of exploitation considered. There are chapters of general and economic interest on the agricultural outlook; the conservation of natural resources in relation to the manufacturing industry; and on "The Conservation of Man." Lightly touched upon are problems relating to private versus state or federal control of the conservation and exploitation of some of the natural resources. A bibliography and index are included.

Entwicklungsbiologie und Ganzheit. *Ein Beitrag zur Neugestaltung des Weltbildes.*

By B. Dürken. B. G. Teubner, Leipzig and Berlin. RM. 6.80 (cloth); RM. 5.80 (paper) (in Germany). RM. 5.10 (cloth); RM. 4.35 (paper) (outside of Germany). 9 x 6; vi + 207; 1936.

This work by the Director of the Institute for the Experimental Analysis of Development and Heredity at the University of Breslau gives us a comprehensive exposition of the biology of development from the viewpoint of biological holism. Of strictly scientific character and at the same time written in a lucid style, provided with numerous illustrations and elucidating notes together with references to the literature on the subject, it will prove of great value not only to the professional biologist but also to the educated layman interested in the modern trends of scientific research. A list of explanations of the most important technical terms and an index of names and topics are provided.

Urdeutschland. *Deutschlands Naturschutzgebiete in Wort und Bild. Lieferungen* 21, 22, 23.

By Walther Schoenichen. J. Neumann, Neudamm. 2 marks each. 10¾ x 8¼; Lief. 21, 193–216 + 9 plates; Lief. 22, 217–240 + 9 plates; Lief. 23, 241–280 + 9 plates; 1936 (paper).

The latest numbers of a publication, already mentioned frequently in this journal. Parts 21 and 22 continue with the account of Parks begun in Part 20, and then deal at greater length with marsh lands of North Germany, and the plant and animal life found in them.

Part 23 deals with the heaths. All three sections contain many beautiful full page illustrations.

Transzendentale Grundlagen der Biologie.

By Erich Oelze and Otto Schmith. Johann Ambrosius Barth, Leipzig. RM. 1.80. $8\frac{1}{4} \times 5\frac{7}{8}$; 36; 1937 (paper).

A philosophical analysis of the fundamental concepts and presuppositions of biology and a critical examination of the theories of monism, mechanism, holism and vitalism in the light of Kantian epistemology. The worthy aim is to establish the science of biology upon a sound rational basis.

Practical Photo-Micrography.

By J. E. Barnard and Frank V. Welch. Longmans, Green and Co., New York; Edward Arnold and Co., London. $8.25. $8\frac{1}{2} \times 5\frac{3}{8}$; xii + 352 + 23 plates; 1936.

This text is practical in fact as well as in name, being clearly written and not too technical for those who wish to use photo-micrography in their work without going into it as a separate science. In the third edition more attention has been given to the photography of metal surfaces. Sections have been added on the subjects of infra-red and ultra-violet photography.

The Mendel Bulletin, *Volume IX, Number I.*

Villanova College, School of Science, Villanova, Pa. 35 cents a copy; $1.00 a year. $9\frac{1}{4} \times 6\frac{3}{4}$; 28; 1936 (paper).

This is the first number, in new format and attractively printed, of a quarterly that had hitherto been an annual.

HUMAN, BIOLOGY

The Puritan Pronaos. *Studies in the Intellectual Life of New England in the Seventeenth Century.*

By Samuel E. Morison. New York University Press, New York; Humphrey Milford, London. $3.75. $9\frac{1}{4} \times 6$; x + 281; 1936.

Religion is usually considered the most important factor in the intellectual life of Puritan New England. In this book the author emphasizes also the interest in the humanities, which included secular poetry and the classics, and in science. The establishment of elementary schools, public grammar schools and Harvard College so soon after the arrival of the colonists in America indicates the importance they attached to education. When political and historical literature appeared, it was made available by printers and book sellers, and preserved in the libraries. The overwhelming importance of religion in the Puritan civilization is brought out, and the sermon is discussed both as a literary form and as theological exposition.

In providing institutions that later generations could put to new uses, the New Englanders were more successful than in actual intellectual production. No lasting books were written, no new or significant ideas were worked out on New England soil; but three institutions of lasting significance for American life were firmly established: the college, the public-school system, and the Congregational Church. The story of the intellectual life of New England in the seventeenth century is not merely that of a people bravely and successfully endeavoring to keep up the standards of civilization in the New World; it is one of the principal approaches to the social and intellectual history of the United States. Primitive New England is a puritan pronaos to the American mind of the nineteenth century, and of today.

The Cradle of Mankind. *Life in Eastern Kurdistan. Second Edition.*

By W. A. Wigram and Edgar T. W. Wigram. A. and C. Black, London; The Macmillan Co., New York. $4.00. $8\frac{1}{4} \times 5\frac{1}{2}$; xi + 430 + 16 illustrations + 1 folding map; 1922.

This is an altogether remarkable book. The country with which it deals lies roughly between the easternmost end of the Mediterranean and the southwestern border of the Caspian Sea, and two or three hundred miles south and a little east of the southern border of the Black Sea. More precisely it lies for the most

part between the upper reaches of the Euphrates and Tigris rivers. It is a country of wild and magnificent scenery. It contains some of the most venerable monuments in the world. "It is the very *fons et origo* of our Indo-European ancestors. Its traditions connect it with the Garden of Eden, with Noah, and with Abraham. Its folk-lore preserves the old Nature-worship which originated in the brains of the Apeman. Its history records the very dawn of civilization, and the rise and fall of the earliest of the great empires."

What makes this book so extraordinarily interesting is fundamentally the fact that the every-day life of the present inhabitants remains to this moment in all essential details the life of Europeans in the Dark Ages—a sort of living paleontological record of human biology. The country is extremely inaccessible and does not welcome visitors gladly—quite the contrary in fact. "Never another law hae they but the length of their dirks; the broad-sword's pursuer, and the target is defender, and the stoutest head bears langest out."

The senior author lived in Eastern Kurdistan ten years as a member of the "Archbishop of Canterbury's Assyrian Mission," which exists at the request of the Patriarch and other authorities of that most ancient communion, the Nestorian Church. He quite evidently got the complete confidence, respect and affection of the people, as his understanding humanity, tolerant good humor, and native wit obviously fitted him to do. He writes simply and with great charm. The result is, as we have said before, a very remarkable book. We recommend it in the highest terms.

Africa's God. III. *Nigeria. Anthropological Series of the Boston College Graduate School, Volume 1, Number 3.*

By Joseph J. Williams, S. J. Boston College Press, Chestnut Hill, Mass. $1.00. $9\frac{3}{4}$ x $6\frac{1}{2}$; 56; 1936 (paper).

Africa's God. IV. *French West Africa. Anthropological Series of the Boston College Graduate School, Volume 1, Number 4.*

By Joseph J. Williams, S. J. Boston College Press, Chestnut Hill, Mass. $1.00. $9\frac{3}{4}$ x $6\frac{1}{2}$; 32; 1936 (paper).

Nigeria lies on the west African coast just north of the equator. The author attempts to show that the bronzes of Benin actually antedate the advent of the Portuguese to West Africa, and that they are closely associated with their religious beliefs. He quotes a few earlier accounts of coastal Nigeria and passes on to a consideration of modern narratives. He also takes up answers to his questionnaire which was originally sent out in 1932. The last part of the monograph is an account of Stephen Septimus Farrow who has from personal experience delved into the esoteric secrets of the Yorubas as a people. In conclusion, the religious status of the Nigeria tribes is less clearly defined than the author's findings for other tribes in previous monographs. He claims, however, that there is unquestionably a Supreme Being extant among them.

In French West Africa, just north of Nigeria, there is further retrogradation due to Mohammedan influences. The author finds a decadent form of monotheism existent among the pagan tribes. They offer no cult to the Supreme Being, but they still recognize Him as the Creator.

Both monographs are carefully planned and well written. They each have long bibliographies and two alphabetical indices for persons and topics discussed.

The Life and Convictions of William Sydney Thayer, Physician.

By Edith G. Reid. Oxford University Press, New York. $2.50. $8\frac{1}{4}$ x $5\frac{1}{2}$; xii + 243; 1936.

Although less publicized than some of the early Hopkins medical luminaries, William Sydney Thayer certainly deserves, on both professional and personal grounds, a biography. It is eminently apparent that his biographer, Edith Gittings Reid, also is of the same opinion for one notes patent and not always subtle evidences of "hero-worship" even before the first page has been turned. This attitude persists throughout the volume but, happily, is kept from being irritating by the genuine

enthusiasm of the author for her subject and by the introduction into the text of numerous interesting anecdotes. The reviewer, not having had the privilege of knowing Doctor Thayer personally, is unable to appraise critically the character analysis developed in the book. It is felt, however, that an interesting and commendable job of reporting is done that results in a portrait of a man artfully skillful as a diagnostician and successful as a practising clinician; sartorially impeccable; rock-bound New Englandish (and proud of it); militaristic and completely the gentleman. Not being a medical person herself the author wisely refrains from entering into research or technical discussions, but nevertheless succeeds in turning out a pleasant little volume that will be especially welcome around Baltimore and other American medical centers. The book has an index and an interesting outline-log of Thayer's life including such memorabilia as collegiate degrees, positions, society memberships, editorships, and world-war records. Several photographic studies accompany the text.

Audubon.

By Constance Rourke. Harcourt, Brace and Co., New York. $3.00. $8\frac{3}{4} \times 6\frac{1}{4}$; 342 + 12 plates; 1936.

For years a mild interest has centered about the personage of John James Audubon. Recently, when Donald Culross Peattie in his excellent book *Green Laurels* compared Audubon with Wilson (somewhat to the former's disadvantage) a new interest in the man and his works cropped up. Peattie, having many illustrious naturalists to epitomize, touched on Audubon only briefly and provocatively. He succeeded, however, in unconsciously setting the stage for a more extended biography which has been attempted in the present book.

The author, Constance Rourke, is well-known as an interpreter of frontier Americana and approaches Audubon, his peregrinations and his accomplishments from this viewpoint. The book, after briefly reviewing Audubon's boyhood in France, soon carries him to America. There he engaged in commerce and amateur ornithology—the former with tepid success—the latter with growing artistic brilliance. In the remaining pages Audubon's character, matrimony, personal quirks, woodland jaunts and bird studies are expounded. The author succeeds in making all these stories interesting, and, although the book is not a penetrating character study, it does fill competently a niche in the shelves of American biographies and, as such, should enjoy considerable popularity. The text is illustrated with 12 colored plates taken from original Audubon prints and has an index and annotated bibliography.

Man Makes Himself.

By V. Gordon Childe. Watts and Co., London. 7s. 6d. net. $7\frac{3}{4} \times 5$; xii + 275; 1936.

One of the most distinguished authorities on the archeology of Europe and the near East carries the arguments and evidences of the economic interpretations of history into the fertile field of prehistory. Avoiding controversial points and "high sounding terms that give confusion the semblance of logic," Professor Childe discusses the origin and development of the more important cultural traits upon which civilizations are based—stressing the material traits but including those of social organization and religion—from Paleolithic times to the dawn of recorded history. The rôles of diffusion, environment (raw materials, geographic position, climate, etc.), race and other factors contingent upon the growth of culture are considered not in contrast to economic factors, but as intimately and inextricably bound with them. Of particular interest to human biologists are his evidences of the phenomenal increase in population following upon the first two revolutions—the Neolithic Revolution, which was based upon the production of food through domestic plants and animals in contrast to hunting and collecting; and the Urban Revolution, attendant upon the inventions of the techniques of metallurgy and involving highly developed trade, specialized craftsmen, etc. These population

spurts closely parallel the well known one connected with the third or Industrial Revolution. In easy style and with masterful command of the data of prehistory, Professor Childe demonstrates "how economic revolutions . . . promoted the growth of institutions, science, and literature—in a word, of civilization as currently understood."

THE MYSORE TRIBES AND CASTES. *Volume I. By L. K. Ananthakrishna Iyer. The Mysore University, Mysore.* Rs. 15 or 24s. $8\frac{1}{2} \times 5\frac{1}{2}$; lxxii + 205 + 66 plates + 2 folding maps; 1935.

While this is indicated as Volume I, actually it is the fourth and last of a series of publications to report on a detailed ethnographic survey of Mysore. This country which has a population of about 5 million and an area of almost 30,000 sq. miles is second in importance only to Nizam among the Indian States. Situated as it is, near the southern tip of India, it has been touched by practically all the cross-currents of population movements which have taken place in India since time immemorial. As a result the anthropologist finds here an interesting conglomeration of different ethnic groups, some of which have modified considerably while others maintained their original customs. In this volume, a summary of the preceding ones, the author first describes in detail these historical movements and then discusses the effects on the original mores due to contact and diffusion. The origin of castes is one of the principal questions on which he attempts to throw some light. He recalls how they are already mentioned in the Rig Veda and outlines the important factors which have favored the formulation and maintenance of such endogamous groups: contact with foreign peoples, similarity of occupation, of culture and of religion. In some detail he compares the customs and beliefs held by the different tribes and castes. The book closes with an appendix containing a most interesting description of the mores of six tribes whose main occupation is thievery.

It really seems unnecessary to add that this is a most important contribution to anthropology for the wealth of material which it contains and for the scholarly treatment of the subject.

THE AMERICAN PEOPLE. *Studies in Population. The Annals of the American Academy of Political and Social Sciences, Volume 188. Edited by Louis I. Dublin. The American Academy of Political and Social Science, Philadelphia.* \$2.50 (cloth); \$2.00 (paper). $9\frac{1}{4} \times 6\frac{1}{4}$; xii + 396; 1936.

A group of America's most eminent demographers have contributed to the special population number of this journal. Its purpose, as Dublin states, is to exhibit the present status of population studies in this country and from this summary possibly develop an intelligent and consistent point of view towards the general problem of population. The articles, 33 in number, are distributed in 6 sections which treat, respectively, of reproduction, morbidity, population increase and structure, relation of population to resources, world population problems and organization of population research. A few of the articles present original data but the remainder simply provide excellent and scholarly summaries of the known facts regarding each particular topic. A number of authors conclude with a lamentation about the scarcity of data and with a plea for more and more accurate information. This is necessary, but it is doubtful whether the lack of data is alone responsible for the general sterility exhibited by some of these articles. Demography is at bottom only a branch of human biology and until it is well founded on biologic bases its study will generally consist of periods of preoccupation about surplus population and periods of preoccupation about insufficient population.

THE STRUGGLE FOR POPULATION. *By D. V. Glass. With an Introduction by A. M. Carr-Saunders. Oxford University Press, New York; Clarendon Press, Oxford.* \$2.75. $8\frac{3}{4} \times 5\frac{1}{2}$; x + 148; 1936.

This is the outcome of an inquiry undertaken by a select Committee under the

chairmanship of Professor Carr-Saunders set up by the Council of the Eugenics Society (English). The main idea was to find out why the birth rate was declining and what had best be done about it. The net upshot of the inquiry—after reaching the sound conclusion that an increase in contraception and abortion is not a quite complete or satisfactory explanation of slowing rates of population growth—is stated in the following words:

Our conclusion is, then, rather important even though it is not positive. What seems imperative is, first, a series of detailed studies of movements within the population of this country, and, secondly, a careful analysis of the factors which are urging people to keep down the size of their families. Until this is done, attempts to raise the birth-rate will be so much struggling in the dark with small chance of success. There are only two points on which we may be fairly positive at present. First, repressive measures are unlikely to be effective; what appears to be much more necessary is the creation of a general environment conducive to the bringing up of relatively large families. Secondly, if there is to be any significant increase in the birth-rate, the major part must come from the working-class. Consequently, no action is likely to have a permanent influence unless it provides conditions in which the working-class is able to bring up children without thereby suffering from economic and social hardship.

Custom is King. *Essays Presented to R. R. Marett on his Seventieth Birthday, June 13, 1936.*

Edited by L. H. Dudley Buxton. Hutchinson's Scientific and Technical Publications, London. 12s 6d. $8\frac{1}{2} \times 5\frac{1}{2}$; xiii + 325 + 4 plates; 1936.

This volume contains a series of 19 essays written by students and colleagues of Professor R. R. Marett as a tribute for his seventieth birthday. While the articles cover a rather wide range of subjects in the field of cultural anthropology and ethnology the majority of them adhere to the general plan of the book as indicated by the title, and report observations on the customs of peoples. The two subjects mainly discussed are religious beliefs and intra-tribal relationships. Thus there are articles on totemism in West Africa and along the north-west coast of North America, kinship, incest and exogamy in the northern territories of the Gold Coast, bond friendship in Tikopia, the Wiro sky-god, the chameleon and the sun-god Lisa on the West-African slave coast, the Paradise myth, religion in south-eastern Asia. There are, besides, papers dealing with such generalities as the Western seaways, snobbery and the relation of physical anthropology to cultural anthropology. Except for the last two articles mentioned which are of a philosophical nature, the remaining are characterized by a clear description of observations and little or no speculation. All are rather brief but well written. The book contains also a short sketch of the scientific career of Professor Marett and a bibliography of his scientific writings.

Comte. *The Founder of Sociology.*

By F. S. Marvin. Chapman and Hall, London. 6s. net. $7\frac{1}{4} \times 4\frac{7}{8}$; 216; 1936.

The author introduces this book with the question: "Can Comte really be called a philosopher?" and dedicates the chapter to prove the affirmative. It is Comte, the philosopher, the idealist, the metaphysician, who is important in the author's view and it is primarily from this standpoint that he evaluates the contribution of the father of Positivism. This volume contains a brief sketch of Comte's rather sad existence, his social and domestic environment, and the influence of St. Simon. The author emphasizes Comte's dream of a regimented universe, under the leadership of a scientist, in which humanity would not suffer and in which there would be universal peace. Unless the author is deliberately courting the favor of pacifists *et similia*, it is not readily understood why he returns to this point again and again while at the same time he is forced to defend the fact that Comte's hopes for establishment of Positivism never came true. The author goes so far as to state that the League of Nations, "the largest new definite factor in the world" (sic) is already an approach to the ideal of Comte. There is more in Comte's sociology than "humanity," "faith," etc. and it is a pity that the author should have made him into a mystic.

THE CLEAR MIRROR. *A Pattern of Life in Goa and in Indian Tibet.*

By G. Evelyn Hutchinson. The University Press, Cambridge; The Macmillan Co., New York. \$2.50. $8\frac{3}{4} \times 5\frac{1}{2}$; xi + 171 + 13 plates; 1936.

This book is disappointing. It does not give us what the subtitle promises—a pattern of life in Goa and in Indian Tibet. The author uses as a symbol a Spanish mirror to which is fastened a figure of the Madonna. Spain and Portugal sent out "a spiritual army entrusted with placing the Madonna in front of every strange and barbarous culture in which could be recognized the distorted image of her own." But then he does not interpret the civilizations of which he writes in accordance with this idea.

The section on Goa is reminiscent of a guide book with the exception of a description of the feast of St. Catherine. In the part on Tibet, the paintings in the temples are described and an account of the Black Hat Dance is given. The last section is devoted to the numerous lakes of Tibet and the forms of life found in them.

It is obvious that the author is an observant traveler, but his book is not very interesting.

THE GENTLE SAVAGE. *A Sudanese Journey in the Province of Bahr-el-Ghazal, commonly called "The Bog."*

By Richard Wyndham. William Morrow and Co., New York. \$2.75. $8\frac{1}{2} \times 5\frac{3}{4}$; 287; 1936.

The author—an English artist—writes with an ease not common to all, and in this interesting story of a three months' sojourn in "the Bog" of Anglo-Egyptian Sudan he has demonstrated an ability to portray vivid word pictures just as he can produce with paint and brush those pictures for which he is better known. He is quite naturally interested in the natives as models for his canvas, but his interests are by no means limited to this feature. He finds pleasure in observing odd characters, the traits and characteristics of the various tribes he comes to know and in particular of those individuals with whom he is most closely associated. Two young girls selected for models had to be purchased with cattle since their parents refused to see the difference between a model and a wife. Accounts of curious scenes, minor adventures, and details of everyday life among these black people are all woven into the fabric of the tale. The photographs with which the book is illustrated are artistic and extremely well reproduced.

OPPORTUNITIES FOR THE MEDICAL EDUCATION OF NEGROES.

By E. H. L. Corwin and Gertrude E. Sturges. Introduction by Walter L. Niles, and a Foreword by Walter White. Charles Scribner's Sons, New York. \$1.50. $7\frac{1}{2} \times 5\frac{1}{4}$; xv + 293; 1936.

In 1920 the first appointment of a Negro physician was made at Harlem Hospital—a city hospital situated in the heart of the Negro district of New York. In 1933 there were 27 Negroes on the indoor staff. But during this period there was considerable agitation over the appointments made. One faction opposed the appointment of graduate students of Negro medical colleges and approved only of those Negroes who had competed successfully with whites in the northern medical schools of high standing. This brought to a head a consideration of the problem of opportunities for advancement in the medical education of those graduates of Negro institutions who are eliminated through racial prejudice from practice in practically all large hospitals. This book is the report of a critical study made of Harlem Hospital by a group of medical experts and laymen from a point of view of its peculiar fitness for the further training of all qualified Negro doctors. A brief account is also given of the Negro medical, hospital and health situation in the country at large.

PLASTIQUE MAMMAIRE. *Considérations médico-chirurgicales.*

By Irène Bernard. Librairie Maloine, Paris. 40 francs. $10 \times 6\frac{1}{2}$; 245; 1936 (paper).

The pamphlet *L'Esthétique mammaire à travers l'histoire* by C. Claoué and I. Bernard, noticed in the immediately preceding number of the Q. R. B. is reprinted here to serve as the first chapter. Then follows a description and evaluation of the various measures which have been tried in France in an effort to make the feminine breasts conform to the modern ideal of beauty. The operative procedure of Claoué, under whom the author worked, is recommended on the basis that no scars are left on throat or arm-pits. This is undoubtedly an advantage.

The illustrations consist largely of photographs and diagrams of stages in operative procedure used by the leading plastic surgeons, and "before" and "after" pictures. The bibliography is confined to French titles, from which, curiously enough, references to the writings of the men whose methods have received the greatest discussion in the text, with the single exception of Claoué, are omitted.

STONE-AGE BUSHMEN OF TO-DAY. *Life and Adventure among a Tribe of Savages in North-Western Australia.*

By J. R. B. Love. Blackie and Son, London and Glasgow. 8s. 6d. net. $8\frac{1}{2} \times 5\frac{1}{2}$; xxiii + 220 + 16 plates; 1936.

This book is an account of the life and customs of the Worora, a tribe of savages in North-Western Australia, and of the author's experiences among these people. J. R. B. Love, a missionary, studied these aborigines and learned their language so that he might really understand them. In this book we receive the full benefit of his work, for he has given us a detailed and interesting picture of these primitive men.

At the end of the volume Mr. Love says, "the gulf between the most highly civilized man and the most savage is far less than many a casual thinker believes." And the reader is apt to agree with him when he realizes that this tribe of savages is actually a well-organized social group with its standards, morals, and taboos not so very different from those of our own more highly civilized society.

THE ECONOMICS OF PRIMITIVE PEOPLES.

By Stephan Viljoen. P. S. King and Son, London. 12s. 6d. $8\frac{1}{2} \times 5\frac{1}{2}$; 282 + 2 plates; 1936.

This is a synthesis of the social conditions of primitive peoples with special reference to their ways of making a living. In order, the author discusses the influence of environmental conditions, climate and other geographical elements; then the demographic factors resulting from their mode of living and customs. There follow chapters on the several types of economy found among them, the variations observed among peoples following the same general type, the difference in the development of arts and crafts, division of labor, class division, trading technique, and money. In brief, it is a complete survey of the economic system as manifested among peoples as diverse as Eskimos and Bushmen. Moreover, while it is mainly descriptive, the author shows a remarkable ability in deftly disposing of theories that do not correspond to facts. There is an index and a fairly adequate bibliography.

RATS, PLAGUE, AND RELIGION. *Stories of Medical Mission Work in India.*

By John Spencer Carmen. The Judson Press, Philadelphia. \$1.25. $7\frac{1}{2} \times 5$; viii + 246 + 11 plates; 1936.

The author of this small volume gives in a series of realistic and intensely interesting incidents, some of his experiences as a medical missionary in India. As might be inferred from the title it is not altogether pleasant reading. Nevertheless the book fascinates, stirs sympathy, and leaves a lasting impression of the excellent work being done by some missionaries in foreign fields. Dr. Carmen describes some of the deplorable sanitary and hygienic conditions existing in rural India, and tells how they are being remedied even in the face of strong opposition because of customs, castes, and superstitions.

The book has several maps of the areas of India considered, and a number of photographs typifying life and living conditions in the country.

KÖRPERBAU UND CHARAKTER. *Untersuchungen zum Konstitutionsproblem und zur Lehre von den Temperamenten.*

By Ernst Kretschmer. Julius Springer, Berlin. RM. 13.60. $9\frac{3}{4} \times 6\frac{1}{2}$; x + 243; 1936.

This new edition of Kretschmer's epoch-making treatise on physique and temperament differs only slightly from the preceding ones. The exposition of the subject matter remains practically the same except for some additions. These include a summary of the principal studies which confirm his findings and a report of a few of the more recent investigations on the different physiological reactions observed in asthenics and pyknics. Of particular interest is the author's reply to those who have stated that the somatic types are really racial physical characteristics and that the temperaments associated with them are expressions of differences in racial psychology. Kretschmer presents data derived from an investigation on Japanese which show that the distribution of the three somatic types in the general population is markedly different from that found either in circular or schizophrenic individuals.

LEAVES FROM THE JUNGLE. *Life in a Gond Village.*

By Verrier Elwin. John Murray, London. 9s. net. $8\frac{1}{2} \times 5\frac{1}{2}$; 243 + 9 plates; 1936.

In the forests of Central India lives an aboriginal tribe, the Gonds, to whom Verrier Elwin and his helpers have tried for several years to bring relief from physical suffering and, to their children, "some of the elements of hygiene and healthy living." This book is an entertaining account, in diary form, of the author's experiences over a period of four years and includes an introductory chapter on the Gonds, their ways of life, their philosophy, and their problem of adjusting themselves to the rest of the world.

The text is illustrated with photographs, and at the end of the book there is a section of notes elaborating on some of the things referred to in the diary.

THE PHYSICAL ANTHROPOLOGY OF THE SEMINOLE INDIANS OF OKLAHOMA.

By Wilton M. Krogman. With an Introduction by Corrado Gini. Comitato Italiano per lo Studio dei Problemi della Popolazione, Rome. Lire 20. 10 x 7; xi + 199 + 39 plates; 1935 (paper).

For one summer in the field, Dr. Krogman and his party collected an extraordinary amount of information and apparently did it very thoroughly. Their major purpose at the outset was to investigate the racial effects of Indian-Negro crossing. For this purpose, as much information as possible was gathered about the family history and relationship between the individuals studied. Particular attention was given to skin color. The author does not feel that his data are sufficient to demonstrate the physical effects of the cross between Negro and Indian. However, there is evidence in this study that a large part of the Indian blood in Negro veins has come from Seminole and Creek Indians.

OFFICIAL YEAR BOOK OF THE COMMONWEALTH OF AUSTRALIA. *Number 28, 1935.*

Prepared by E. T. McPhee. Commonwealth Bureau of Census and Statistics, Canberra. 5s. 9 x 6; xxxi + 971; 1936.

This year book supplies data concerning the state of things in Australia in 1934, together with some historical material. Information is given on colonization, physiography, government, trade, education, public justice, public hygiene, vital statistics, labor, wages and prices, the utilization and preservation of natural resources, manufacturing and transport and communication. A chronological table of the chief events since the establishment of settlement in the Commonwealth, a list of the official statistical publications of Australia, a "select list of representative works dealing with Australia," and an index are provided.

DAS BEVÖLKERUNGSPROBLEM UND SEINE AUSWIRKUNG IN DER NEUEN DEUTSCHEN STEUERREFORM.

By Karl Barth. Hans Buske Verlag,

Leipzig. RM. 4.50. $9\frac{1}{4} \times 6$; 158; 1936 (paper).

This pamphlet gives a general account of the trends in the German population particularly in the last few years in respect to all the commonly noted variables: age and sex distribution, birth and death rates, emigration, etc. As might be expected, these statistics are fitted into a general discussion of population to prove both that Germany is overcrowded and needs more land and that it is the patriotic duty of every German to raise a large family.

ABRAHAM. *Recent Discoveries and Hebrew Origins.*

By Sir Leonard Woolley. Charles Scribner's Sons, New York. \$3.00. $7\frac{7}{8} \times 5\frac{1}{2}$; 299; 1936.

One of the most distinguished authorities on Mesopotamian archeology presents evidence that the biblical Abraham was an historical character, born in the metropolis Ur during the 20th Century B.C.—or, more probably, that he is now three personalities rolled into one. About a third of the book gives pure description of the ancient urban culture. The balance specializes on Abraham, although also rich in cultural data. The style is delightful. A short index and chronology is appended.

HIGH UP IN MEXICO.

By O. A. Merritt-Hawkes. Ivor Nicholson and Watson, London. 15s. net. $8\frac{3}{4} \times 5\frac{5}{8}$; xi + 279 + 17 illustrations; 1936.

A somewhat impressionistic picture of a wide-ranging variety of Mexican scenes and persons, brightly and smartly painted by an Englishwoman trained as a biologist. The book evidently has no very deep purpose beyond the laudable one of entertaining the reader. This it does. The intending visitor to Mexico will find it interesting and stimulating, particularly if he intends to concern himself at all with the sociological problems that present themselves under the present regime.

ZOÖLOGY

MÉLANGES PAUL PELSENEER. *Mémoires du Musée Royal d'Histoire Naturelle de Belgique, Deuxième Série, Fasc. 3.*

Musée Royal d'Histoire Naturelle de Belgique, Bruxelles. $13 \times 9\frac{3}{4}$; 1206; 1936 (paper).

This ponderous volume is a testimonial to one of the foremost living exponents of systematic zoology. Pelseneer is probably the last representative of that school of natural science typified by Lamarck in France, Swainson in England, and Rafinesque in the United States, within the scope of whose intellects was embraced the entire field of taxonomic zoology. By way of expressing their high esteem for his accomplishments, seventy-four of the world's leading biologists have contributed sixty-eight miscellaneous articles, drawn from the fields of zoology, botany, geology, genetics, and medicine.

The reviewer can mention only a few papers which seem of great interest to him personally in this miscellany of contributions. First among these is a *History of the Classification of the Mollusca*, by Aug. Lameere, of Brussels, which summarizes the opinions of all authorities of importance from Aristotle down to his own publication of a few years ago. Strangely enough, although this article is dedicated to Pelseneer by its author, no mention is made to Pelseneer's own contribution to molluscan taxonomy in Lankester's *Treatise on Zoology*, Vol. V. Neither does he mention the taxonomy of bivalves which Dall contributed to the last edition of Zittel's *Paleontology*. Lameere's most striking contribution to the classification of mollusca is the union of the so-called paleozoic pteropods discovered by Walcott in Canada, with the Bellerophonts.

Next in interest is probably the discussion of the Nudibranchiata by Nils Hj. Odhner of Stockholm, the longest article in the volume, and then one by Arnold Pictet of Geneva on the effect of geography

on genetics. The other contributions are equally meritorious but it is obviously impossible to do justice to all of them here.

STUDIES IN EXPERIMENTAL ZOOLOGY. (*Regeneration, Experimental Embryology, Endocrinology.*)

By A. Elizabeth Adams. A. Elizabeth Adams, Mt. Holyoke College, South Hadley, Mass. $1.25. $10\frac{3}{4} \times 8\frac{1}{2}$; v + 74; 1936.

LABORATORY STUDIES IN GENERAL ZOOLOGY.

By Ann H. Morgan. Ann H. Morgan, Mt. Holyoke College, South Hadley, Mass. $2.00. $10\frac{3}{4} \times 8\frac{1}{2}$; iii + 135; 1936.

It is a wonder that more laboratory manuals dealing with experimental phases of biology have not appeared. This is certainly an instructive and profitable field for the student as it not only teaches him technique, theory and scientific discipline but allows him to get a brief taste of scientific research as well. There is certainly no logical reason why college laboratories of zoölogy should ban living animals from their premises. In fact, it might be a good plan to show the student a live organism once in a while just for luck. The adoption of Doctor Adam's manual will not only require the presence of living creatures in the laboratory but will demand that the young zoölogist make certain manipulations with them. In this book the author has skillfully organized a group of classroom studies chosen from the fields of regeneration, experimental embryology and endocrinology. The following data are included under each individual exercise: (1) object of the experiment; (2) technique to be followed; (3) observations to be made and recorded, and (4) references to research investigations. As a result of original planning and ingenious treatment *Studies in Experimental Zoology* rises considerably above the run-of-mine manual and can be heartily recommended.

Laboratory Studies by Morgan, while more orthodox than the preceding book, is likewise a well-prepared guide for elementary students. The volume covers the phases of morphology, physiology and ecology commonly studied in college laboratories. Its chief attributes lie in a careful selection of material; an authoritative treatment and organization of that material; a clear style of presentation, and an excellent cross-reference system to other textbooks and papers. The reviewer feels the author has done all in her power to make the book as useful as possible for the student even to the extent of advising him to "be on time."

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society, Volume XXI, Numbers 12–18.*

New York Zoological Society, Zoological Park, New York. $1.75. $10\frac{1}{2} \times 7$; 96 + 22 plates; 1936 (paper).

The following papers are included in this issue of *Zoologica:*

1. The Morphology, Cytology and Life-history of *Oodinium ocellatum* Brown, a Dinoflagellate Parasite on Marine Fishes. By Ross F. Nigrelli.

2. The Winter Movements of the Land-locked Alewife, *Pomolobus pseudoharengus* (Wilson). By C. M. Breder, Jr. and R. F. Nigrelli. This study was prompted by complaints to the New York City Water Department of fishes emerging from household taps. Odell(1934) wrote of this same species but had no data on the whereabouts of the fish during the winter months, whereas this is the period best covered by the data secured from the Department of Water Supply.

3. Systematic Notes on Bermudian and West Indian Tunas of the Genera *Parathunnus* and *Neothunnus*. By William Beebe and John Tee-Van. Plates are included with 13 figures of specimens of these two species.

4. Food of the Bermuda and West Indian Tunas of the Genera *Parathunnus* and *Neothunnus*. By William Beebe. Many surprises of great interest were uncovered during the course of examination of the stomach contents of these two genera. New and rare species of fish and invertebrates were found, and some forms generally supposed to be a common item of diet—such as copepods—were completely missing.

5. Notes on the Biology and Ecology

of Giant Tuna, *Thunnus thynnus* Linnaeus, Observed at Portland, Maine. By Jocelyn Crane. Descriptions are based on 34 newly-caught tunas.

6. The Templeton Crocker Expedition. I. Six New Brachyuran Crabs from the Gulf of California. By Steve A. Glassell.

7. Neoplastic Diseases in Small Tropical Fishes. By G. M. Smith, C. W. Coates and L. C. Strong. A tendency toward neoplasia has been noted in only 5 species out of an approximate 400 under observation at the New York Aquarium during the last five years. These diseases are described, and a group of pigmented cutaneous tumors in the hybrids of the Mexican killifish are discussed from the genetic viewpoint.

CONTRIBUTIONS TO A SCIENCE OF NEMATOLOGY. *Numbers I to XXVI.*

By N. A. Cobb. (Obtainable from Mrs. F. C. Blanchard, Botanical Gardens, University of Michigan, Ann Arbor.) $5.35. 9⅝ x 6¾; 490; 1935.

Dr. N. A. Cobb was a remarkable man on many accounts. His was an adventurous, original and wide-ranging mind, in which orthodoxy (in the widest sense of the word and not merely the religious) was conspicuous by its absence. He was a non-conformist *pur sang*, and for that and other reasons a delightful person.

If any single part of his manifold interests could be separated from the rest and justly characterized as his life-work it would be his work over many years on nematodes, or, as he preferred to call them, "nemas." For more than a third of a century he ranked as one of the world's foremost authorities on this group. Long before the word "biometry" had come into general use, or any considerable amount of work in the modern sense had been done in that field, Dr. Cobb had invented and put into use a biometric scheme for the specific identification of nematodes. In its essentials it is today established as a foundation stone of nematode taxonomy.

This volume is Cobb's enduring monument as a nematologist. It consists of 25 papers of varying length which together at least touch upon where they do not exhaustively treat, virtually every aspect of nematode biology. Some of them are reprinted from journals in which they originally appeared. Others find their first publication in this volume. The whole expense of manufacturing the volume he and his family privately contributed at no small sacrifice, and with no other motivation than to contribute to the advancement of the science of nematology.

The volume constitutes a unique reference work. Zoological laboratories, libraries and individuals owe it as a duty to themselves, to their science, and to the memory of a great colleague to purchase and make more widely available this book.

THE TSETSE FLIES OF EAST AFRICA. *A First Study of Their Ecology, With a View to Their Control. Transactions of the Royal Entomological Society of London, Vol. 84.*

By C. F. M. Swynnerton. With a Preface by Right Hon. W. Ormsby-Gore. Royal Entomological Society of London, London. £5 10s. 10¼ x 6¾; xxxvi + 579 + 22 plates + 7 folding maps; 1936 (paper).

This is a complete report of the work done in the past few years by the Tsetse Research Department of Tanganyika together with their hopes and general plans for the future as well as an account of all that is known of this fly and its natural habitat. The effects of temperature, humidity and land conditions on their breeding, activity and migration is discussed in great detail. The main concern of the authors is to devise means of preventing the spread of the flies and reclaiming areas now occupied by them. At the same time they have to provide water and other necessities to make the land fit for settlers. A large part of the book is devoted to descriptions of various facts about Tanganyika territory, and in particular the need for larger appropriations from the government to carry on the good work is stressed. In such a practical, factual study we were pleasantly surprised to find a most interesting discussion of "man's right to destroy the

tsetse fly" in which this much maligned animal is viewed as a preserver of the natural beauty and resources of the country!

This is a contribution to the literature of ecology of the first rank of importance.

THE BIOLOGICAL CONTROL OF INSECTS. *With a Chapter on Weed Control.*

By Harvey L. Sweetman. Foreword by L. O. Howard. Comstock Publishing Co., Ithaca. $3.75. 9 x 6; xii + 461; 1936.

In essence, the problem of the control of one (or more) species by another is a population problem. It is only recently that the economic biologists, as a group, have appreciated and grasped this point. Stimulated and informed by the basic contributions of such workers as Volterra, Pearl and Lotka they are turning out regularly data that furnish both practical aid in the limitation of undesirable pests and contribute as well to the more theoretical aspects of biology generally.

The present book is a good example of this modern development. It presents, in a sane and well organized fashion, a wealth of practical material about the technique of insect control without losing sight of the underlying biological principles. The plan of the text is to consider by chapters the importance of various organisms in limiting the growth of injurious insect populations. Thus sections are devoted to microörganisms, parasitic lower invertebrates, insect parasites and predators and other forms that have value for the economic biologist. Each chapter attempts to outline and appraise the particular control method under discussion. Many studies are cited in the text; these are cross-referenced in the bibliography. The book is illustrated and contains an index and glossary.

Миграции Рыб.

П. Ю. Шмидт. Государственное издательство биологической и медичинской литературы, Москва-Ленинград. 4 р. 95 коп; 327; 1936.

[FISH MIGRATIONS.

By P. J. Schmidt. State Publishing House for Biological and Medical Literature, Moscow-Leningrad. 4.95 roubles. 5 x 8 inches; 327, 1936 (cloth)].

The study of fish migrations presents an attractive problem for biologists interested in evolutionary problems, primarily because the migration instinct and ways seem to be significantly conditioned not only by the present environmental situation, but also by those that have existed for millions of years past. But undoubtedly the chief reason for the variety and wealth of observations at present accumulated lies in their practical importance for the fishery industry. In the present volume Professor Schmidt treats the problem in detail, and discusses some important and hitherto unpublished data on the migration of herring, plaice and various salmons in the Far East, where he has been in charge of investigations for several years. There are five chapters: Migrations of pelagic, benthic, passing and freshwater fishes; and Environment and migrations. This is the book of an expert on the subject.

BIRD MIGRATION. *A Short Account.*

By A. Landsborough Thomson. H. F. and G. Witherby, London. 5s. net. $6\frac{7}{8}$ x $4\frac{1}{2}$; 224 + 6 plates; 1936.

Within the last twenty years great progress has been made in unravelling the riddle of bird migration. Studies have changed in character from cursory observations on bird movements to the systematic recording and mapping of migratory routes. In addition, the problem has recently been attacked experimentally with highly suggestive results. This general activity has accrued a wealth of technical data not always readily available to the naturalist and bird-lover. To remedy this situation Doctor Thomson has written the present book ". . . in response to a request for a short account of bird migration, in simple terms." The text is divided into four general sections and covers the historical, descriptive and dynamic aspects of the subject. The latter discussion is of great interest in that it attempts to picture the

relative rôle of ecological, physiological and psychological factors in controlling and initiating bird migration. It is becoming more and more obvious that only by a rounded study of all these factors can the true explanation of migration be reached.

This is a highly readable book. It has the attribute of simplicity without loss of precision and epitomizes a number of diverse facts. There is an annotated bibliography, an index and a number of text illustrations.

Mode of Life, Feeding, Digestion and Symbiosis with Zooxanthellae in the Tridacnidae. *Great Barrier Reef Expedition 1928–1929 Scientific Reports, Volume I, Number 11.*

By C. M. Yonge. British Museum (Natural History), London. 5s. $12\frac{1}{4} \times 9\frac{1}{8}$; 39 + 5 plates; 1936 (paper).

The giant clams *Tridacnidae* may be divided into two groups according to the mode of life. .One group contains surface living species, among which is the form that attains greatest size, *T. deraso*, $4\frac{1}{2}$ ft. long; the other group contains the boring species *T. crocea*, and *T. fossor* that dig into coral boulders. One of the unique characteristics of this genus *Tridacna* is the unusual presence of the algae Zooxanthellae—in the tissues. The Tridacnidae in fact "farm" the algae in large quantities in their extremely enlarged mantles. The inner surface of the dorsal mantle edge contains lens like structures (hyaline organs) the purpose of which seems to be to effect internal illumination of the mantle tissues for the benefit of the Zooxanthellae, which are to be consumed in the course of time. *Tridacna* is probably the best example of the exploitation of associated algae by an animal. The Zooxanthellae are believed by the author to have caused the clams to evolve their gigantic size, since once established in the siphonal region it became advantageous to the *Tridacna* to extend the mantle tissues. This involved the twisting around of the mantle and displacement of the umbo and hinge, and the disappearance of other organs. The mantles were then free to develop enormously. A valuable contribution.

Big Game Hunting in Manchuria.

By N. Baikov. Adapted from the Russian by Serge Ivanoff and Gertrude Mack. Hutchinson and Co., London. 18s. net. 9 x 6; 285 + 17 plates; 1936.

A delightful series of sketches in which the author, a Russian trapper, describes his adventures in the Manchurian forests. We learn that big game hunting by no means provides the only thrills of the Manchurian wilds. Besides his adventures with Great Van, the tiger, and other forest animals some of the author's most thrilling and gruesome tales have been of the bloody deeds of the Khunghuz, the forest bandits who are more to be feared and more to be respected than all the wild life put together.

Blood and thunder and excitement, however, are far from the only recommendations for this entertaining volume. Simply and artistically the author revives for us the spirit and laws of the great Taiga and its many and varied inhabitants. The hunters, the trappers, the Khunghuzs, and the Varnaks who fled into the Russian wilds from Siberian imprisonment become in this enthralling volume living people with fascinating stories. Baikov writes too of forest kidnappings; vividly he describes the "Snakes' Grand-Dad" and this old hermit's mysterious powers over the deadly serpents. Ginseng seekers are realistically portrayed as they wander aimlessly and unarmed through the dangerous country in their religious and unending search for the famous ginseng—the Root of Life.

Minnows of Michigan. *Bulletin No. 8.*

By Carl L. Hubbs and Gerald P. Cooper. Cranbrook Institute of Science, Bloomfield Hills, Mich. 50 cents. 9 x 6; 95 + 10 plates; 1936 (paper).

The authors emphasize the point that a true minnow is not just any small fish, but only those belonging to the family Cyprinidae. True minnows, and there are over 2000 species of them, are found

throughout the north temperate zone over North America, Europe and Asia. With the exception of one Japanese species they are only found in fresh water. Economically they are of great importance as they constitute a considerable part of the food for the important game fishes. The authors describe the ten most important and abundant varieties of the Michigan minnow giving detailed accounts of their breeding, spawning and feeding habits. The bulletin is illustrated by excellent photographs. It has an index and a good sized bibliography.

THE HABITAT AND FOOD OF THE CALIFORNIA SEA MUSSEL. *Bulletin of the Scripps Institution of Oceanography of the University of California, Technical Series, Volume 4, No. 1.*

Edited by Denis L. Fox. University of California Press, Berkeley. 75 cents. $10\frac{1}{4}$ x $6\frac{3}{4}$; 64; 1936.

This work, by four coöperating authors, is a complete investigation into the life habits of a well known mollusk. Beginning with a discussion of the literature covering related forms, there follow accounts of original experiments on the degree of salinity tolerated by the California Mussel, the anatomy of its digestive tract, the nature of its food and the method of obtaining it, and chemical analysis of the digestive enzymes and of the faecal material. Not the least interesting feature of the investigation is that of the rate at which water is circulated through the siphons and over the gills. All told, this series of experiments is an excellent example of what a scientific investigation ought to be.

There is a table of contents, together with lists of tables and figures, but no general index.

RESULTS FROM BREEDING RABBITS THAT ARE SUCKLING YOUNG. *U. S. Department of Agriculture, Circular No. 410.*

By Charles E. Kellogg. Government Printing Office, Washington. 5 cents. 9 x $5\frac{3}{4}$; 8; 1936 (paper).

This study was undertaken to determine how intensively rabbits can be bred without detriment to the stock. While still suckling young from a previous litter—the young being weaned in this experiment at 56 days—one group of 40 does was mated 28 days after each kindling, a second group at 42 days, and a third group at 56 days. Records were kept as to number of young in litter weaned and average weight at weaning, service acceptance by the doe and the result of the mating. From the results of this experiment, of one year's duration, the author deems a 28 day breeding schedule inadvisable, a 42 day schedule as possibly advantageous during the period of the natural mating season, but in general the 56 day schedule as the best.

THE MAMMALS AND LIFE ZONES OF OREGON. *North American Fauna, No. 55. U. S. Department of Agriculture, Bureau of Biological Survey.*

By Vernon Bailey. U. S. Government Printing Office, Washington. 75 cents. $9\frac{1}{4}$ x $5\frac{3}{4}$; 416 + 52 plates + 1 folding map; 1936 (paper).

There has been a decided trend in recent years towards a greater interest in the conservation of the wild life of this country. This publication is another evidence of that trend. An attempt is made not only to list the species of this region, but to give information concerning their physical characters, distribution, abundance, habits and economic status, as a means for better understanding, appreciation, management and control of the fauna. The annotated list of species is preceded by brief descriptions of the physiographic features of the state and of the life zones, and by tables listing the mammals, reptiles, breeding birds and the plants of Oregon, by life zones. A bibliography, glossary of Indian names of mammals, and an index are included.

DEEP-SEA FISHES OF THE BERMUDA OCEANOGRAPHIC EXPEDITIONS. *No. 3—Family Serrivomeridae. Zoologica, Volume XX. Number 3.*

By William Beebe and Jocelyn Crane. New York Zoological Society, New York. 50 cents. $9\frac{1}{2} \times 6\frac{1}{4}$; 50; 1936 (paper).

This paper on the genus *Serrivomer* is based on a study of the type specimens in museums and the eels captured by the Bermuda Oceanographic Expeditions. There are three distinct species: *S. beanii* and *S. brevidentatus* from the Atlantic, and *S. sector* from the Pacific and Indian Oceans. The principal difference between the species are in dental and skeletal structures. Detailed descriptions are given of *S. beanii* and *S. brevidentatus*, including adult developmental growth stages, and of their ecology.

THE ORIGIN OF HIGHER CATEGORIES IN CYNIPS. *Indiana University Publications, Science Series No. 4.*

By Alfred C. Kinsey. Indiana University Bookstore, Bloomington, Ind. \$2.50. 10×7; 334; 1936 (paper).

This is a systematic study of seventy species of gall wasps new to the genus *Cynips*. Most of the new species are from Mexico and this study includes all that is known of the Mexican and Guatemalan representatives of the group. New data have also been added on some of the species included in the author's 1930 monograph. "Higher categories" refer to sections inclusive of smaller sections (the lower categories) in a chain of species. Illustrated with pen and ink drawings and photographs, the monograph has an alphabetical index and complete bibliographies.

HOW TO KNOW THE INSECTS. *An Illustrated Key to the More Common Families of Insects, with Suggestions for Collecting, Mounting and Studying Them. Biological Survey Publication No. 1, Iowa Academy of Science.*

By H. E. Jacques. H. E. Jacques, 709 N. Main St., Mt. Pleasant, Iowa. \$1.00. $8\frac{1}{4} \times 5\frac{1}{2}$; 140; 1936 (paper).

The enthusiastic young entomologist should find this a most helpful guide in his attempt to build up a good insect collection. Clear directions for collecting and mounting insects are given and the key to the orders and principal families is made quite simple. Drawings illustrate the typical external structures which are used in classification, and there are about 200 pen and ink sketches of representative species. The guide has been prepared with special reference to the insects of Iowa but its usefulness is by no means limited to this region.

SUCHE, AUFNAHME UND ENZYMATISCHE SPALTUNG DER NAHRUNG DURCH DIE WELLHORNSCHNECKE BUCCINUM UNDATUM L. *(Grundlegung einer ganzheitlichen Deutung der Vorgänge im Beute- und Verdauungsfeld.) Zoologica, Heft 92.*

By Friedrich Brock. E. Schweizerbart'sche Verlagsbuchhandlung, Stuttgart. RM. 46. $12\frac{1}{2} \times 9\frac{3}{4}$; 136 + 1 plate; 1936 (paper).

The author discusses habits of feeding and problems of digestion in the marine snail *Buccinum undatum* L. There is a description of the anatomy of the snail, and the organs used for capturing and assimilating its food. The glands of the stomach and intestine were studied to determine what part they played in digestion. It was found that the glands of the upper intestine were not actual digestive glands though to some extent they break up connective tissue and fat. However neither olive oil, nor soluble starch nor saccharose were effected by these glands. This snail is a carrion eating animal.

LITTLE BLACK ANT.

By Alice Gall and Fleming Crew. Oxford University Press, New York. \$1.50. $9 \times 6\frac{1}{2}$; 128; 1936.

This book describes life in an ant hill. It tells how the black ants secure food from the flowers and from their "honey cows"; of their wars with the red ants who seek to enslave them; and of their industry and coöperation.

It is written with a charming simplicity which should appeal to children and is delightfully illustrated.

Do You Know about Fishes?

By Janet Smalley. William Morrow and Co., New York. $1.25. $5\frac{3}{4} \times 9$; 45; 1936.

This little book for very small children is really good, in respect of both illustrations—which mainly fill it—and text. What it tells youngsters about a baker's dozen sorts of fish is sound, both in its implications as well as direct statements. Its zoology is vouched for by the Philadelphia Academy of Natural Sciences.

The Genera of Parasitic Wasps of the Braconid Subfamily Euphorinae, with a Review of the Nearctic Species. *U. S. Department of Agriculture Miscellaneous Publication No. 241.*

By C. F. W. Muesebeck. U. S. Government Printing Office, Washington. 5 cents. $9\frac{1}{4} \times 5\frac{7}{8}$; 38; 1936 (paper).

Detailed descriptions of the morphological characteristics of 14 genera are given as well as notes on geographical distribution, host-parasite relationships, etc. A key to the genera of Euphorinae is included. There is an index and a one page bibliography.

Three New Species of Fridericia (Enchytraeidae) from California. *University of California Publications in Zoology, Volume 41, Number 12.*

By A. W. Bell. University of California Press, Berkeley. 35 cents. $10\frac{1}{4} \times 6\frac{3}{4}$; 20; 1936 (paper).

Trypanosoma neotomae, sp. nov., in the Dusky-Footed Wood Rat and the Wood Rat Flea. *University of California Publications in Zoology, Volume 41, Number 11.*

By Fae Donat Wood. University of California Press, Berkeley. 25 cents. $10\frac{1}{4} \times 6\frac{3}{4}$; 12; 1936 (paper).

California Isopods of the Genus Porcellio with Descriptions of a New Species and a New Subspecies. *University of California Publications in Zoology, Vol. 41, No. 13.*

By Milton A. Miller. University of California Press, Berkeley. 25 cents. $10\frac{1}{4} \times 6\frac{3}{4}$; 8; 1936 (paper).

A Key to the Rattlesnakes with Summary of Characteristics. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 20.*

By Laurence M. Klauber. Society of Natural History, San Diego, Calif. $10\frac{1}{8} \times 6\frac{7}{8}$; 92 + 1 folding table; 1936 (paper).

New Porcellanids and Pinnotherids from Tropical North American Waters. *Transactions of the San Diego Society of Natural History, Vol. 8, No. 21.*

By Steve A. Glassell. Society of Natural History, San Diego, Calif. $10\frac{1}{8} \times 6\frac{7}{8}$; 28; 1936 (paper).

West American Species of the Genus Phos. *Transaction of the San Diego Society of Natural History, Vol. 8, No. 22.*

By A. M. Strong and H. N. Lowe. Society of Natural History, San Diego, Calif. $10\frac{1}{8} \times 6\frac{7}{8}$; 16; 1936 (paper).

BOTANY

Our Friends the Trees.

By P. G. Cross. E. P. Dutton and Co., New York. $5.00. $9\frac{1}{4} \times 6\frac{1}{4}$; 334 + 31 illustrations; 1936.

Here is a book about trees by a man who loves them and wishes others to enjoy with him some of the happiness which they have brought him. It is essentially a practical book, with much information as to the best kind of tree for different situations, as to how to transplant different kinds of trees, and how to care for them afterwards. All of this is most helpful to any one who has just built a home and is about to plant the grounds, but probably the reader who takes seriously the recommendation of the Ben Davis apple for eating will be disappointed.

The author displays a great deal of righteous indignation against the wanton slaughter of our forest trees, by those whose brains are in their pocket books. In this he seems to be amply justified. The lumber industry is of course a legitimate one, but it needs intelligent supervision before it is too late. Already the tallest Douglas fir in Canada, and the

tallest eucalyptus in Australia have fallen victims to the bandsaw in the name of progress—a fate which the coast redwoods have only narrowly escaped. Not only does the destruction of trees such as these constitute a calamity from the aesthetic standpoint, but the unsupervised deforestation of hill and valley opens the way for the rainfall to erode surface soil with annual floods, and so become a curse instead of a blessing.

This is not strictly a scientific book—note the use of the popular terms Douglas fir for a spruce tree (which is not a fir) and red cedar for a juniper (which is not a cedar) but in general it is scientifically accurate. All kinds of trees are discussed—those which bear fruit, those which bear nuts, those which are evergreen, and those whose foliage turns orange, purple, and scarlet in the fall. There is even a chapter devoted to the palms. In fact, the only tree whose absence makes it conspicuous is the gingko. This is surprising since it is a fairly rapidly growing tree, and harbors no parasites, which makes it ideal for anyone who wants trees about his home but who cannot devote all his time to caring for them.

The index covers eight pages. The illustrations are good, and each chapter is headed by a poem, the inevitable one of Joyce Kilmer being among them.

Теоретические Основы Яровиэации. Т. Д. Лысенко. Государственное издательство колхозной и совхознои литературы, Москва-Ленинград. 1р. 95 коп; 152; 1935.

[THEORETICAL BASIS OF VERNALIZATION. *By T. D. Lysenko. State Publishing House for Agricultural Literature, Moscow–Leningrad.* 1.95 roubles; 152; 1935 (cloth)].

In this excellent and important book Professor Lysenko describes his discovery and extensive investigations on the phenomenon of vernalization, namely, the acceleration of ear formation in winter varieties of cereals and in various other plants by exposing the germinating seeds to low temperature. The theoretical conception lying at the bottom of these investigations is that the processes of differentiation are essentially independent of growth, and that there are in differentiation definite stages or phases requiring particular sets of environmental conditions. Ear formation in winter varieties of cereals is impossible if a specific 'vernalization phase' proceeding under low temperatures is not completed. Artificially prolonged exposure of germinating seeds to cold in the spring can substitute for the natural course of events in the winter-fields. An outstanding feature of this line of investigation consists undoubtedly in the fact that laboratory experiments were very soon followed by extensive agricultural use of this valuable method for selection and various other practical purposes of plant breeding.

COFFEE. *The Epic of a Commodity.* *By Heinrich E. Jacob. Translated by Eden and Cedar Paul. The Viking Press, New York.* $3.50. $9\frac{1}{4} \times 6\frac{1}{4}$; xiii + 296 + 38 plates; 1935.

This book tells the history of coffee and its social and economic effects on civilization. The legend of its discovery in Arabia and its subsequent spread among the Mohammedans is given. "Coffee has sometimes been spoken of as the 'wine of Islam'; and, in actual fact, Mohammedan civilization, the Moslem love for drawing fine distinctions, for hair-splitting, for disputation—all the 'cold heat and flaming sobriety' of Arabic civilization, are closely connected with the effect of coffee upon the human brain." In contrast to this is the Bacchic culture of the classical period and of Christianity. The discussion of the psychological antithesis between coffee and wine as typifying different civilizations is extremely well done.

From the seventeenth century on, the use of coffee spread rapidly. It was brought into Europe by wars and commerce and moved on to other parts of the world during the period of the great colonial expansion. It affected the social and political life of most of western Europe through the establishment of coffee houses. And it partly took the place of beer which had been consumed in enormous quantities in England and Germany

in the Middle Ages. Finally the part Brazil plays as the producer of over ninety per cent of the world's supply of coffee today is discussed.

This book is extremely well written giving a sketch of world history for the past three centuries while telling the story of the production and consumption of coffee. As is often the case the author has perhaps become over enthusiastic on his subject. He has a brilliant imagination and his interpretation of the changes in the social order due solely to coffee is doubtless exaggerated but always interesting. There are many beautiful illustrations, a bibliography, and an index.

THE WESTERN RANGE. *Letter from the Secretary of Agriculture Transmitting in Response to Senate Resolution No. 289 a Report on the Western Range—a Great but Neglected Natural Resource. Senate Document No. 199, 74th Congress, 2d Session.*

U. S. Government Printing Office, Washington. $9\frac{1}{4} \times 5\frac{3}{4}$; xvi + 620; 1936 (paper).

It is hoped that this treatise will aid in arousing public interest in the problem of conservation of the resources of the large western range which comprises about 40 per cent of the total land area of the continental United States. Depletion of the range appears to have been rapid, largely due to excessive stocking and consequent over-grazing. Erosion of the land due to misguided agricultural projects is another major source of depletion. It is estimated that not more than 5 per cent of the entire range area is now in a thoroughly satisfactory condition. The object of this publication has been to present "an all-inclusive survey of the range resource, its original and present condition, the causes and effects of changes, the social and economic function which it does and should render to the West and to the Nation, and, finally, to outline practical solutions for at least the more important problems." The field has been well covered in this excellent report. An index and extensive bibliography are included.

FERNS OF NORTHEASTERN UNITED STATES. *Illustrations and Descriptions of All Known Species in the New England and Middle Atlantic States. A Pocket Manual for the Amateur with a New Idea in Aids for Fern Identification.*

By Farida A. Wiley. The American Museum of Natural History, New York. \$1.00. $3\frac{3}{4} \times 6\frac{1}{4}$; 98; 1936.

This pocket manual contains drawings and descriptions of over 200 ferns. The lower pinnae of all the larger ferns are shown in life size, while the drawings of the entire fronds of large forms have been necessarily reduced. The scale of reduction is indicated in feet on the margin, or noted in the text. The small fronds are shown in life size. Except for four of the rare ferns, the drawings have all been made from specimens collected by the author and include all the known species found in New England and the middle Atlantic states. By the novel, simple, and clear methods developed in this work even the inexperienced should easily be able to identify these ferns. The manual contains an alphabetical glossary and index. There is no bibliography but the author gives credit in her foreword to the few references she has used.

THE COMPOSITION AND DYNAMICS OF A BEECH-MAPLE CLIMAX COMMUNITY. *Scientific Publications of the Cleveland Museum of Natural History, Volume VI.*

By Arthur B. Williams. Cleveland Museum of Natural History, Cleveland. 50 cents. $9\frac{1}{4} \times 6\frac{1}{4}$; 92; 1936 (paper).

The modern student of ecology is convinced that it is practically impossible to make a proper community analysis unless both plants and animals are taken into consideration. Despite this growing opinion it is unusual to find a study that emphasizes the complete biota of any particular community. The present report is a step in this direction for the author has taken a 65 acre tract of beech-maple-hemlock forest and made careful surveys of its animal and plant populations as well as measurements of common environmental variables. In some of the

surveys the organisms have been individually counted and this quantitative emphasis is an important feature of the book. A number of interesting facts are reported. These are concerned largely with (1) the description of the interrelations existing between specific plants and animals of the community, and (2) the evaluation of climatic factors in shaping the growth of the community as a whole. The book has a moderately extensive bibliography and contains a number of graphs.

THE VEGETABLE GARDENER'S HOW BOOK.
By Chesla C. Sherlock. The Macmillan Co., New York. $3.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; xix + 285 + 28 plates; 1936.

This book contains fifty-seven chapters on gardening, each beginning with appropriate quotations by famous authors from St. Luke down to Sherlock's own abstracts from the *Ladies Home Journal.* It completely covers vegetable gardening, fruit and small fruit culture. The author gives simple and practical information on how to plant, culture, fertilize and harvest twenty-seven cooking vegetables, fifteen varieties for the salad garden, and fifteen kinds of fruit both for the orchard and the berry patch. Numerous photographs illustrate this complete volume and an appendix includes fertilizer, pruning, and spraying charts.

TEMPERATURE STUDIES OF SOME TOMATO PATHOGENS. *United States Department of Agriculture, Technical Bulletin No. 520.*
By Alice A. Nightingale and G. B. Ramsey. U. S. Government Printing Office, Washington. 5 cents. 9 x $5\frac{3}{4}$; 36; 1936 (paper).

Data are presented in this paper to show the influences of temperature and maturity of fruit upon the growth and development of some fungi pathogenic to tomatoes. Nine organisms were selected for study, and records kept of the growth made on dextrose-potato agar plates held at 5° intervals of temperature between 30° and 95°F., and on tomatoes of varying degrees of ripeness at temperatures ranging between 32° and 85°F. Temperature, the pH of the agar and ripeness of the fruit were all found to be important factors in influencing the development of the pathogens.

IDENTIFICATION, HISTORY, AND DISTRIBUTION OF COMMON SORGHUM VARIETIES. *U. S. Department of Agriculture, Technical Bulletin No. 506.*
By H. N. Vinall, J. C. Stephens and J. H. Martin. Government Printing Office, Washington. 25 cents. $9\frac{1}{4}$ x $5\frac{3}{4}$; 102 + 59 plates; 1936 (paper).

This report was prepared for the purpose of clearing away the confusion surrounding the identification of the many varieties of sorghum. This has been accomplished by giving (1) a description of the commercial varieties, (2) a key to assist in the identification of the varieties, (3) the estimated acreage and distribution of the important varieties, and (4) the known history of all varieties described. There are also included full page reproductions to scale of panicle, spikelets and kernels of about 60 recognized varieties. There is a bibliography.

BOTANISCHES VADEMECUM FÜR BILDENDE KÜNSTLER UND KUNSTGEWERBLER.
By G. Haberlandt. Gustav Fischer, Jena. Rmk. 3.50 (paper); Rmk. 4.50 (cloth). $9\frac{3}{4}$ x $6\frac{1}{2}$; viii + 80; 1936.

This little pamphlet is intended as a guide for artists, both professional and commercial, who need, according to the author, to know something of the anatomy of plants. Too many bad pictures have been drawn of flowers and there is a great need for artists to know something about their anatomy just as they must know something of the anatomy of the human figure. The author discusses the anatomy of the thallophytes, mosses and ferns, and the flowering plants. There are many excellent illustrations. This book will also be of use as a guide to the amateur botanist.

THE TROPICAL GARDEN. *Its Design, Horticulture and Plant Materials.*

By Loraine E. Kuck and Richard C. Tongg. The Macmillan Co., New York. $3.00. $8\frac{1}{2}$ x $5\frac{1}{2}$; x + 378 + 16 plates; 1936.

To those interested in gardens and gardening in the tropics this book is of the utmost value. It not only gives thorough information and practical advice concerning the plants suitable for tropical gardens, but also has chapters on design, plant culture, greenhouses, and horticulture in general. Color and blooming charts are included.

TREES AND SHRUBS OF KENYA COLONY. *A Revision and Enlargement of a Descriptive Catalogue of Some of the Common Trees and Woody Plants of Kenya Colony.*

By E. Battiscombe. The Government Printer, Nairobi. 5s. $9\frac{3}{4}$ x $6\frac{1}{4}$; xi + 201; 1936.

This book is purely a descriptive catalogue of the common trees and woody plants of Kenya. The families follow the classification system proposed by Hutchinson in his book *The Families of Flowering Plants.* The main part of the book, pages 1–173, comprises the descriptive list of the various species of plants. There are included an index of botanical names, index of vernacular names, and an index of English names.

SILVA FENNICA 37. *Lehdoista ja Lehtokasvien Leviämisestä Pohjois-Pirkkalan-Tyrvään Alueella. [Über Haine und Verbreitung der Hainpflanzen im Gebiet von Nord-Pirkkala-Tryvää.]*

By Taimi Mäkelä. Society of Forestry in Suomi, Helsinki. $9\frac{5}{8}$ x $6\frac{1}{2}$; 61 + 1 plate; 1936 (paper).

SILVA FENNICA 38. *Hankikylvöt Tuomarniemen Hoitoalueessa VV. 1913–1930. [Die 1913–1930 ausgeführten Schneesaaten im Rivier Tuomarniemi.]*

By L. E. T. Borg. Society of Forestry in Suomi, Helsinki. $9\frac{5}{8}$ x $6\frac{1}{2}$; 136; 1936 (paper).

ACTA FORESTALIA FENNICA 42. *Publications of the Society of Forestry in Suomi. Helsinki.* $9\frac{5}{8}$ x $6\frac{1}{2}$; vii + 589 + 5 plates; 1936 (paper).

MORPHOLOGY

VERGLEICHENDE ENTWICKLUNGSGESCHICHTE DER TIERE. *In two Volumes.*

By Korschelt and Heider. Revised by E. Korschelt. Gustav Fischer, Jena. Two volumes Rmk. 52 (paper); Rmk. 56 (cloth). $10\frac{1}{4}$ x 7 inches; xx + 1314; 1936.

The first edition of this classic appeared nearly half a century ago, at a time when the science of invertebrate embryology was so young that it was possible to cover all of it in a book no bulkier than the present work. Since then the increase in knowledge of this subject has been so great that a modern edition constructed on the same general plan as the original would require at least half a dozen volumes, and many more than two collaborating authors. The reviser has had to select his material, and in doing so he has exercised the same good judgment as those familiar with the earlier editions have a right to expect from him.

The new edition is characterized by the same admirable illustrations, the same exhaustive bibliographies and references, together with many new ones, the same lucid exposition of embryonic phenomena augmented by summaries of and conclusions drawn from the works of all investigators of importance in this field in the twentieth century. Also the number of groups of uncertain phylogenetic position has increased, indicating perhaps that we do not know as much about evolution as we used to think we did. An innovation is the addition of a chapter devoted to the vertebrates, previous editions having been confined exclusively to invertebrate embryology.

The comprehensive index covers seventeen pages. The typography is excellent but the binding is very poor. Anyone contemplating the purchase of this book would do well to obtain an unbound copy.

Bailey's Text-Book of Histology (Elwyn and Strong). *Ninth Edition.*

Revised and rewritten by P. E. Smith, R. L. Carpenter, C. M. Goss, W. M. Copenhaver and A. E. Severinghaus. William Wood, Baltimore. $6.00. $9 \times 5\frac{3}{4}$; xvi + 773; 1936.

In revising the ninth edition of this well-known textbook the emphasis has been placed on dropping material out of date; adding new data; expanding passages to make them more inclusive, and generally clarifying the exposition. Certain figures have been discarded and many new illustrations added. These various revisions have been undertaken, as a coöperative enterprise, by five members of the Anatomy Department of Columbia University who have attempted ". . . to present, fairly, the major controversial differences of opinion" and to exclude ". . . a considerable body of facts which should rightly have been included if this were a source book for teachers and research workers rather than a textbook for students." The volume is logically organized for classroom use and possesses a good index. The figures are of first-rate calibre.

Cytologie du Liquide Céphalo-Rachidien Normal chez l'Homme. *Monographie Critique et Pratique.*

By H. Jessen. Masson et Cie, Paris. 40 francs. $10 \times 6\frac{1}{2}$; 168; 1936 (paper).

Although the cerebro-spinal fluid has been the subject of much study little definite and conclusive information has been obtained concerning the normal morphology and number of the cells. This book is devoted to a critical exposition of the various methods which have been employed by others, and a detailed presentation of the author's own researches on the subject. The author formally disclaims any intention of presenting his results as conclusive, or as serving as anything more than a point of orientation. Nevertheless, this is an important contribution. A bibliography covers twenty pages.

Das Muskelspiel des Menschen.

By Hermann Hoepke. Gustav Fischer, Jena. Rmk. 5.50. $9\frac{7}{8} \times 6\frac{1}{2}$; viii + 83; 1936 (paper).

An elementary description of man's muscular system and its functions, with a minimum of references to bones and practically none to other organs, is here presented. It is not a textbook or guide, but would be of value to non-laboratory students. The fifty illustrations are mostly excellent diagrams of selected muscles on a skeletal background to show origins, insertions, general positions and functions; the balance are photographs of nudes straining certain muscles for special demonstration.

PHYSIOLOGY AND PATHOLOGY

The Problem of Nutrition. *Volume I. Interim Report of the Mixed Committee on the Problem of Nutrition. Series of League of Nations Publications II. Economic and Financial 1936. II. B. 3.*

League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York. 50 cents. $9\frac{1}{2} \times 6\frac{1}{4}$; 98; 1936 (paper).

The Problem of Nutrition. *Volume II. Report on the Physiological Bases of Nutrition drawn up by the Technical Commission of the Health Committees at the meeting held in London (November 25th–29th, 1935), revised and amplified at the meeting held at Geneva (June 4th–8th, 1936). Series of League of Nations Publications II. Economic and Financial 1936. II. B. 4.*

League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York. 15 cents. $9\frac{1}{2} \times 6\frac{1}{4}$; 27; 1936 (paper).

The Problem of Nutrition. *Volume III. Nutrition in Various Countries. Series of League of Nations Publications II. Economic and Financial 1936. II. B. 5.*

League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York. $1.40. $9\frac{1}{4} \times 6\frac{1}{4}$; 271; 1936 (paper).

The Problem of Nutrition. *Volume IV. Statistics of Food Production, Consumption and Prices. Documentation Prepared by the International Institute of Agriculture. Presented*

to the Mixed Committee on the Problem of Nutrition at Its Second Session, June 4th, 1936. Series of League of Nations Publications II. Economic and Financial 1936. II. B. 6.

League of Nations, Geneva. World Peace Foundation, 8 West 40th St., New York. 75 cents. $10\frac{1}{2}$ x 8; 110; 1936 (paper).

In 1935 the Health Organization of the League of Nations set up a Mixed Committee on the Problem of Nutrition to make a detailed investigation into the agricultural, economic and health aspects of nutrition.

Vol. I in the series of reports presented by this committee is essentially an analysis of the problem, and an outline of the principles of nutrition and their application through which it is to be hoped national health may be improved. Volume II gives data on the average food requirements (calory, protein, fat, mineral and vitamin) for males and females of different age groups, and for special cases such as heavy labor and pregnancy. Volume III presents a survey of the measures taken in about 20 different countries to bring about an improvement in the nutrition in the various sections of the population. The following chapters are included under this heading: (1) Measures taken on behalf of mothers and infants; (2) measures taken on behalf of children of school age and young people; (3) measures taken on behalf of adults with special reference to unemployed adults, (4) army and navy dietaries, (5) measures to enable particular categories of consumers to obtain foodstuffs at reduced prices, (6) measures for ensuring the quality of foodstuffs, (7) research, education and popular instruction. Volume IV is submitted as a provisional document. Statistics on production, consumption, and prices of the protective and other foodstuffs are summarized in tables for each of about 20 different countries. The statistics on production and consumption are largely estimates and are accompanied by comments as to their scope and significance.

THE PLACE OF PSYCHOLOGY IN THE MEDICAL CURRICULUM *and other Papers. Individual Psychology Medical Pamphlets No. 16.*

By Sir Walter Langdon-Brown, R. G. Macdonald Ladell, Frank Gray and F. G. Crookshank. C. W. Daniel Co., London. 2s. 6d. net. $8\frac{1}{2}$ x $5\frac{1}{2}$; 56; 1936 (paper).

The four papers included in this volume have been written by doctors who believe that psychology is essential in the practise of medicine. Sir Walter Langdon-Brown, in his paper on the place of psychology in the medical curriculum, stresses this point and advocates some education in this field for medical students. Dr. Ladell, in a paper entitled medical psychology: pre-war, war-time and post-war, stresses the importance of psychology in diagnosing and treating diseases. Citing instances from his own career, he shows how post-war methods are much more accurate and effective than were pre-war methods and how the war was instrumental in awakening doctors to the reality of functional diseases. He concludes that since no distinct line can be drawn between functional and organic diseases, the medical man as well as the psychiatrist and the psycho-analyst should have some understanding of psychological methods. Dr. Gray discusses the psycho-pathology of organic disease and after citing a number of cases arrives at the rather surprising conclusion that "every illness is the neurotic solution of an internal conflict." Dr. Crookshank's paper deals with Adler's theory of organ inferiorities. It discusses the effects of organ inferiorities on the development of character, personality, and diseases (functional and organic) and concludes, very much as Dr. Gray does, that "the way to right living and avoidance of disease . . . is in the control of the psychical life."

MOTOR PERFORMANCE OF THE DEAF. *Comparative Psychology Monographs, Vol. 13, No. 6, Serial No. 66.*

By Joseph E. Morsh. Johns Hopkins Press, Baltimore. \$1.00. 10 x $6\frac{3}{4}$; 51; 1936 (paper).

About 150 children with auditory defects ranging from 25 per cent to complete hearing loss were given tests of ability in motor performance. These included a tapping test with the Dunlap double tapping plate, a static hand control or steadiness test with Dunlap's apparatus for the purpose, a test for visual memory and location memory devised by the author, and Johnson's speed of eye movement and hand-eye coördination tests. Except for this last test, the performance of the subjects with impaired hearing was compared to that of an approximately equal number of children with good hearing. The results appear to indicate that the hard of hearing children demonstrate greater imbalance in the double-plate tapping test but their average speed is about equal to that of the other children. They are superior to the normal hearing children in the steadiness test, except when blind-folded, and in the location memory test, although for this they tend to confuse the associated objects more frequently than do the control children. The latter are superior in the speed of eye-movement test. It is not clear how significant are these results and consequently what conclusions may be drawn from them. The data are presented in a manner deserving strong criticism. There is no index or bibliography.

Theoretische Grundlagen zum Aufbau einer Biologischen Medizin.

By Karl Kötschau and Adolf Meyer. Theodor Steinkopff, Dresden and Leipzig. RM. 13 (cloth); RM. 12 (paper). 9 x 6; xvi + 217; 1936.

This book represents an attempt to present the various philosophical concepts derived from biology in a manner useful and stimulating to the medical profession. The first section deals with such subjects as mechanism and vitalism, relation of body to "soul," the holistic conception of psychology, and the "Gestalt" theory. Section II more fully covers the holistic concepts of theoretical biology and psychology. Section III deals with parallel problems of medicine and biology. After an introduction on the common origin of biological and medical thought, the authors devote the rest of this part to a critical discussion of the attitude in medical scientific thought that seeks to reduce everything to a physical-chemical basis. This involves a discussion of the rôle of mathematics and statistics in medicine. Section IV, entitled *Laws and Rules*, is a philosophical discussion of induction and deduction in relation to medical thought and techniques.

This is an important contribution to the literature of theoretical biology and medicine.

Laboratory Practice. *Manual of Public Health.*

By J. R. Currie and Contributors. William Wood and Co., Baltimore. \$6.75. $8\frac{1}{2}$ x $5\frac{1}{2}$; xix + 378; 1936.

This book is designed to aid public health officers cope with the practical and technical problems that arise during the routine of their duties. It is a laboratory guide in the sense that it gives directions for the study and appraisal of a host of unrelated topics and a text in the sense that a number of these topics are discussed briefly in terms of their theoretical background. The book is divided into the following six sections: chemistry, bacteriology, protozoölogy, helminthology, entomology and meteorology. These diverse sections are related only to the extent that they contain material of conceivable importance for the sanitarian. Thus, in the chapter on bacteriology, a discussion of the staining technique for gonorrhoea films is found, while in the meteorology chapter the physical nature of fog is explained.

It is difficult to predict what real usefulness this volume will have. In matters of fact and organization the treatment appears excellent. In our opinion, however, the general scope is too heterogeneous to warrant much serious usage. There is an index and the text contains 169 adequate illustrations.

Live Long and Be Happy. *How to Prolong Your Life and Enjoy It.*

By Lewellys F. Barker. D. Appleton-Century Co., New York. $2.00. $7\frac{1}{2} \times 5$; viii + 224; 1936.

As the medical profession makes its strides toward prolonging the life of mankind in general, the average person naturally wants to know what he can do to increase his own life span and at the same time be free from suffering. To the person of average intelligence many medical terms and explanations are beyond comprehension. There has been a definite need for a book that would explain in language readily understandable to the layman the latest developments in medicine designed to prevent disease and prolong life. Dr. Barker's book fills just this need. Written in simple, every day language it tells the layman not only what aids medical science offers for prolonging life, but also how to make the most of added years. There is a brief index.

Vergleichend- Physiologisches Praktikum. *Mit Besonderer Berücksichtigung der Niederen Tiere.*

By W. v. Buddenbrock and G. v. Studnitz. Julius Springer, Berlin. RM. 9.60. $9\frac{1}{2} \times 6$; vi + 127; 1936.

A new manual encouraging a greater use of invertebrates for experimental and demonstration purposes. The authors believe that invertebrates serve the purpose just as well as higher animals in some cases in the demonstration and study of physiological phenomena, cost less, and require simpler and less expensive apparatus. Some marine forms are included in experiments outlined here. The experiments are arranged under the headings of the various senses, including chemical and kinetic; blood; water economy; nutrition and excretion. Many are suitable for high-school courses. American teachers can get some useful hints from a perusal of this book.

Absorption from the Intestine.

By F. Verzár, assisted by E. J. McDougall. Longsmans, Green and Co., New York. $9.00. $8\frac{3}{4} \times 5\frac{7}{8}$; xii + 294 + 12 plates; 1936.

The authors give a review of the more important experimental work, particularly recent work, dealing with the absorption of water, inorganic, and organic substances by various portions of the intestines, and the rôle this plays in regulating hydrostatic pressure. The discussion centers about their own research, which has mainly been to determine whether or not the physical laws of diffusion through a semipermeable membrane are sufficient to account for the activities of the intestine without appealing to biological selective forces. A very extensive bibliography is included.

Nutritional Factors in Disease.

By William R. Fearon. William Heinemann (Medical Books), London. 7s. 6d. net. $8\frac{1}{2} \times 5\frac{1}{4}$; xiv + 141; 1936.

The substance of this book is based on the author's essay which won the 1935 Buckston-Browne prize given by the Harveian Society of London. In the first part of the book are listed the chemical elements which enter into the composition of the human organism or occur in the usual human diet. The caloric consumption of man is also given a brief discussion. The second part includes chapters on the relation of disease to alterations of carbohydrate, protein and lipid balance. The inorganic elements and inorganic and organic microconstituents of foodstuffs are described in the three remaining sections. The exposition of the subject matter is unusually lucid and objective. There is an index and a bibliography of 268 titles which attest to the scholarly quality of this work.

Rapport sur le Pèlerinage au Hedjaz *de l'Année de l'Hégire 1354 (A.D. 1936).*

Conseil Sanitaire Maritime et Quarantenaire d'Egypte, Alexandrie. Free. $12\frac{1}{2} \times 9\frac{1}{4}$; 126 + 7 plates + 5 folding tables; 1936 (paper).

Each year there are a great number of people who make the pilgrimage to

Hedjaz, the vilayet in Arabia that has Mekka as its capital. And each year this report of the health conditions is published. It includes data on the number of pilgrims, the countries from which they come, the health regulations of those countries, the results of medical and bacteriological examinations at quarantine ports, and the number of caravan ships passing through the Suez Canal. It is interesting to note that while the number of pilgrims has steadily increased, the health conditions, due to better sanitary conditions and strict public health regulations, have steadily improved.

MEDICAL CLASSICS. *Volume 1, Number 1.*
Compiled by Emerson Crosby Kelly. Williams & Wilkins Co., Baltimore. Subscription price $10.00 per volume; single copies $1.25. 10 x 7; 78; 1936 (paper).

Medical Classics aims to present to the medical man in their original forms some of the classical discoveries by famous physicians. The first number opens with the works of Sir James Paget. There is a short biography and a complete bibliography, followed by two of his most famous articles, one on *Osteitis deformans* and the other on Paget's disease of the nipple. These papers are both given in full in the belief that as nothing more is known now about either of these diseases than Paget himself knew, the medical man of today will be better able to deal with the subjects for having read them in their entirety.

EPIDEMIC AMEBIC DYSENTERY. *The Chicago Outbreak in 1933. National Institute of Health Bulletin No. 166.*
U. S. Treasury Department, Public Health Service. Government Printing Office, Washington. 20 cents. $9\frac{1}{4}$ x $5\frac{3}{4}$; xi + 187 + 2 plates + 3 folding charts; 1936 (paper).

This is exclusively an account of the Chicago epidemic of 1933 which originated in two hotels. Apparently certain employees were infected and the disease was spread from them due mainly to a sewerage leakage into the drinking water. The book is divided into three separate studies (1) the epidemiology of the outbreak, (2) sanitary engineering in the hotels concerned, and (3) a clinical study of thirty-five cases.

LES HYPERGLYCÉMIES. *Étude Clinique et Physiopathologique.*
By Henri Warembourg. Preface by Professors Loeper and Polonovski. Masson et Cie, Paris. 65 francs. 10 x $6\frac{1}{2}$; xiii + 584; 1936 (paper).

Part I of this book contains an analysis of the current notions and facts relating to the general problems of glycemia. In Part II the author presents in detail the hyperglycemic reactions resulting from a series of physiological and experimental conditions. Parts III and IV discuss the diabetic and non-diabetic hyperglycemias, respectively, and the treatments tried for the maintenance of sugar regulation during such conditions. Much original work is included. The chief interest of the book will be to clinicians and pharmacologists. The bibliography covers 55 pages printed in small type. A monograph of first rate importance.

L'OSTÉOSYNTHÈSE AU POINT DE VUE BIOLOGIQUE. *Influence de la Nature du Métal (Étude Expérimentale).*
By G. Menegaux and D. Odiette. Preface by J. Verne. Masson et Cie, Paris. 37 francs. 10 x $6\frac{1}{2}$; vii + 174; 1936 (paper).

The authors experimented with various simple metals and alloys in an attempt to find the most suitable and practical from the points of view of non-toxicity, cost, availability, and malleability for osteosynthetic purposes. The experiments described in detail in this book were made with cultures of bone tissue and with live animals. The three rustless steels, V^2 A Extra, Nicral D, Platino-stainless D, were the only metals tested which met all the requirements. There is a bibliography of five pages but no index.

Toxicity of Food Containing Selenium as Shown by Its Effect on the Rat. *U. S. Department of Agriculture. Technical Bulletin No. 534.*

By Hazel E. Munsell, Grace M. DeVaney and Mary H. Kennedy. Government Printing Office, Washington. 5 cents. $9\frac{1}{8}$ x $5\frac{7}{8}$; 26; 1936 (paper).

Wheat containing selenium when fed to rats produced toxic symptoms; in particular generalized edema and liver injury. The lethal dose, the detrimental effect on growth and reproduction, the storage of selenium in the body of the rat, and the after effects of selenium are discussed.

Quarterly Bulletin of the Health Organisation. *Volume V, No. 3.*

Health Organisation, League of Nations, Geneva; World Peace Foundation, 8 West 40th St., New York. Annual subscription $2.50; separate issues 65 cents. $9\frac{1}{2}$ x $6\frac{1}{8}$; 180; 1936.

BIOCHEMISTRY

Il Glutatione. *Sua importanza nella biologia generale e nella fisiopatologia umana.*

By Giuseppe Barbaro-Forleo. Preface by Luigi Zoja. Tipografia già cooperativa, Pavia. 30 Lire. 10 x 7; xiv + 449; 1936 (paper).

Since its discovery, glutathione has been the object of numerous investigations and much speculation regarding its action. The staggering amount of literature thus accumulated is here critically and thoroughly reviewed. After due consideration of the chemical structure and biological significance of glutathione, the author reviews the subject with respect to its distribution in the living organism, its variation associated with physiologic and pathologic alterations, and finally the methods of demonstrating the presence and determining the quantity of this substance in the tissues and organs. From the mass of evidence—often contradictory—which has been published, the author arrives at the somewhat discouraging opinion that due to faulty methods of determination, much of the work done so far should be repeated and that many of the earlier conclusions derived should be abandoned. He believes, however, that beyond doubt one can assume that the principal action of glutathione is that of a catalyzer in the processes of tissue autolysis and an activator of tissue enzymes. The magnitude of the task accomplished by the author will be properly appreciated by all those interested in glutathione and in the metabolic activities of sulphur compounds in general.

Über Katalyse und Katalysatoren in Chemie und Biologie.

By Alwin Mittasch. Julius Springer, Berlin. RM. 3.60. $8\frac{1}{2}$ x $5\frac{3}{4}$; vii + 65; 1936 (paper).

Although the author probably knows more about catalysts in technological processes than anyone else, and has certainly contributed more than his share of important work on the subject, it is disappointing to find how little this book contributes to the elucidation of the principles of catalytic action. No fundamental idea underlies this factual survey of the various aspects of catalysis and a great number of biological phenomena. "One cannot see the forest for the trees" aptly describes the obscurity in this field associated with the great amount of technical work that has been done in it. There is an index of names.

SEX

Prostitution. *An Investigation of Its Causes, Especially With Regard to Hereditary Factors.*

By Tage Kemp. Levin and Munksgaard, Copenhagen; William Heinemann, London. Dan. Cr. 12 (cloth); Dan. Cr. 10 (paper). 10 x $7\frac{1}{2}$; 253; 1936.

The author has made a medico-psychiatric examination of 530 prostitutes living in Copenhagen. Since the purpose of the investigation was to study prostitution mainly from the biological standpoint, an attempt was made also to obtain as much information as possible concerning

the families of the subjects. At the time of the investigation the Danish police arrested prostitutes only when these women were found to have no regular legitimate occupation. Since the women examined were all in this category, the author believes that they are not a representative sample of prostitutes because it must be assumed that only the less intelligent ones fell into the hands of the police. This probably accounts in part for the fact that only 29.4 per cent of these women were found to be mentally normal and without defective intelligence. On medical examination, the author finds that about 50 per cent of the women had some serious chronic illness, gonorrheal salpingitis being of course the most common disease. As has been observed by others, the social and economic environment of these women is usually deplorable and there is a high incidence of alcoholism, criminality, prostitution, suicide, psychopathy, insanity and oligophrenia in their families. The fact that in some cases the mothers and maternal grandmothers were also prostitutes leads the author to believe that the mental make-up which predisposes to prostitution is frequently hereditary. While the facts, such as they are, are presented in an objective manner, the author is unable to arrive at any definite conclusion regarding the biologic aspect of prostitution. More hopeful than convinced he advocates better eugenic regulations and mental hygiene programs and believes that a reduction of social inequality might be helpful in preventing prostitution.

Sex and Personality. *Studies in Masculinity and Femininity.*

By Lewis M. Terman and Catharine C. Miles assisted by Jack W. Dunlap, Harold K. Edgerton, E. Lowell Kelly, Albert D. Kurtz, E. Alice McAnulty, Quinn McNemar, Maud A. Merrill, Floyd L. Ruch and Horace G. Wyatt. McGraw-Hill Book Co., New York. \$4.50. 9 x 6; xii + 600; 1936.

Terman and Miles offer in this work a masculinity-femininity test. It is a test of the mental personalities of the masculine and the feminine. The results of extensive data on this test are correlated with physical structures and psychological reactions of the individual.

Not so long ago the psychiatrist classified his subjects as "normal" or "feebleminded," or as "normal" or "insane." Thanks largely to Binet and his successors on the one hand, and to modern psychiatry on the other, no competent investigator in abnormal psychology now regards such a simple classification as adequate or even possible. The purpose of the investigations here reported has been the accomplishment in the field of masculinity-femininity of something similar to Binet's early achievement in the field of intelligence—the quantification of procedure and of concepts.

As a pioneer effort the work deserves praise. However, the test is lengthy for convenient use and, in spite of very extensive statistical work, no particularly significant or reliable results have thus far been obtained, with the exception of its correlation with homosexuality.

Période de Fécondité et Périodes de Stérilité chez la Femme. *(Lois d'Ogino et de Knaus.)*

By H. Vignes and M. Robey. Masson et Cie, Paris. 14 francs. $7\frac{7}{8}$ x $5\frac{1}{4}$; 87; 1936 (paper).

In the study of the periods of relative fecundity and sterility in the menstrual cycle two theories divide the favor of the physiologists and gynecologists. One affirms the strict limitation of the periods of physiological fecundity and sterility, while the other asserts the possibility of fecundation on each day of the menstrual cycle. This book is an impartial and detailed presentation of the respective arguments of these two schools of thought.

Vignes and Robey show how the facts in this kind of study must be interpreted, and give an example of the critical observations necessary in drawing conclusions. The observations presented here are the result of their practical experience and their intensive study of reproductive physiology. No exact law has yet been proved, and it is necessary to study each

case with individual attention and method. More delicate and exact methods must be adopted before it can be determined with assuredness whether a particular woman is the norm or the exception. The book contains a good bibliography.

White Women, Coloured Men.

By Henry Champly. Translated from the French by Warre Bradley Wells. John Long, London. 18s. 9 x 6; 319 + 8 plates; 1936.

The theme of this ill-organized, diffuse, and tiresomely verbose treatise is that: "*The Coloured peoples have discovered the White woman*—as a marvel; as a wonder from the physical, the artistic, the social and even the religious points of view; as an idol worthy of being desired above all else." This theme is repeated, developed, inverted, varied, and subjected to all the ornamentation of scandalous gossip that could be garnered by an ingenious author through the expedient of seeking low company in the lowest corners of cities all over the world.

In fairness it should be said that no library of pornography will be *quite* complete without this well-indexed book. But equally we should warn intending buyers that it has none of the hearty robustness of Pantagruel, but tends rather towards the slightly slimy school of the *demi-vierge*, with never a naughty word in it.

Oestrus, Receptivity, and Mating in Chimpanzee. *Comparative Psychology Monographs, Vol. 13, Serial No. 65.*

By Robert M. Yerkes and James H. Elder. Johns Hopkins Press, Baltimore. 75 cents. 10 x 6¾; 39; 1936.

This is an interesting and well executed study of the reproductive cycle and copulatory behavior of the chimpanzee. It is shown for this animal that the sexual cycle lasts about five weeks and can be divided into six physiological phases—menstrual, postmenstrual, tumescent, maximal swelling, detumescent and premenstrual. The female chimpanzee largely controls the mating pattern and is highly receptive sexually during the tumescent and maximal swelling periods. This receptivity reaches its height at the time of ovulation when the female expresses her sexual desire to the male both gesturally and posturally. The authors conclude that "social and psychobiological conditions" as well as "physiological factors" are highly important in shaping the mating behavior of this primate. In this respect they feel that apes and humans have much in common.

Encyclopaedia of Sexual Knowledge.

By A. Costler, A. Willy and others under the general editorship of Norman Haire. Eugenics Publishing Co., New York. $6.00. 9 x 6; xx + 567; 1936.

The practiced reviewer of "sex books" could easily sit down and prepare a standardized review that would serve, with minor alterations, as an appraisal for nearly the entire group. This is another way of saying that most of these books are stereotyped and exhibit little variation or originality, regardless of author, publisher, or prospective reader. All contain remarks about the physiology of reproduction, the nature of sexual intercourse and sexual perversions, and nearly all leave the boundaries of science and enter the fields of aesthetics and ethics with poetic gestures. The present book is no exception. In terms of organization and exposition it is better than some and worse than others. In terms of content it is like all the rest. It can be recommended only with an evasive yes and no.

BIOMETRY

Elements of Probability.

By H. Levy and L. Roth. Oxford University Press, New York. $5.00. 8¾ x 5½; x + 200; 1936.

The authors have, in their own words, "striven to provide a detailed criticism of the various self-contained theories of probability that have been advanced

from time to time." The result is an excellent text, one that should be of considerable use to all students of mathematical probability.

The second chapter gives a very clear statement of the meaning of chance and of the definition of probability. The derivations of the addition and product formulae, Tchebycheff's problem, Bernoulli's theorem, etc. follow lines common to books on mathematical probability. A separate chapter is devoted to the extension of continuous distributions. The derivations of the Gaussian law and other forms of hypothetical populations are given in detail. The last chapter on "The Use of Probability in Scientific Induction" is a clear presentation of the problem of sampling. Correlation theory, and the tests of significance for small samples are treated somewhat briefly.

Numerous examples are scattered through the book. There is an index, but no bibliography.

NARRENSPIEGEL DER STATISTIK. *Die Umrisse eines statistischen Weltbildes.*

By Ernst Wagemann. Hanseatische Verlagsanstalt, Hamburg. RM. 6.80 (paper); RM. 7.80 (cloth). 9 x 6; viii + 255; 1935.

The remarkable nature of German official and semi-official statistics since the beginning of the Nazi regime has been the subject of not a little criticism and ridicule by distressingly skeptical foreign statisticians. The present volume is rather more of a brilliant counterattack upon the hecklers than a meek defense. They are reminded that "people in glass houses..."

STATISTICAL METHODS FOR RESEARCH WORKERS. *Sixth Edition, Revised and Enlarged.*

By R. A. Fisher. Oliver and Boyd, Edinburgh and London. 15 s. net. $8\frac{5}{8} \times 5\frac{5}{8}$; xiii + 339 + 6 folded tables; 1936.

In this new edition of Professor Fisher's well-known and widely used text a number of additions have been made, notably a new test for homogeneity of material with hierarchical sub-divisions. The general plan and structure of the book has been in no wise altered from earlier editions.

PSYCHOLOGY AND BEHAVIOR

THE NATURAL HISTORY OF MIND. *Tarner Lectures delivered in Trinity College, Cambridge, 1935.*

By A. D. Ritchie. Longmans, Green and Co., New York. $4.50. $8\frac{1}{2} \times 5\frac{1}{2}$; viii + 285; 1936.

This series of lectures apparently had for its major purpose the orientation of psychology in the general field of modern science and philosophy. We feel the author deserves high praise for the clear, logical, and unprejudiced manner in which he carries this work out.

We state briefly a few of the topics he develops at some length: (1) As life processes including those of the mind quite probably follow the "generic" laws of physics and chemistry, these laws will form a useful basis for psychological research but alone can never answer the highly "specific" problems of this study. (2) The ordinary conception of cause and effect may be held to be true in the macroscopic scale (in which scale psychology lies) even though a sequence of events may follow a given law only as a statistical result of random motion in the microscopic scale. (3) "The division of objects into inanimate, animate, and personal is convenient as a first approximation but cannot be the last word."

Of special interest is his critique of a number of the present day approaches to the study of psychology. The experimental method requires that one variable be changed in known quality or quantity while all other important variables be held constant. In nerve physiology this can be accomplished with single nerve fibers, but too many variables are involved in higher nervous and mental activity to make this possible. Pavlov's work with conditioned reflexes is again a study of isolated, relatively simple nervous channels rather than a study of the normal reactions of the mind. Of one such experiment the author remarks that it

was "a model of experimental technique from the point of view of eliminating uncontrolled variables; unfortunately the dog was one of the variables eliminated!"

THE SCIENTIST IN ACTION. *A Scientific Study of His Methods.*
By William H. George. Williams and Norgate, London. 10s. 6d. net. $8\frac{1}{2} \times 5\frac{1}{2}$; 355; 1936.

To develop a complete and precise picture of the scientist at work is a difficult undertaking and requires no meager amount of analysis of both the man and his method. The author of this volume has very successfully developed just such a picture. Organized and written in a very logical and detailed manner, the book is the result of much keen and intensive scientific observation and reaction.

The fundamental idea conveyed is that scientific research is a form of human action which gives two kinds of results: the human yet impersonal observations called facts, and such arrangements of facts as classifications, laws, and theories. George believes that

> if the scientists' methods are to be applied to problems of civilization, men able to make the applications must be found and must be trained in scientific action. The scientific method itself must be taught, and more than that, it must be taught in combination with a science or some other subject in which the application is illustrated.

Stimulating in essence and progressive in outlook, the book should be a part of every research worker's private library. There is a short bibliography on the general aspects of the scientific method, and an index.

CONDUCT AND ABILITY. *A Text-book of Psychology from the Hormic and Noegenetic Standpoints, with Practical Exercises for Students.*
By Frances Banks. Methuen and Co., London. 10s. 6d. net. $7\frac{1}{4} \times 4\frac{7}{8}$; xi + 399; 1936.

This is a text book for educators. The aim of the author is to present the most recent findings in learning, remembering, forgetting, attention, etc. in a practical way for use primarily in teaching children. Part I, on the *psychology of conduct*, is devoted primarily to a discussion of the neural mechanism, reflexes, instinct and emotion from the physiological standpoint. There follow several chapters on sentiment, character, will, the development of self in relation to (a) authority, (b) society, (c) sex and (d) reality. Part II on the *psychology of ability* deals in detail with Spearman's theories of intelligence and cognition, intelligence testing (Binet-Simon), sensation, perception, learning processes, remembering and higher thought processes. Part III is devoted to *methods* of psychology and practical experiments and exercises.

Throughout the book a great deal is said about the hormic view of conduct and the noegenetic doctrine of cognition in simple language. A useful book for teachers.

PRACTICAL EXAMINATION OF PERSONALITY AND BEHAVIOR DISORDERS. *Adults and Children.*
By Kenneth E. Appel and Edward A. Strecker. The Macmillan Co., New York. \$2.00. $8\frac{1}{2} \times 5\frac{1}{2}$; xiv + 219; 1936.

This book has been designed for the use of medical students, general practitioners, psychiatrists, personnel advisors, teachers, etc. Although it is impossible to give exact rules for the practise of psychiatry, the authors believe that "psychiatry does have hints, suggestions, techniques and methods to impart" which can supply needed guidance to the beginner until he has had sufficient experience to devise methods and techniques of his own.

The first part of this book deals with the psychiatric examination of adults, the second part with the psychiatric examination of children. Both parts contain suggestions for making the examination, outlines of the information to be obtained in general and specific problems (parent-child relationships, temper tantrums, etc.), suggested tests and other practical information. The techniques described have been used by the students

in the School of Medicine at the University of Pennsylvania, by physicians in the Department of Mental and Nervous Diseases of the Pennsylvania Hospital, and in the private practise of the authors. Thus the material contained in this book has been found to fulfill its purpose not merely in theory but also in actual practise.

INTELLIGENCE.

By Theodore Dorros. (This book is not for sale.) $8\frac{3}{4}$ x $5\frac{3}{4}$; 153; 1936 (paper).

The author, as far as we can make out, believes that all human beings, everywhere, and at all times have ordered, now order, and (unless they follow his advice) will continue to order their lives with 100 per cent lack of "intelligence."

The following extract was selected at random:

> "*The only way* for the race to start on the road to intelligent collectivism is: to unceasingly reduce reproduction and coalesce its varieties. To produce units of an always 'better quality' and in an always better collective environment. It is not a question of 'supermen,' 'angels,' 'demigods,' but *simply* of eventually giving birth to 'ordinary, very ordinary mortals' with their chemistry functioning in the best possible *human* way and with an 'inherited' and actual 'environment' that would produce *most normal* conditions of collective life and physico-mental states for these *normal* mortals. Nothing else. But on the other hand *it is* a question of eliminating the 'masses' which are not needed to serve the 'superior' units, since these 'superior' ones *are not needed either*, both being *below* the first level of the elementary normal human intelligence."

[Reginald the Office Boy, whose quite normal adolescent love for rhetorical flourishes is sprouting along with his beard, says that Mr. Dorros's announcement that his book is not for sale "is a hyperbolically supererogatory contribution to the catallactics of publishing!]

THE SENSORY BASIS OF MAZE LEARNING IN RATS. *Comparative Psychology Monographs, Vol. 13, Serial No. 64.*

By C. H. Honzik. Johns Hopkins Press, Baltimore. $1.50. 10 x $6\frac{3}{4}$; 113; 1936 (paper).

The purpose of the investigation reported here was to study experimentally the relative influence of the different sense-organs on maze learning in rats. Twenty-three groups, each containing from 42 to 53 pigmented and albino rats, were used. The ages of the animals ranged from three to six months. Two identical elevated 14-blind mazes constructed by the author were employed in the experiment. By operative and other procedures the function of one or another sense organ was reduced or abolished. In agreement with previous investigations, the author finds that vision assumes the dominant rôle both in learning and in maintenance of the maze habit. Olfaction also plays an important part but secondary to vision. Next in importance comes audition. Tactual stimuli are of only minor importance. Kinesthesia alone is not sufficient for learning but it appears that these impulses are essential to acquisition of skill. From this series of experiments, well executed and clearly presented, the author reaches the conclusion that the motor responses constituting the maze habit are not due to specific stimuli but to complexes or patterns of stimuli. The list of references is fairly adequate.

THE PSYCHOLOGY OF DEALING WITH PEOPLE. *Appealing to the Want for a Feeling of Personal Worth.*

By Wendell White. The Macmillan Co., New York. $2.50. $7\frac{3}{4}$ x $5\frac{1}{2}$; xiv + 256; 1936.

Part one of this book on dealing with people in life situations in general could serve as a text for classes in elementary salesmanship, and, in fact, might serve as an introduction to the more inspirational methods of Vash Young. The remaining three parts deal with the prevention of wrong doing and otherwise peculiar behavior, and the furthering of mental health. Neat and tidy solutions are given with each sample of "peculiar" behavior. On page 217 the author quotes, with some approval from *Elementary Principles of Education* by Thorndike and Gates as follows: "When we inquire what occasions human happiness, we shall find that it depends upon human wants."

Reactions of the Human Machine.
By John Y. Dent. Victor Gollancz, London. 8s. 6d. $7\frac{3}{4} \times 5$; 288; 1936.
The Human Machine.
By John Y. Dent. Alfred A. Knopf, New York. \$2.50. $8\frac{1}{8} \times 5\frac{1}{2}$; x + 294 + v; 1937.

These two volumes (one for English, the other for American readers) are unlike in format but alike in substance matter. A plea is made especially for "the physiological, material attitude of patients and doctors towards life and its discomforts and the exclusion of magic and the supernatural from the treatment of the human mechanism." An attempt is made "to describe this mechanism and the way it responds to its environment, especially the way it responds to words, to suggestions." Psychoanalysis and hypnosis are discussed in the latter part of both books. "Psychoanalysis seeks to revive buried vestiges of desire. Hypnotism seeks to bury them deeper. I consider their removal unnecessary, as they have to be buried again and this may not be easy." How to be a hypnotist is taught in one easy lesson of only six pages.

Measurement in Psychology.
By Thelma Hunt. Prentice-Hall, Inc., New York. \$3.00. $8 \times 5\frac{1}{2}$; xx + 471; 1936.

This text book designed for college courses in the subject completely reviews the various types of psychological measurements with a general description of the use of each. Covering so many techniques of various types in one volume, there is practically no attempt made to examine any of them critically. Unless the teacher is entirely capable of supplying this lack, the text is hardly adequate for a course intended to give a really intelligent understanding of this difficult subject. It is, however, an extremely useful book for handy reference.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

Scientific Progress. *Sir Halley Stewart Lecture, 1935.*
By Sir James Jeans, Sir William Bragg, E. V. Appleton, E. Mellanby, J. B. S. Haldane and Julian Huxley. The Macmillan Co., New York. \$2.00. $7\frac{3}{4} \times 5\frac{1}{2}$; 210 + 4 plates; 1936.

Six popular lectures are here presented by prominent British authors. The titles are, respectively: Man and the Universe; The Progress of Physical Science; The Electricity in the Atmosphere; Progress in Medical Science; Human Genetics and Human Ideals; Science and its Relation to Social Needs. Each one is excellently written and contains much material of value to biology.

In chapter IV the history of medicine is traced from its beginnings, although recent developments are emphasized—entertainingly and with profitable, rich detail. Chapter V concerns the two questions of sterilizing the unfit and racial superiority, with that usual clarity and intelligence distinguishing Professor Haldane's writings. The final chapter considers scientific research as a social-anthropological trait; its history, functions, general behavior, adaptations to environment, and dependence upon other traits, particularly those of capitalism and the profit motive. Stress is laid upon the imbalance between research motivation and social needs.

The Harvey Lectures. *Delivered under the Auspices of The Harvey Society of New York 1935–1936. Under the Patronage of the New York Academy of Medicine Series XXXI.*
By Max Bergmann, Robert M. Yerkes, Peyton Rous, B. A. Houssay, John Farquhar Fulton, Richard E. Shope, Warren H. Lewis and I. deBurgh Daly. Williams and Wilkins, Baltimore. \$4.00. $8 \times 5\frac{1}{4}$; 255; 1936.

The high standard set by previous lectures in this series has been well maintained in those presented in this volume. Biologists will find The Significance of Chimpanzee-Culture for Biological Research by Robert M. Yerkes of special interest. The other lectures included in the present volume are: Proteins and proteolytic enzymes, by Max Bergmann; the virus tumors and the tumor problem, by Peyton Rous;

relations between the parathyroids, the hypophysis and the pancreas, by B. A. Houssay; the interrelation of cerebrum and cerebellum in the regulation of somatic and autonomic functions, by J. F. Fulton; the influenzas of swine and man, by Richard E. Shope; malignant cells, by Warren H. Lewis; the physiology of the bronchial vascular system, by I. de Burgh Daly. Bibliographies are provided for most of the papers. There is no index.

THE STUDY OF THE HISTORY OF SCIENCE.
By George Sarton. Harvard University Press, Cambridge. $1.50. $8\frac{1}{4}$ x $5\frac{3}{4}$; 75; 1936.

The author defines history of science as the history of the acquisition and systemization of positive knowledge. The functioning of the historian, as he sees it, does not differ essentially from that of the entomologist. The one collects scientific ideas, the other collects insects. Both have the common objective to increase knowledge. In this small volume, Sarton emphasizes certain elementary but important points in method. Such are, for example, accuracy and precision in citing facts, and determination of the relative value of era, environment, and personalities. Included is a bibliography, almost half as long as the text matter, which contains a selected list of references that will prove very useful to the student.

THE AUTOBIOGRAPHY OF A SCIENTIST. *Being the Memoirs of Doctor Henry Manure, Professor of Archaeology, Palaeontology and Egyptology at Derbytown University as Recorded by His Amanuensis.*
Scientific Publishing Co., Princeton. $2.25. 9 x 6; xiv + 177; 1936.

Anybody who is anybody expects to be caricatured and usually likes it provided it does not hit a really sensitive spot. But the truth hurts. Whoever the learned "Dr. Henry Manure of Derbytown University" may be, he applies a sharp tongue and keen wit to a satire of scientists probably a little too pointed to be thoroughly enjoyed by them. Many of the points are deserving of ridicule and the book is amusing in spots, but most of the humor is rather bitter than funny. Lest others get a free laugh exclusively at the scientist's expense, the clergy, the business man, and institutions in general all come in for their share of the beating.

PREPARATION OF SCIENTIFIC AND TECHNICAL PAPERS. *Third Edition.*
By Sam F. Trelease and Emma S. Yule. Williams and Wilkins Co., Baltimore. $1.50. $7\frac{1}{4}$ x 5; 125; 1936.

This little book has proved its usefulness by appearing in a third edition. It has been revised and enlarged but not *sufficiently* enlarged to satisfy this reviewer. The authors give sound advice on all the main points in preparing and seeing through the press a manuscript dealing with scientific and technical subjects. But with the addition of only a few more pages—certainly not more than twenty-five—treatment of some of the more perplexing questions on punctuation, the arrangement of complicated tables, etc. would add greatly to its value.

THE HANNA STAR DOME. *Pocket Natural History No. 6, Astronomical Series No. 1. First Edition.*
By Dorothy A. Treat. The Cleveland Museum of Natural History, Cleveland. 25 cents. $6\frac{3}{4}$ x $3\frac{3}{4}$; 47; 1936 (paper).

The relation of this programmatic pamphlet to biology may seem to the superficial mind a bit on the exiguous side, but such a view overlooks some important points, to wit: (1) Mark Hanna was, after all, a quite extraordinary specimen of the genus *Homo*; (2) as scientific amusement enterprises planetaria are running close seconds in gate receipts to the anatomical

exhibitions sponsored by the Minsky Frères; (3) natural history museums acting as planetarium entrepreneurs get the rake-off; and so (4) are by that much better able to promote their strictly biological activities. Thus all works together *ad majorem gloriam Dei*, and merits—even demands—notice in this forward-looking Family Magazine for Biological Homes.

University of Colorado Studies, *Volume 24, Number 1. Abstracts of Theses and Reports for Higher Degrees.*

University of Colorado Press, Boulder. \$1.00. 10 x 6⅞; 77; 1936 (paper).

Tufts College Studies, *Volume VI, No. I. (Scientific Series Nos. 51–56.)*

Tufts College Library, Tufts College, Mass. 9 x 6; 72; 1936 (paper).

WILLIAM MORTON WHEELER

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THE NATURE OF ORGANIC WHOLENESS

By FRANCIS G. GILCHRIST

I

WE HAVE become so accustomed to building toy houses of blocks, or constructing automobiles from accurately machined parts, that we think of wholes as formed by putting pieces together. It comes therefore as somewhat of a struggle to picture to ourselves a process, such as the molding of a vase from a lump of clay, in which the whole precedes the parts and gives to the parts their meaning. Yet it is this latter analogy and not the former which we must have in mind if we are to comprehend aright the nature of the organism and the processes of its becoming.

If one would understand a machine he will take it apart, either actually or in his imagination, and will study it piece by piece, noting the characteristics of each piece and the relations of each piece to each other piece. Then by synthesizing the knowledge thus obtained, he will understand the machine. On the other hand, if one would understand the vase or organism, he must begin by first comprehending the thing as a whole. Only then will he be able to discern the significance of the parts. Indeed, he may fail to discern discrete parts at all; yet his appreciation of the whole will not thereby be impaired.

But how does one comprehend a whole except by knowing the parts and observing their relations to one another? Can one know a whole directly? Yes. Indeed, it is the natural thing to do. The child knows a cat before it distinguishes a head. It knows its mother's face before it discerns her eyes. It would be very unnatural indeed for a child to reconstruct its concept of a face by piecing together its various anatomical structures or its characters. It is just as unnatural for a biologist to construct his concept of an organism by adding together entities of any sort, be they organs, germ layers, unit characters, cleavage cells, or organ-forming stuffs. It is only a habit of mind which we have acquired through familiarity with houses and machines which leads us to think that way.

But admitting that one might form some general concept of a whole before discerning parts, is it nevertheless not true that a whole is nothing more than the sum of its parts plus whatever interrelations may exist between its parts? This is indeed true with regard to some wholes; namely, the artificial wholes

which men construct; but it is not true of organic wholes such as we commonly find in nature. Certainly it is not true of organisms. Let us examine this assertion from the standpoint of a part.

A thing may become a part in one or the other of two ways: (a) It may begin as a discrete and independent unit and then by integration with other units secondarily find itself a part of a whole. Thus the brick becomes a part of a house. Thus also a balance-wheel becomes a part of a watch. Houses and watches as thus fabricated are merely the sums of inter-related parts. Their wholeness, or had we not better say, their togetherness, is the result of a "creative synthesis" which has taken place. Their special properties depend upon the special relations between their constituent parts. (b) In organic wholes, on the other hand, the whole exists first, and the part arises by differentiation within the pre-existing whole. Homogeneity precedes heterogeneity. At first the parts are indistinguishable from one another except, perhaps, by future reference. Thus a "part" of a lump of clay may be described as the prospective handle of a vase, or a part of an amphibian egg may be termed "presumptive neural plate"; but the part is not as yet handle, nor neural plate; nor is it entirely certain that it will become such.

Organic wholes, then, are not primarily composed of parts, or even of inter-related parts. They are not the result of an integration or of a "creative synthesis." Quite to the contrary, they precede their "parts" in time, and remain superior to them in space.

It is likely that the reader is not convinced. Perhaps he is a morphologist and is engaged in dissecting organisms and studying their structures, dead or alive. To him the organism is a definite pattern in space; or indeed a succession of such patterns, if he be an embryologist. But the morphologist's organism is, of course, not the whole organism. It is only that aspect of the organism which he has chosen to abstract and study. Perhaps the reader is a physiologist, and concerns himself with the functioning of organs or the rôles of forces. He, no doubt, has become much interested in the interrelationships of processes, and has noted the remarkable self-regulating devices within the functioning system. But his organism, like the organism of the morphologist is an abstraction from the whole.

To study the organism as an organic whole, one must be interested not merely in the organism which is and does; but he must know also what under other conditions and in other relationships this same mass of living matter will be and do. He will want to know how and why the organism came to be what it is and to do what it does. Indeed, he will go further and will seek to comprehend, if possible, the nature of the organic wholeness in itself; that is, the wholeness which precedes the being and the doing, and which gives to parts their significance.

Let us illustrate this by reference to a very primitive animal, the fresh-water polyp *Hydra*. A piece of the body stalk of this animal is morphologically only a piece of the body stalk. As such it may be killed, sectioned, reconstructed, and described. Physiologically it is an organization of more or less specialized cells carrying on the numerous functions of living. But isolate this piece of the stalk (the following is true of the central portions of the stalk), and immediately these same cells commence to reorganize themselves; and in a short time a whole new polyp has been formed (Trembley, 1774). It is therefore true that what a part of a polyp is and does is a function of its

position in a whole; and the whole must be understood first, if the structure and functioning of the part is to be comprehended.

In the discussions which follow, the living matter will be treated as though it were a single system; which, of course, it is. For our present purposes it will not be necessary, except incidentally to consider the division of protoplasm into nucleus and cytoplasm; nor its subdivision into cells (Whitman, 1893; Sharp, 1934). It will even be unnecessary to consider the individual activity of the various neoblastic and restitution cells by which the molding and regulations of organisms is accomplished (Stolte, 1936). These cells, important as they are, act within and are subordinate to the pattern of the organism as a whole. Furthermore, problems of heredity and environment, such as the action of genes and of physical factors upon the course of development, need not detain us here. All of these various elements which contribute to development and regeneration may be looked upon as problems of the second order; second, that is, to the problem of the development of the organism considered as one system.

II

At least two concepts of wholeness are possible. According to one, the part is what it is, and does what it does, by reason of its present relationship to a labile and self-regulating whole. The organism, according to this view, is a system tending toward equilibrium, of the type which has recently been termed a "configuration" or "gestalt" (Köhler, 1927). According to the second view, the part is what it is because of its relation in a system which has had a common origin and is evolving together (Schaxel, 1922). The organism is a system undergoing a history. According to the first view time means little to the organism; and development may be backward as well as forward. According to the second view, time is of the very essence of life (cf. Bergson, 1913, p. 16). We shall consider the first of these two concepts in the present section.

The configurational or gestalt viewpoint gives us a clear picture of reproduction. Every organism is at first a part of a pre-existing organism. It is a "chip off the old block." By development the part regulates itself into a whole (cf. von Baer, 1828). The part may be a portion of a cell, as when a protozoön divides to form two protozoa; or it may be a single cell, as when an egg develops into a many-celled being; or it may be a group of cells, as in asexual reproduction by budding or by fission. In fact, when looked at thus, reproduction and regeneration are seen to be of the same stuff, for in both a part remodels itself into a new whole.

How shall one visualize a gestalt or organismic pattern of wholeness? Those who have made the attempt have generally conceived of an active "field" (physiological gradients, Child, 1915b, 1928; embryonic field, Gurwitsch, Rudy, 1931, Weiss, 1926; gradient-field, Huxley and DeBeer, 1934, Huxley, 1935a) in which the forces; for example, the concentrations, potentials, rates of activity, tensions, and such like; are oriented and gradated with respect to the pattern as a whole. Each point within the field thus possesses the two vector characters of direction and dimensions; and it derives these characters from its position within the field. The field may be a simple linear field, as when a hydranth regenerates at the end of a piece of hydroid stalk (Driesch, 1897; cf. Gilchrist, 1937). It may be a two-dimensional field, as when a chick blastoderm (Waddington, 1932) or the neural plate of an amphibian embryo (Kingsbury,

1922) becomes structurized. Or it may be a three-dimensional field, as in the differentiation of a limb bud (Harrison, 1918, 1921; Milojewić, 1924). But in any case it is the location within the field which gives to parts their significance, and which makes it possible to say of a given cell or area that it is prospectively this thing or that.

Fields obey certain "field laws" which have to do with wholeness and with unity. (They obey these laws provided, of course, that the material which composes the field remains plastic and labile.) By *wholeness* is meant the tendency of the field to assume and maintain orderly and complete patterns, in which the parts are arranged in normal sequences and characteristic relationships one to another. By *unity* or oneness is intended the proclivity of the field to possess a single center of dominance or control, to which adjacent areas are subordinated, and in spatial relation to which field forces are organized (i.e. oriented and gradated). It is not easy to separate unity and wholeness; yet to a degree they are distinct. Thus a crystal may be said to possess wholeness but not unity, since it lacks a center of control, and since it would be possible to cut a crystal into parts, each part a whole. A crystal is thus an orderly aggregate rather than an individual. On the other hand an incomplete object, for example, a half embryo or a defective limb, may have a degree of unity (single center of control) and yet not be a whole.

The "field laws" which primarily concern wholeness may be stated as follows (cf. Weiss, 1926):

(a) When material is taken from a field, the remainder reorganizes itself into a new whole field. In fact, when a field is subdivided into two or more parts, each part immediately reconstitutes itself a whole field of smaller size. Thus a piece of a flatworm, or an isolated blastomere of a sea-urchin egg (Driesch, 1900a) or half of the limb area of an amphibian embryo (Harrison, 1918) will develop as a whole. A very small piece of an unfertilized sea urchin egg may, after fertilization, develop a whole embryo, balanced in all its structures, yet of small size (Tennant, Taylor, and Whitaker, 1929).

(b) When two fields are brought together harmoniously; that is, with their axes oriented and gradated parallel to each other, the two fields fuse into one, and a single whole field of double size results. Thus two sea urchin eggs fused together may produce one giant embryo (Driesch, 1900b).

(c) When two fields are brought together disharmoniously, that is with their forces not parallel, adjustment between the two fields takes place, and some sort of reduplication results (Bierens de Haan, 1913). Of this nature are the numerous types of double and multiple monstrosities which are sometimes found in nature.

(d) When a small region of one field is implanted into another location (or in another orientation) in another field, either one or the other of two things happens: If the implant is sufficiently plastic and labile, it will become an integral part of the larger field, and will develop according to its new location in the whole (Spemann, 1918; Mangold, 1923). If, however, it has attained to some degree of stability, it will distort the field, at least locally, and some sort of an abnormality will result.

(e) When a field is subjected to an environmental force acting in one direction, it sometimes happens that the orientation and dimensions of the field are altered, or even reversed altogether. For example, the orientation and hydranth size of a regenerating piece of hydroid stem is much influenced by any contacts of the piece with the bottom (Child, 1915b, 1927). Also a temperature differential imposed upon a developing amphibian egg is able to profoundly affect the symmetry and balance of the embryo which results (Gilchrist, 1928, 1929). By such distortions, however, the wholeness of the field is not destroyed.

A second series of "field laws" have to do primarily with unity. (We here follow rather closely the account of Child, 1929, pp. 22–25; see also Huxley and DeBeer, 1934.)

(a) Some region of the field is commonly a "center of intensity" (Spemann), apex, or high point, in spatial relation to which the entire field is organized. This is the principle of "physiological dominance." The orientations and gradations which are thus determined constitute one aspect of what is termed "polarity."

(b) The center of the field becomes during develop-

ment some particular part of the organism. For example, the high end of the gradient of an egg or a fragment of a hydroid becomes by development or regeneration, the lips of the mouth. This is a self-differentiation, since it may take place quite independently of other regions. For example, a very small piece of a hydroid, if it regenerates at all, will form oral lips even when other parts are lacking (Child, 1907).

(c) Other regions of the field develop in spatial relation to the center of the field. This is known as "physiological subordination." The center becomes one thing; the subjacent areas become other things according to their relative positions in the center-controlled field. It is thus that the oral, tentacular, and sub-tentacular zones of the hydranth are determined.

(d) One of the influences exerted by a dominant center upon subjacent levels is that of inhibition (Rand, 1924). For instance, if two apices should begin to develop not far from each other, it commonly happens that the more active or favored of the two inhibits the development of the other. It is itself not inhibited.

(e) By sufficient local stimulation it is sometimes possible to liberate a subordinate region from the control of a dominant center and permit it to be free. This is "physiological isolation." The region then becomes the center of a field of its own, a new development occurs, and a new individual is produced. For example, a wounded area on the side of a hydroid is sometimes able (presumably as a result of the increased metabolism of healing) to become the dominant center of a new organism (Child, 1928).

(f) The different regions of a field are differentially affected by conditions of the environment. Thus the dominant area of a field is commonly the first to succumb to lethal concentrations of a toxic chemical ("differential susceptibility"), and the first to recover from the effects of sub-lethal concentrations of the same reagent ("differential acclimatization") (Child, 1927, 1928).

Too much emphasis, however, should not be placed upon the one-way (dominance-subordination) relations of regions within the gradient-field. Hörstadius (1935) in particular has showed that normal development in the sea urchin requires a balance between the "ectodermalizing" gradient which centers at the animal pole, and the "entodermalizing" gradient which has its center at the vegetative pole of the blastula. There is reason to think of the field as a self-adjusting configuration which adapts itself to the size and shape of the material, the distribution of such differentiated stuffs (see later) as may be present, and also to the forces of the environment.

An important question concerning a gestalt or field pattern is its relation to the second law of thermodynamics. Does the field represent a static equilibrium or configuration of rest, toward which the living system tends, and in which its forces are balanced (Köhler, 1927)? If so, then the field is a state of maximum entropy, like the fluid of a suspended drop. Or does the field represent a "dynamic equilibrium," comparable to that which gives form to a water-spout, or to a candle flame (cf. Roux, 1905, pp. 105ff). If this latter is the case, then the forces of the field are at least in part kinetic; entropy is not at a maximum, and work is being done. To one observing the organism solely as a form in space, it will seem as though the field were indeed a static equilibrium; but to anyone who will come close enough to the organism to see its life processes in action, the field will appear, rather, as dynamic (T. H. Huxley, 1879, pp. 84–85).

Certain corollaries follow, if we may accept this second viewpoint. Should experiment finally confirm these corollaries, the fact would go far to place upon a physiological basis, the science of organismic form or pattern.

(1) If the dynamic field is to persist, energy must be continuously supplied. Let the wind cease to blow, and the water-spout will collapse. Let combustion fail, and the candle flame will go out. Let oxidative metabolism (or other source of energy) cease, and the field within the organism will be wiped away. It is thus, so we may believe, that many of the marine invertebrates when exposed to cold or to lack of oxygen, lose their patterns of differentiation and melt down into formless masses (Driesch, 1902;

Huxley, 1926). Thus also an egg or embryo, when similarly treated may lose the unitary field which would have produced a single embryo, and on return to more favorable conditions may regenerate two or more, often disordered fields (Stockard, 1921). In some such manner twins are produced.

(2) A second corollary is that the energy required at any point to sustain the gradient-field at that point will depend upon the steepness of the physical and chemical gradients rather than upon the absolute value of concentrations or potentials. In mathematical terms, the energy required to maintain the field at a position, S, will depend, not upon C (e.g. an absolute concentration) but upon dC/dS. This is true, of course, provided that the field remains labile.

(3) A third corollary is that two fields will require more energy to sustain them than one field which is equal to the sum of the two fields in size. At least this will be true if the larger and the smaller fields are equally able to produce and to sustain like differentiations of structure. This follows from the fact that in the smaller fields the gradations will be steeper, and will therefore require more energy to preserve them. For example, it is said to take more energy for two isolated blastomeres of the sea urchin egg to produce two whole embryos of half size, than it takes for these same two blastomeres when united to produce one embryo of full size. (Tyler, 1933, cf. 1935. Tyler gives another explanation.) Again, when pieces of the lower organisms are cut smaller and smaller, the regenerations which result are less and less complete, and require more and more time. Presumably in the smallest pieces the energy available is inadequate to support a field of sufficient steepness to produce organic differentiation at all.

What may be the physical nature of a field? Some speak in terms of molecular orientation and crystal structure (Loeb; Driesch, 1908; Przibram, 1929); but if so, one must think of the orientations as at least for a time labile and subject to the forces of the field as a whole. Some speak of tensions (Morgan, 1901; Weiss, 1933), or of forces of attraction and repulsion. Others find evidence of gradations in structural and functional properties of many sorts: concentrations, granule size, electrical potentials (Burr and Northrop, 1935), rates of respiration (Child), degrees of irritability. Perhaps no one of these qualities *per se* is the nature of the field, but the field is all of them taken together. Perhaps we can never hope to find one thing which is the physical basis of organic wholeness. This possibility, however, should not discourage anyone from seeking to discover the relations of the field factors to each other and to the processes of development.

It is of interest to enquire as to the extent to which the dynamic field forces—if such they be—persist into the adult life of the organism; or does the field subside as an active, functional field and require to be reawakened whenever regeneration takes place? Is the visible organism comparable to the bed of an actively flowing river (Child, 1915a, p. 27f), which is being continuously formed and molded by the forces of the moving water? Or is it the bed of a "dry river" which once flowed but is flowing no longer? Are the gradients which reveal themselves in rates of development and growth, to be taken as the direct evidence of an active morphogenetic field; or are they the relics of a field which once was present but is no more (cf. Huxley, 1935a)?

In the lower organisms it is fairly apparent that the active field persists through life, although possibly it is of less potency in the adult than in the embryo. Thus a hydra retains considerable powers of self-regulation. If a tentacle be grafted into the shaft of one of these animals, it will do one of several things (according to location): It may fall away. It may be resorbed. It may migrate into a proper position within the circle of tentacles. Or it may establish around itself a field of its own and cause an outgrowth or bud to develop (Rand, 1900; cf. Steinmann, 1927).

Similar regulatory processes occur in the embryos of higher forms. Thus if the limb bud of a salamander embryo be revolved into a new orientation, it will

commonly rotate back to its proper position (Nicholas, 1924). There is considerable evidence, however, that in the adults of higher animals, the fields subside to a very low ebb or disappear altogether. The various gradients and orientations which persist are probably evidences of a field that was and is no more. Thus the integumentary pattern which is revealed in the size, shape, and rates of growth of regenerating feathers of a bird, would seem to disclose only a "residual field" (Fraps and Juhn, 1936). The active field forces which gave rise to the pattern are gone.

This likelihood that the active fields of higher forms may deposit, as it were, various material and physiological gradients and then cease to be, brings us to an entirely different conception of organic wholeness from the one which we have just considered.

III

The second general concept of organic wholeness to which we call attention is the historic. According to this view the parts are unified in development, and present a definite pattern or configuration because they are derived from a common source (the egg) and have endured a common experience. Of this nature is the wholeness of the sky-rocket, which bursts into a symmetric pattern and fades away. Such also is the wholeness of a flower, which unfolds its characteristic pattern and dies. Any injury to the bud must result in imperfection in the flower.

The historic concept of wholeness gives us a clear picture of the life cycle. The germ cell or bud blossoms, as it were, into an embryo, and then progresses onward step by step through the stages of youth, maturity, and old age, to end finally in death or reduction. Viewed as history the processes of development are thus straight-forward and irreversible. If a protozoon be injured to any except a very minor degree, it does not directly heal its wound, but instead it wipes away its entire previous organization and begins a new development from the start (Taylor, 1935). (The wiping away or reduction is something quite different from a mere backtracking of the previous course of development.) If a defect be inflicted upon an egg or embryo, either one or the other of two things happens: If the defect occurs before development has structurized the part removed, then development continues normally. No reversal of history takes place, because no history has been made. The apparent regulation is not true regulation at all, for no differentiation is present to be regulated. If, however, the defect be inflicted after structurization has begun, the result is a more or less defective organism. (This is not to deny the possibility later of considerable "functional regulation" of the size and even of the positions of organs; nor of the remarkable processes in which specialized "embryonic" cells tear down and rebuild structure.) If a tadpole be fed thyroid it will transform itself into a frog, but the reverse transformation has never been observed. (There are characters, however, which remain plastic; for example, the secondary sexual characters in some of the higher organisms may even be reversed by changing the internal secretions of the gonads. Witschi, 1934.) Finally, an organism which grows old can apparently never become young again, except in so far as old differentiated cells may be removed and young embryonic cells take their place. It is thus possible to conclude, after making due allowance for numerous apparent exceptions, that life histories are predominantly straight-forward and irreversible "from the cradle to the grave."

The historic viewpoint raises the question as to what extent the straightforward, irreversible processes of ontogeny are predetermined? Or are they free? Is each individual organism actually something new? Or is it only the unfolding of something which was already present?

From ancient times to the present day biologists have generally rebelled against concepts of freedom. History there is, so they will agree, but it is not true history, creative and full of novelty. Organisms develop according to type. It has seemed necessary, therefore, on *a posteriori* grounds, to find the consequent in the antecedent; the organism in the egg. Let us review some of the ways in which a doctrine of freedom and novelty in development has been avoided.

(1) The early microscopists (of the 17th and early 18th centuries) not only denied novelty in development, but they denied development. With the aid of their imaginations they thought that they could actually see the organism preformed in miniature within the germ cell and needing but to grow. Some said that they saw it in the egg, some that it was present in the sperm. This is the doctrine of crude preformation.

(2) The pioneer embryologists of the late 18th and early 19th centuries, with better lenses and less vivid imaginations, saw that the organism is not preformed, but that it is at first simple, and that during development its structures come into existence *de novo*, one by one. This view is radical epigenesis. To escape the doctrine of novelty in development, however, these same embryologists postulated a vital force or soul. This was supposed to superintend development and render the result orderly. Thus they placed the problem of organic wholeness outside the bounds of science altogether.

(3) The last half of the 19th century brought attempts to substitute materialistic explanations for the vitalistic assumptions of a soul. The organism, so it was now thought, although not crudely preformed in the egg, was at least prerepresented in some fashion, part for part. Thus there appeared a series of hypotheses based on coteries of representative stuffs or particles.

Darwin (1875) proposed his hypothesis of cell-determining gemmules. Roux (1883a) had a doctrine of region-forming qualities. DeVries (1889) advanced his cytoplasm-forming pangenes; and Weismann (1893) his complex architecture of the germplasm, consisting of idants, ids, determinants, and biophores. The rediscovery of mendelian inheritance (about 1900) brought to the front the hypothesis of unit-character-determining genes. Related to these doctrines of representative particles were those of cytoplasmic inheritance. His (1874) proposed his pre-localized organ-forming germ regions; Nägeli (1884) his highly organized, net-like idioplasm. There were many other views of pro-morphology, of egg organization (Whitman, 1878), and of the egg as an "embryo-in-the-rough" (Loeb, 1916). The doctrine of organ-forming stuffs was developed (Conklin, 1905; Rabl, 1906); and special importance was assigned to certain regular aspects of the cleavage process (Conklin, 1898).

Each of these hypotheses, which we have just named over so rapidly, denied freedom to the organism, and any possibility of novelty to the normal processes of development. Each hypothesis was in fact a method of finding the organism in the egg. (This is, perhaps, not quite true of the doctrine of organ-forming stuffs, which lightly invokes processes of stratification, localization, or segregation of the stuffs, and so begs the question of the origin of organic wholeness.)

(4) Different in principle from the foregoing hypotheses are those which consider the organism to be the product of the interaction of numerous discrete entities which are *all alike*.

Of this type was Spencer's (1866) hypothesis of physiological molecules; Driesch's (1908) concept of a polarized intimate structure; and Przibram's (1929) space lattice or crystal theory. In each of these each smallest unit of the organism is in effect a microcosm which bears within itself the whole character of the species. These theories come very close to being gestalt views, and should perhaps have been considered under that heading, for they consider the developed organism as a sort of equilibrium in which the forces of the constituent units are satisfied. Moreover the view ascribes a controlling function to the whole; for, as Spencer says, the units, although all alike in structure, yet vary widely in their functioning, acting "at all times in such a manner as the whole demands." Yet the microcosmic theories, if they do not entirely rule out novelty, at least hold it to a minimum, for the organism is pre-represented in each and every one of the numerous units which compose it.

(5) The modern theory of the gene, although derived directly from the mendelian doctrine of representative particles (factors representing unit characters), has tended more and more to become a microcosmic hypothesis in which each nucleus possesses within itself the entire character of the species. Manifoldness exists from the beginning, but not in the form of numerous discrete units, as Mendel supposed; but rather as an organization of entities known as genes. At the times of cell divisions these are seen to be arranged in definite order in the chromosomes, which in turn compose the nucleus. Each gene, moreover, has not only its effect as a unit, but it has also a position effect, due to its relation to the other genes (Dobzhansky, 1936). In the opinion of most geneticists, however, there is but little opportunity during development for freedom of action, or for novelty. The organism, so to speak, already exists in microcosm within each nucleus. (This view was well stated by Wilson, 1905, p. 292.)

(6) From the geneticists we pass to the experimental embryologists. We may designate their usual outlook as the "modern theory of the potency." Quite unconsciously, for the most part, embryologists have supplied the organism in the egg, and thereby lost sight of novelty in development—by the mere use of a word.

Ever since Driesch in his analytical theory of development raised the question as to how the "prospective potency" of a part (that is, all the possible fates which a part might have) becomes restricted during development to its "prospective value" (that is, to the actual fate which the part does have), the usual formulation of the problem of development has been somewhat as follows:

At the beginning of development each portion of the egg possesses the *potencies* of becoming any structure of the embryo. During the course of development a segregation (Lillie, 1927, 1929) and restriction of *potencies* occurs, with the result that each portion retains the *potency* of becoming but one thing. It then proceeds to become that thing.

Now, the first difficulty with this formulation is that it uses the word *potency* in two senses, and so buries the essential problem of development completely out of sight. This point is so germane to our subject that it will be necessary to discuss it at some length.

Originally the related word *potentiality* meant "power" (puissance; Vermögen), but by a weakening of sense it has come to mean also "possibility, not actuality" (Möglichkeit). Similarly the word *potency* was originally and rightly used only in the sense of power. Webster's *New International Dictionary* defines it variously as "inherent strength or power"; "specific efficacy or capacity"; "ability to effect a certain result"; "capability for developing in accordance with its nature". Unfortunately, at least since Driesch's earlier writings, the word *potency* has been used by embryologists in the weakened sense of "possibility". Thus we read of the *potencies* of a cell for example. But the modern embryologist is quite apt to go further and to reify the concept; that is, he uses the word *potency* as if it stood for a thing (cf. Woodger, 1929).

With these definitions in mind, let us reformulate our statement of the problem

of development, substituting the words *possibility* and *power* as the sense may require. (Since power is also sometimes used in a weakened sense, let it be emphasized that it here always refers to inherent, specific efficacy or "self-differentiating" capacity.)

> At the beginning of development each portion of the egg possesses the *possibility* of becoming any structure of the embryo. During the course of development processes occur by which each portion loses the *possibility* of becoming anything, and acquires the *power* of becoming one thing. It then proceeds to become that thing.

Stated thus it appears that development is a dual process, involving on the one hand loss, on the other hand acquisition (cf. Vogt, 1927; Dürken, 1928; Raven, 1935). But let us examine further this idea of loss. What are lost are possibilities. (We shall allow for the moment the use of the plural.) Now possibilities are not real things. They are only possible courses of development which a given system as a whole may pursue. Moreover, we can know possibilities only by thinking backwards. We perform numerous experiments to find out, so far as we can, the many different things which a region can do and become. Then we turn our thoughts around and say that the region possesses the possibilities of becoming all of these things. In doing so we throw away the various inducing and interacting conditions which enabled the region to realize this, that, and the other fate. Nevertheless, forgetting what we have thrown away, we say that the region possesses the *potencies* of becoming these many things. No doubt this use of language has some descriptive value; but as a way of explaining the processes of development, it is, to say the least, misleading.

It would be equally misleading, however, to suppose that a sharp line can be drawn between possibilities and powers, or that a region becomes a sharply defined organ rudiment with a definite power suddenly. Powers come into existence gradually, and, moreover, they are at first diffuse in their distribution. It is possible, therefore, for a region to be in a state intermediate between possibility and power. The epidermis of the head of a young amphibian embryo, for example, possesses the "competence" (Waddington, 1932) of forming a lens, but (with some exceptions) it does not actually form a lens unless it comes into contact with an optic vesicle (future retina of the eye). "Competence" here refers to a sort of imperfectly developed power or latent potency which requires some further stimulus to "activate" it and render it effective. We shall have occasion to return to this topic in the next section.

By a shift in the meaning of the word *potency*, then, and by reifying the concept of potencies, many embryologists have quite lost sight of the fact that *powers must originate during development*, whether suddenly or gradually; and thus they have avoided the problems of freedom and novelty altogether.

(7) Let us now leave these several ways in which the concept of freedom has been avoided, and let us frankly face the possibility that development is true history; that each individual organism is not pre-represented in any sense within the egg; but that it is actually created afresh, step by step, as development progresses. We may then reformulate the problem of development as follows:

> At the beginning of development no portion of the egg possesses in itself the *power* (specific efficacy) of becoming any definite structure of the embryo. During the course of development processes occur by which the various portions acquire *de novo* definite *powers* to do and to become definite things. These things they then proceed to do and to become.

The processes referred to have been variously termed determinations, chemodifferentiations (Huxley, 1924), and invisible differentiations. Years ago Wilson (1894) called them physiological specializations.

He regarded "the specific structure of the cell to have arisen, not through the segregation and isolation within its boundaries of special idioplasts or germ substances, that have been sifted out by qualitative division, but through a physiological specialization (as de Vries and Hertwig insist) that may have taken place before, during, or after cell-division, according to circumstances."

The essential feature which we would emphasize here is that these determinations, invisible differentiations, or specializations, are novel events, which are free and in part fortuitous and unpredictable (although not, of course, indeterminate). Let us illustrate this matter by reference to the history of the amphibian egg.

Presumably there is a time in the very early (ovarian) history of the egg when the position of the animal pole has not been finally decided. However this may be, it is certainly true that while the egg is yet in the ovary the dorsal side is still uncertain (Roux, 1883 b; Fankhauser, 1930). At an early state, therefore, it cannot be said of any definite region of the egg that it is prospectively this or that structure of the embryo. Then processes of structurization occur. Hypothetically we may picture the egg-to-be as developing an intrinsic tendency to become polarized; that is, to form an eccentrically situated pole-plasm. Some force of the environment outside of the egg no doubt decides on which side of the egg the pole-plasm shall form. Next (in amphibian development this occurs soon after fertilization) a process takes place by which there develops on one meridian of the polarized egg a specialized dorsal-plasm. Thus the egg becomes bilaterally symmetrical. From the standpoint of the organism itself, both these processes are novel events which could not have been predicted, and in which something entirely new has been created.

With the appearance of these two "axes" (or rather specialized areas), each region of the egg may be thought of as acquiring a definite prospective value according to its location. But prospective value is not a real property which a part possesses. It is only the embryologist's expectation of the future fate of the part, an expectation which he has formed by thinking backwards along one particular course of development; namely, along that particular course which he has chosen to designate as "normal" development. A given region may thus be called "prospective mesoderm"; but it is still quite a fortuitous circumstance whether it becomes mesoderm. If perchance when the time of its invisible differentiation arrives, the region in question finds itself still on the external surface of the embryo (due, shall we say to some interference with the usual process of its gastrulation), the region will not acquire the power to become mesoderm, but instead may become epidermis (Töndury, 1936). Similarly the outer ectoderm of the gastrula either develops the power to become neural plate, or it does not develop this power. Whether it does or does not depends entirely upon whether it becomes underlain by some so-called inducing agent, normally by the archenteric roof (Marx, 1925; Holtfreter, 1934). It is of course true that nature with the help of natural selection has developed stabilizing mechanisms, for example, investing membranes, by which to preserve a fairly uniform environment outside and inside the egg or embryo. As a result development commonly slips along quite smoothly and pretty much according to a pattern.

Moreover, nature has apparently provided the developing system with various mutual interactions of a regulatory nature, so that, as Spemann has put it, the development of an orderly embryo is "doubly or even multiply assured" (Spemann and Geinitz, 1927). Yet in spite of these precautions, the natural occurrence of monstrosities is all too common; and the artificial production of abnormalities by the experimenting embryologist is almost no trick at all. Thus, we conclude, the development of an organism is not something that was prescribed in advance. On the contrary, it is in essence a program of creation which is free and full of novelty. (Freedom, of course, signifies lack of constraint, not absence of determinism.)

To the developing organism, then, every step is a new adventure over ground which it has never been over before, and over which it never will return. Every problem is for it a fresh problem, which it must meet in the spirit, as it were, of a pioneer. And if, as usually happens, it meets its problems in essentially the same fashion in which countless similar organisms have met their problems before, it is because it is made of the same stuff as they. No ready made charts are at hand to guide the organism on its way; no supervising genii, either æthereal or corporeal are present to make decisions for it. It is free; and it has no laws to guide it but the laws of the nature of its own being and of the place in which it dwells.

IV

We have now two concepts of organic wholeness before us: On the one hand we have the view that the organism is a configuration or gestalt of a particular sort; namely, a labile system tending toward a "dynamic equilibrium," and comparable, for example, to a candle flame. On the other hand, we have the view that it is of the nature of a true history, straight-forward and creative, to be compared to a sky-rocket which bursts into an expanding pattern and fades away. On the face of it these two viewpoints are incompatible and contrary, the one to the other. How can development be at the same time centripetal, the realization of a pattern of wholeness which was to come; and centrifugal, the achievement of a germ cell past? According to the first view time means little; and development may be backward (as in rejuvenation, so-called) as well as forward. According to the second view time means much; development gets somewhere.

Evolutionists and students of heredity have generally adopted the historic viewpoint. To them the organism is as a flower, borne on its ancestral tree, which blossoms and fades; but the stem, that is the germplasm, continues on immortal but not unchanging. Embryologists and students of the life cycle have likewise tended to stress the historic alternative; for they have observed the straightforward processes of invisible differentiation by which the egg becomes progressively structurized into a mosaic of parts having unlike capacities in development. They also have come to think of the organism much as a flower, which injured in the bud must ever show its injury. Students of regeneration and of the regulatory powers of the organism, on the other hand, have been impressed with the gestalt viewpoint; for they have seen the organism adjusting and adapting itself, and have observed its ability to attain the same specific end by more than one route (Berrill, 1935). With these phenomena in mind they have rejected the analogies of the tree and flower, and have pictured the organism more as plastic clay, any part of which is able, at least in principle, to live and reproduce its

kind. They have found continuity, not in a separate and immortal germplasm, but rather in the living system itself, one primary property of which is its power to assume and to maintain a specific and unitary organization or wholeness. How shall we resolve this basic and fundamental dilemma which lies at the heart of the biologist's doctrine of the organism? Any adequate answer must of necessity be based on well-planned experimentation and upon careful analysis. The argument which follows has been adapted from a recent article by the author (Gilchrist, 1937) entitled: "The scyphozoan polyp *Corymorpha palma* as gestalt and as history." (See also my analysis of budding and locomotion in the scyphistoma of *Aurelia*, Gilchrist, 1937.) However, as here presented, the argument refers to amphibian development, rather than to regeneration.

As a first assay at analysis, let us suppose that a human workman was to undertake to form an embryo from, let us say, a ball of clay. He would no doubt begin by measuring out the portions to be assigned to each part of the embryo. After this was done, he would commence to fold and mold. But before he could measure he would have to make some preliminary observations. First he would have to comprehend the material as a whole, for only then would he be in a position to choose where to begin, in what direction to measure, and the scale. He would need to choose the scale so that the embryo when finished would be proportional to the material available.

Does nature go about her work in this same methodic manner when she hews an embryo from an egg, or from a part of an egg, or perchance from two eggs fused together? Does she progress by these three steps when she structurizes a new limb from the blastema of an amputated appendage of a salamander, or a new flatworm from a fragment of an old flatworm? A. Does she first comprehend the material as a whole, choose the place to begin, the direction in which to progress, and the scale? B. Does she then measure out and assign to each part the rôle it is to play? C. Lastly, does she begin the process of actually molding the material into its final form and structure? Considerable evidence is at hand which indicates that nature does indeed proceed by these three steps.

Before we outline the evidence, however, it will be well to restate our analysis in as nearly as possible physical terms: The structurization of a region (in the first instance of the whole embryo or regenerate) may be considered to pass through three phases: A, a *phase of organization* (cf. Weiss, 1926) in which a simple, quantitative pattern or gradient-field is established within the region; B, a *phase of invisible differentiation*, in which qualitative differences arise within the region, and the parts acquire definite and diverse powers (specific efficacies) for development; and C, a *phase of visible differentiation*, in which the invisible powers which have thus arisen express themselves in different developmental activities; such as thickenings, thinnings, inpocketings, outpocketings, cell migrations, and tissue differentiations.

A. By organization a gradient-field originates which gives order and unity to the region undergoing structurization. In the case of an amphibian egg, the animal pole has usually been assumed to be the high end of the gradient (Child, 1924), the center of the field. However this may be at first, there is some evidence that in time the material which borders the denser yolk (corresponding to the margin of a chick blastoderm) becomes the "center" which at least stabilizes

the primary or polar gradient-field (Penners and Schleip, 1928). Next, one side of the marginal material (marginal presumptive entoderm) becomes the "center of organization" (Spemann, 1918) which determines the dorsal side of the embryo. If two such centers of organization should develop, as sometimes happens, or if two such centers should be experimentally produced (Spemann, 1919), then a double field will be organized, and a double embryo will result. The gradient-field thus establishes the polarity, bilaterality, and laterality of the embryo. The steepness of gradation within the field presumably decides the dimensions upon which the ensuing differentiations shall proceed, and hence the relative sizes of the organ rudiments. Smaller pieces produce smaller embryos. The gradient-field is at first highly labile and readily adapts itself to the form of the piece and the forces of the environment. For instance, during the first few minutes following fertilization, the dorsal side of the amphibian egg is subject to modification (Fankhauser, 1930). As soon, however, as any differentiation has begun, the specialized regions tend to stabilize and anchor the field against forces which might disturb it. Thus in the blastula the position of the early dorsal lip is established, although the locations of the remaining organ areas of the egg are still labile (cf. Gilchrist, 1935). Most important of all, the gradient-field has always the aspect of wholeness, and obeys the laws of fields, as already listed.

In short, the gradient-field is simple, quantitative, and labile. It gives unity, direction, and dimensions to the differentiations which follow. It has always the aspect of wholeness; and when it has become established, the parts within the field have definite prospective fates (provided, of course, that the further course of development be "normal"). The field thus gives significance to location, and special meaning to the concept of the "organism as a whole." If it were not for the process of organization, the differentiations which follow could not possibly give rise to integrated structure, but must produce a hodge-podge or heap.

B. By invisible differentiation qualitative diversity originates in spatial relation to the gradient-field, and imparts to the several parts of the field the powers to do and to become definite things in development. We may picture the process as a wave of chemical change, which, beginning at the center of the gradient-field, sweeps over the field, changing in its specific nature as it goes. First, we may suppose, a material is elaborated in the initial dorsal lip area which imparts to it the power to form the mouth region and the roof of the pharynx, and to induce in overlying ectoderm the capacity to form sensory and other ectodermal structures of the head. Lateral to this a zone of material is laid down which brings with it the power to form entodermal gill pouches, and to induce in adjacent mesoderm and ectoderm their corresponding visceral structures. Above the initial dorsal lip is an area which acquires, presumably by chemo-differentiation, the efficacy of becoming prechorda, notochord, and somites, and of inducing neural structures in any ectoderm with which it may later come into contact (Spemann and Mangold, 1924; Bautzmann, 1926). Thus, beginning at the initial dorsal lip and progressing outwards, the various developmental powers of the entoderm and chorda-mesoderm are established.

Even in this very brief account we have begged several vital questions. For example, it is likely that the invisible differentiations which proceed from the region of the dorsal lip of the early gastrula, impart merely the *power to move* (Gestaltungsbewegung) in

definite fashions during gastrulation (cf. Vogt, 1924). The powers or specific efficacies to differentiate into the various structures of the neurula, probably do not appear, or at least do not become fixed, until after the materials in question have passed beneath the lips of the blastopore (Detwiler, 1933).)

In principle, then, invisible differentiations are qualitative processes in which limited areas of the developing region acquire special powers of development which the same areas did not before possess. In general, the potential rudiments or "preprimordia" which thus arise are larger and more diffuse than the definitive organ rudiments which later become visible (Harrison, 1921b; Huxley and DeBeer, 1934, p. 237). They may even overlap one another.

C. In due time the potential organ rudiments undergo visible differentiation; that is, they express themselves in diverse developmental activities, and the embryo begins to take form. The central areas of the diffuse potential rudiments become the sharply delimited rudiments or "primordia" of the descriptive embryologist.

Now, the first processes of structurization; namely those by which the egg (or regenerating fragment) as a whole becomes subdivided ("autonomized," Weiss, 1926; cf. "elementary processes," Driesch, 1908) into a group of secondary regions, are followed by further structurizations within each sub-area. In general, it would appear that these secondary structurizations begin before the visible differentiations of the first structurizations are complete. However, in a formal way, we may still say that each structurization, primary, secondary, or of higher order, passes through the same three phases of organization, invisible differentiation, and visible differentiation.

With the appearance of diversity within the developing system, however, a new type of developmental activity begins. The differentiated regions and sub-regions act and react with one another by processes known as *embryonic induction* (see Weiss, 1935), with the result that the embryo becomes increasingly manifold—yet orderly. These various inductions, however, (for example, the well known induction by which chorda-mesoderm induces ectoderm in contact with it to become nervous tissue) are to be carefully distinguished from localized invisible differentiations within a gradient-field. The first (induction) involves direct action, and is presumably of a more or less specific chemical nature. It has been termed "contact organization" (Huxley, 1935a) and "evocation" (Needham, Waddington, and Needham, 1934). The second (differentiation within a field) is indirect, and is in all likelihood dynamic. It has been termed "distance organization" and "individuation." Again, inductions are partial processes which depend upon localized "formative stimuli." Field differentiations, on the other hand, are organismic or whole processes which take place in orderly patterns and in characteristic sequences. Examples of induction are the production of nervous tissue in ectoderm through stimulation by chorda-mesoderm, and the production of a lens in epidermis through the stimulus of an optic cup. Examples of differentiation within a field are the structurization of chorda-mesoderm itself, and the structurization of a limb bud.

The problem has been extensively discussed as to whether embryonic inductions are "activations," in which the inducing agent merely stimulates and releases competencies already present but latent in the responding material; or are they "instructions" (to use Weiss', 1935, term), in which the agent also controls, to a greater or less extent, the specific nature of the response. There is evidence to

support both viewpoints. Embryonic inductions appear to be, to varying degrees, mutual affairs involving specific action on the part of both the "actor" and the "reactor." Huxley (1935b) has anyalzed activation further into "local activation," in which the stimulus is some local chemical influence, and "distance activation," in which the agent is a chemical messenger or hormone. The latter may either migrate by diffusion, or be transported in body fluids.

It should be pointed out, however, that in all examples of embryonic induction, a state is involved on the part of the responding material which is intermediate between the possession of a mere possibility and the possession of a definite power or efficacy. Some degree of "competence" must of necessity be present in the material acted upon, some incomplete state of determination, if the inducing agent is to have an effect. Now, this "competence" or imperfect determination is itself of epigenetic origin, having developed through processes of structurization (differentiation within a field; organization followed by invisible differentiation) in the manner which we have described. The inducing agent acts only to perfect a determination already present, to release a competence, to "activate" a "potency."

Now what are the criteria of the several phases of structurization?

A. The tests of *organization* are the properties of the gradient-field, as already listed. A useful method of studying the field is that of the environmental differential. A thermal gradient, for example, may be made to pull a field strongly in one direction (toward the warmed side) and so reveal the extent to which the field is plastic and free, and the extent to which it is anchored in differentiated materials (Gilchrist, 1935).

B. The test of invisible differentiation is the capacity of a part to self-differentiate as a part. Now, of course, there may exist, as we have already hinted, varying degrees of this capacity; and in general the power for self-differentiation increases with time (Gilchrist, 1933). At first differentiations are dependent both for their origin and for their maintenance upon the gradient-field. If the field be modified or destroyed, the pattern of differentiation is altered, reversed, or wiped out altogether. For this reason "weak" experiments, such as minor injuries or defects to the developing egg which do not greatly alter the field, are able to reveal capacities for self-differentiation from the earliest stages of invisible differentiation. But gradually the potential rudiments become more and more stable, until in time only the very "strongest" experiments, e.g. explantations and transplantations, are able to wipe out or modify the field and so alter the course of differentiation. In short, the capacity for self-differentiation as revealed by weak experiments is evidence that invisible differentiation has begun; regulation following strong methods of experimentation is evidence that it is still in progress.

Invisible differentiation may be present to varying degrees in space as well as in time. The central area of a potential rudiment is commonly a "center of intensity" (Spemann; Lehmann, 1928), in which the process of differentiation has progressed furthest and has become most stable. Around this center there is a bordering zone in which invisible differentiation is still reversible. Around this in turn, there may be a larger area which by invisible differentiation has become "competent" to produce the organ in question, but only when sufficiently stimulated. Thus, to take a single example, the limb bud of an amphibian embryo contains a

"central limb disc" which is invisibly differentiated to form a limb, and is, in fact, more or less irreversibly differentiated to do so (Hollinshead, 1932). About this inner area is a zone (the outer regions of the limb bud) which has not gone so far in invisible differentiation. Any part of this region may form a limb, or a part of a limb, or it may not form a limb at all. What it does depends upon the position in which it is placed (Harrison, 1918; Swett, 1927). Finally, outside of the limb bud, there is an extensive territory which includes most of the flank of the embryo, which normally will not form a limb even when a piece of the territory is transplanted to the limb area; but if a spot within this territory be sufficiently stimulated, as for example by implanting next to it a developing ear vesicle or even a foreign object, it may respond by producing a limb (Balinsky, 1933). This competence of flank mesoderm to produce supernumerary limbs when sufficiently stimulated persists long after the embryonic stages are past (Guyénot and Schotté, 1926).

C. The test of visible differentiation is the appearance of more or less sharply defined and delimited areas of morphogenetic activity.

How, by way of conclusion, does this analysis of structurization relate itself to our problem as to whether organic wholeness is gestalt or history? Very simply. *Organization is gestalt.* The gradient-field is labile and self-adjusting like a candle flame. It has always the aspect of wholeness, and it ever tends to an equilibrium within itself and with its surroundings. *Invisible differentiations*, on the other hand, *are history*. They are straight-forward and creative processes. Like the sky-rocket they burst into a pattern and finally fade away. Like a flower bud they unfold into a blossom, and an injury to the bud results in imperfection in the blossom. The organism is both gestalt and history because in the process of its making it undergoes both organization and differentiation.

As students of regeneration we have been concerned largely with processes of organization. We have cut the living material into various patterns and have exposed it to the environment in various ways in order to see how far it will adjust itself to the unnatural conditions. As our reward we have witnessed the plastic flame of the gradient-field adapting itself to form and forces. As experimenting embryologists we have observed for the most part the processes of invisible differentiation. We have taken material already in process of development and have operated upon it. As a result we have seen the increasing evidence of its specialization, and have been impressed with the straight-forward and irreversible nature of the ontogenetic process. As students of the life cycle we also have witnessed progressive changes in the structural pattern. Finally, as students of genetics and evolution, we have studied, for the most part, the more superficial variations which are related to visible differentiation. As a result we have lost sight of the more fundamental problems of localization in time and space which are involved in organization and in invisible differentiation (cf. Lillie, 1927).

Now, both viewpoints, the gestalt and the historic, are but abstractions from the total process of development. The organism is neither pure gestalt nor true history, because it is both. It is first of all a self-regulating system or gestalt, because within its substance there lives a simple, labile, and unified field or flame. It is secondly irreversible history, because within this field localized processes of differentiation take place, and an ever-increasing manifoldness and stability are created.

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THE SEVENTEEN YEAR CICADA, ALIAS LOCUST

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INTRODUCTION

THE cicadas, mistakenly called locusts, are preëminently noisemakers so that a famous naturalist, more than a century ago, called them not cicadas but stridulantes and cantatrices. It was they who invented their forerunner of the Claxon horn, a stiff membrane to be knicked back and forth—quite a different noise-maker from that possessed by any other animal.

When the song birds depart and the silence of winter falls upon us we long for their return even though the morning songs may awaken us too early; but few who recently experienced the day-long racket of the periodical cicada, or locust, will regret that they must wait some seventeen years before hearing them here again in full cry.

All over the warmer regions of the earth there are more than a thousand kinds of cicadas; in New Zealand, Australia, Asia, South and North America and in southern Europe, where, along the shores of the Mediterranean they formed part of the daily lives of the Greeks and Romans. Everywhere the noises made are different for the different kinds, and everywhere man's names and ideas associated with these insects have stressed the sounds that envelop them as a halo.

Much curious lore bearing on the sounds of cicadas is to be found stored up in a book of Myers upon these insect singers. Again it was the sounds of the cicadas that impressed Professor T. H. Huxley when as a very young naturalist he visited Rio, Brazil, in February 1847, for he wrote—"There are two things that forcibly strike anyone going into the country here, first the enormous number, variety and beauty of the butterflies and second the noise of the cicadas. These fellows are as big as a man's thumb and fly about the trees like locusts. They emit a very loud continuous note, and when numbers of them are together it is really deafening. Singularly enough they seem to agree to short intervals of silence, then the note begins, at first weakly, then swelling into a grand chorus and gradually dying away again."

Yet while noise is the essence of the cicada as part of the web in man's history, the cicada after all, is phenomenally quiet through most of its life, since all these kinds of cicadas live underground one or more years and limit their utterances to the few brief months of their life in the sunshine before they died of old age, having left concealed eggs to live after them. Even at the peak of garrulous old age only half of the cicadas are to be heard, since only the males specialize in this rejoicing after the long silent life underground. This was long ago expressed by the poet of classical times:

> "Happy the cicadas' lives
> Since they all have silent wives."

On the other hand the real locust is a sort of grasshopper that makes contact with human kind not so much by any noise it may make as by its ability to devour plants that man needs. A long subterranean life is foreign to the real locust, which leaves the earth as soon as

it hatches from the egg and spends the rest of its life up in the sunshine.

The two are built upon quite different models; the locust with its long slender body with head hanging like that of a horse and powerful jaws to nibble leaves: the cicada of triangular outline, with wings as sides and head as broad base bearing the eyes astonishingly far apart, so that the face resembles that of its gnome-like relatives the "treehoppers." This cicada shows no biting jaws but only a sucking beak, like a true bug, able only to puncture and suck. The locust has often proved a serious threat to man's subsistence: the cicada has chiefly proved an interesting annoyance. That both may congregate in great numbers is the chief resemblance between these two diverse insects. The span of life of our seventeen year cicadas being about one-fourth that of man, to whom has been allotted three score years and ten, few can say as did the contributor to *Insect Life* that he had heard the cicadas in one spot five successive periods of eruption, namely, 1817, 1834, 1851, 1868, 1885. Others there were who heard them there in 1902, 1919, 1936—and in the future?

Many there are who will be laid in the earth before these cicadas emerge again—let us hasten to record our contacts with the cicadas of this period. The following observations and considerations refer to an area of a couple of acres, as one of the many showing cicada phenomena in the north edge of Baltimore, Md. Here the cicadas still hold their own as did their ancestors when the occasional Indian was the only man to listen to them before the white man came to claim the cicada's land as his own.

THE LIFE UNDERGROUND

Knowledge of the life of the cicada under ground is replete with surmises and inferences, but direct observations are difficult and not numerous. The slender little white young are seen to enter the ground and seventeen years later the stout, inch-long cicadas, looking rather like peanuts on legs, are seen to come out of the earth and to transform into winged adults. Just what happens all those years? The entomologists in Washington planted the young, like seeds, and year by year dug up successive crops of growing cicadas. Evidently like other insects these underground creatures grow in steps, each time casting off the outer stiff cuticle to expand. Six such steps were found, each a few years apart.

In many insects the egg gives rise to creatures so unlike the adults that they seem rather like worms and we call them larvae, but in others the young have more resemblance to the adult and we call them nymphs; this latter is what we find in the cicada where all the young have legs and beaks much like those of the adults. Again many insects pass into a resting stage in which they may seem nearly dead—the pupa stage, from which the adult arises as if by a rebirth. In the cicada the pupa shows already outlined the wings it will have as adult. Of the six nymph stages dug from the earth four lack wing-pads and are mere larva-like nymphs while the next following two stages show wing-pads and are pupa-like nymphs. However these so-called pupa stages in the cicada are active workers, and by no means like the inert pupas of higher insects. In the first year under ground the young are not deep down but later they go to a depth of two feet and more. The nine year old nymphs we dug up in October 1928 were about one-half inch long and found at the depth of two feet.

Marlatt discovered that the successive young stages differ not only in size but in the details of their digging claws and, interestingly, the tips of these claw legs that are of more use in walking than in digging dwindled away at successive moults till about lost, but then, when the young was preparing to come forth to walk, that is in the two pupal stages, this tip came back again in style, along with the wing-pads and the distinct external sex organs that show on the pupa.

We find the insect enters the earth weighing but a third of a milligram when a bit over a sixteenth of an inch long. Eventually it comes forth weighing 1000

milligrams and well over one inch in length. Whence this 4000-fold increase in volume and 3000-fold increase in weight? We find these cicada nymphs close to roots and as they always have good sucking tubes they no doubt act like their relatives, the "treehoppers," "the plant lice" and the "scale insects" and suck juice from roots of trees and bushes. Under a maple tree they may be said to feast upon maple syrup. If like their above mentioned relatives these cicadas drink excessive sugar sap to gain some protein also, they may, like those other insects, pass off an excess of water, sugar and dextrin. That would be like the honey-dew of the above insects, which ants and other insects profit from.

But underground we know no insects to profit by such excess, if there be any discharged by the cicadas, and we can only guess as to its use. Apparently the cicada down below has neither friends nor foes and it must lead a life of a "shut-in" with earth and roots as the chief elements in its world. Its chief companion in the subsoil may be the resting earthworm in its tight coil of pink pulchritude. Day and night, even summer and winter, present to the cicada nymph no such contrasts as are forced upon animals above. This isolation has its advantages, living in a rather uniform temperature with food and water at hand, the cicada is free from the attacks of the innumerable insects, fishes, amphibia, reptiles, birds and mammals that decimate the hordes of insects in the waters, in the air, and on the surface of the earth. Even the voracious mining moal may rarely descend to the cicada level. How much does the cicada know of the winter? It may well cease to feed when shedding its shell every few years; but does it rest, or feed, during the winter? Some considerations lead one to assume that our cicadas may not feed in the winter. Thus while cicadas generally live in warm climates our seventeen year kind not only survive the northern winter but also live longer than others in warmer countries.

Moreover, there is a variety in the southern states that needs but thirteen years to attain maturity, as if gaining time when less hampered by cold winters. The above is one of the many questions waiting for experimental evidence. Another is, what does the cicada do when the earth is soaked with rain? Another relates to the possible journeys of cicadas underground. The young falling from the tips of branches should enter the ground near the tree, unless blown away by winds, but when they emerge from the earth in after years they may be far out beyond the tips of branches where only roots extend and so we may infer some journeyings under the earth in long years. Also we find them emerging under porches and roofs where they could not have fallen but must have arrived by migration, probably underground. But do they wander far, from one tree to another? What happens to them when their tree is cut down or killed? Experiments are needed here. If some wander others stay restricted to the same spots, for we found them emerging under the same tree in 1919 and 1936 and under other trees in 1885 and 1902. How long the cicadas may continue to attack one tree is yet to be determined but evidently in the course of time trees will die of old age before the cicada clan is wiped out, and so the cicadas that once occupied the land must have migrated from the trees of then to the trees of now. Their gradual spread over the country was likely by short flights and by creeping on earth rather than by any subterranean migrations. Hence to assume with a Baltimore poet that these cicadas are doomed to wander in the earth

from Baltimore back to the scene of Pharaoh's plagues is an interesting extension of the common misbelief that these Cicadas of Baltimore derive from the locusts that vexed Pharaoh.

"Must thy doomed race impelled by mandate stern,
Traverse the earth's interior and return?"

THE SHAFTS

Digging for cicadas we may find them each within a small mud cell but we fail to see a system of levels and drifts like the borings of many insects in wood. We assume the works are largely horizontal passages of the nature of "stopes" as being soon filled in and not kept open.

However, there comes a time when the miner changes his low life and seeks to ascend to the air above. Now begins the shaft that each and every cicada makes for itself as a means of escape from the dark earth to the realm of light. Each is slowly dug out as a wide well from a foot down up close to the surface where it remains closed for a long time. The cicada is seen working in April of the year it will emerge but in some cases we thought it was working even two or three years before it emerged. This shaft is a work of great labor and more elaborated than a mere passage-way by which to escape. The creature is now in the pupa stage and its activities are aimed toward the final transformation into the winged adult. The upper part will serve as a resting chamber for the pupa till emergence is in order. But there are two other facts about the shaft that deserve notice. One is, that rarely does a shaft connect with another, though often so close together and thus there come to be very many parallel shafts, as in the work of shipworms in wood, and here also we infer the worker must have some method of finding out where the neighboring shafts are situated and this same method may be of use to the cicada when it comes near the surface of the earth and is not yet ready to break through the last thin layer that separates its shaft end from the air above. The second fact is that emphasized by that phenomenal naturalist, Fabre: namely, that the cicada begins the shaft below and ends it near the surface without there being any evident material thrown out or visibly stored away. So many cubic inches of earth excavated to make a shaft, but no earth dump to be found!

While so many earth burrowing animals begin at the surface and throw out the earth, or even carry it away and scatter it so that the burrow is not so easily detected, the cicada begins below, it may be in the dense subsoil where there is no space to store the excavated earth. Fabre explains the mystery in the case of the cicadas he studied in France by assuming that they wet the earth and by force push it back through the walls, so that, we assume, the fine suspension would fit in between the particles and displace the air. An examination of the shafts here in Maryland shows some reason for accepting some such explanation. The shaft has a lining of about one-eighth inch thickness that grades off into the outer earth but is internally fine earth showing flow marks as if deposited when very soft. These walls keep intact for months after the cicada has left and stand as finger wide holes firmly lined. Where the shaft is in subsoil the lining is subsoil and where in topsoil it is topsoil, so that we infer the lining is made locally and not of material carried far. The cicada nymph is very moist when dug out and seemingly full of liquid that might well furnish the needed moisture to the walls. Fabre supposes each cicada has its root from which to take moisture and to which it may return as the shaft advances. He also surmises

the moisture taken in is run through the digestive tract and the surplus used in irrigating. Snodgrass put some of our shaft building cicadas into glass tubes and saw them picking the earth loose with their claws, raking it together, grasping it in their claws and shoving it into the walls of the shaft—but this was in loose earth.

Strange to say when the cicada comes near the surface of the earth it stops abruptly and rests for many days before breaking through to the air. During that time the top of the shaft ends in a carefully made arched dome with a thin layer of the natural earth surface above it. Thus the shaft is not visible at all, yet bluejays and other birds now search for the inmates and one wonders if sometimes the inmate may peek out before emerging and thus see and be seen. The inmate may be resting just beneath the domed end but it is hard to capture since, though a pupa, it is very keen to escape, crawling down, or dropping down, its shaft when disturbed.

In April the sun heats the surface of the earth and the cicada resting for weeks just below may well experience more warmth than it has had for very many years and this may be of great consequence. When the shaft happens to come up under a board or other opaque object the cicada often turns its shaft and makes a tunnel along horizontally till it comes to the edge of the obstacle. Here there is careful building of walls though the roof of the tunnel may be in part the overhanging board.

Another form of this extension of the shaft is frequently seen in dense shade as under hedges, porches, buildings, low branched shrubbery, overturned boxes and high thick grass; regions where it is dark and also where the sun does not shine to warm the surface. This shaft extension is the chimney or turret so much like that made by the crayfish, but smaller. The chimney is over an inch wide and up to five inches high, made of soft mud applied in particles and capped above with mud that inside is the usual dome that ends the shaft, for the cavity of the chimney is but the extension of the shaft above ground. In places these chimneys are exceedingly numerous and as they are often where protected from weather they last for months as little unbaked clay tubes, closed at the end till the cicada comes out there.

It is to be emphasized that generally these chimneys stand out as being made of light colored subsoil and not of topsoil and thus they were made of material brought from below where in April the water table may be high. When in 1902 we put shaft builders in glass receptacles with earth we saw them bring wet earth from below in the following strange fashion. The head of the cicada as nymph is like a wedge as if to penetrate the earth, or something like the "cowcatcher" of an old fashioned locomotive. These captive cicadas had mud plastered on the right and left cheek of this face when they struggled up the shaft to then wipe off the mud, with their large clumsy claws. Thus little hodfulls of mud were carried up to build the chimney, but here the mud was carried on the outside of the hod-like head.

In 1919 Snodgrass added to our meager knowledge of the cicada shafts by pouring plaster of Paris down the shafts and so discovered that all the lower parts were filled in with dark refuse while only the top of the shaft was kept open and of use. In fact many shafts were but little chambers close to the surface, though others went down six inches as patent holes somewhat enlarged both above and below, so that the inmate had opportunity to rest either close to the surface of the earth or else down some inches in the earth.

But it would be an error to assume from these findings of Snodgrass that all the

cicadas' shafts as antechambers to the upper world, are restricted to the upper six inches of soil. In the first place when the earth is dry and hard in September the holes left by the cicadas are still patent and by simply sounding them with straw or twig we find at once that they go down nearly straight, often but three inches but often more than six inches, eight being common and nine found here and there, while two had a clear passageway for ten inches and one for eighteen. These measurements were made under apple, pear, cherry, beech, mulberry, maple, oak, tulip, sassafras, arbor vitae and dogwood trees and under old English box, California privet and Japanese barberry bushes.

Now when we poured in plaster of Paris and excavated we found that some of the shafts went to a depth of fifteen inches, as an extreme. The casts showed the slender neck at the surface where the cicada broke through the roof but did not show any special enlargement of the shaft just beneath the surface as the shaft was about of uniform diameter from just beneath the surface to the lower end.

The bottoms of the shafts, as casts, were enlarged and the little chamber so formed was generally to one side, so that the end of the cast looked like a leg ending with a club foot. Noticeably the bottoms o- these end chambers were flat and horif zontal so that the cicada might be supposed to stand resting there at ease before climbing up to investigate surface conditions. A cluster of such casts carefully washed free of soil presents somewhat the appearance of a group of stalactites of cylindrical form, but sinuous, and often ending below in an irregular enlargement and then rising about forty-five degrees for a few inches before becoming more nearly perpendicular. Some are smaller and some larger and though often very close together they do not touch one another.

In this case all the casts were in the topsoil, here about a foot deep, and when the casts and soil were removed the hard clay subsoil did not show any continuation of the shafts downward. Only here and there a few earthworm burrows lined by dark earth led down into the compacted subsoil. While the finer roots ran about amidst the casts the larger roots were chiefly below the bottoms of the casts. However there was no evident association or contact between roots and casts.

While digging the cast on September 7, we found several remains of cicadas, in four by four feet of topsoil; cicadas that as pupae did not succeed in emerging from below. Also two living pupae were found still down in the earth: like Rip Van Winkles that awoke so late!

These shafts seem the work of the cicadas in their last year or years under the earth; what excavations were made before and how the shaft was begun are samples of the many questions to be answered in some future.

The pupa stage in many insects is so inert as to need special protection by some case or cavity and these open tops of the shafts supply the cicadas with the protection of a pupal case, while also leaving these active pupae the chance to emerge when ready. In these chambers the cicadas are at the mercy of rooting hogs and many fall a prey to birds if they are not quick enough to fall down to their lower retreats. In seeking for a compensating advantage in these carefully made chambers we conceive that the chamber near the surface supplies the cicada with the needed moisture and temperature and modicum of protection to enable it to complete its "ripening" for the time of next shedding, to attain the final phase.

If it could be shown that the sunshine

as warmth is needed to "ripen" the cicada we would add one more to the attempts to explain the chimneys that arise where there is little sunshine; namely, that the cicada digs upward till attaining the right dryness and warmth, and if this is not found at the surface, as when under a board or in dark shade, then proceeds to build up with carried mud, as seeking the warmth and dryness that will quiet its digging tension.

The shaft with its upper antechamber to the world of light is the chief engineering and muscular labor of each cicada, as under-earth creature. The shaft is as wide as half the cicada's length and may have a length fifteen times the length of the cicada that made it. Compared with this, a man of five feet height would dig up from below, without allowing any earth to show, a shaft seventy-five feet long and two and a half feet wide. But such a man should be born a husky infant of five inches length able to dig himself down and stay down till before a few weeks of his death from old age!

EMERGENCE

In the cicada's calendar late May is the season for breaking away from the soil. If we are fortunate enough to be on hand, lying upon the earth, we see a remarkable sight. Where the earth and grass are unbroken there is here and there a faint movement and presently minute holes appear. From each, at length, protrudes the tip of the wedge-shaped head of a cicada and this is often muddy. Appearing and then vanishing it is slowly making a hole through the dome that ends its shaft-chamber above. The soil seems to be moistened. Presently the head with clean eyes and feelers sticks out a bit, and for the first time the eyes may see the new world above. The animal may twist ot one side or the other and may drop down again out of sight, but after some fifteen minutes struggle it manages to jam its body through the small opening it has made and scrambles out onto the earth's surface that it left seventeen years ago. The hole made in the dome is but just large enough for exit and the same is true of the holes seen at the tops of the chimneys so that we fancy the cicadas that emerge from the tops of these chimneys crawl, or fall, down from the tops of the chimneys after having had a chance to see the world from the chimney top. When first seen, and seeing, the cicada may be facing in any direction, but when once outside its hole the cicada seems forced to go in one direction only, namely straight toward the tree it has so long lived with.

The best time to view the emergence from the soil is when daylight is still strong, say between six and seven, for though very many will be popping up much later their exits are then difficult to see. Snodgrass records "At Somerset, in spite of closest scrutiny and long vigils with electric light and lanterns, we were never lucky enough to witness an emergence." However, when we clearly see one or another emerging here or there about a tree we may expect soon to see others peering out even an inch away from the one we are watching and presently it seems as if Gabriel's trumpet had opened up all the little graves round about for the dead to arise.

The emergence is by no means all in one night. Under one large pear tree some 5000 cicadas emerged during thirteen days, first very few, then more to a maximum and then fewer and fewer yet. Other trees showed the same waves of flow, increasing and waning, but trees near together did not ripen their crop on the same days exactly. Some hundred feet apart trees differed near a week and in regions a mile apart the differences were a week or more. In 1919 the emergence under a given tree began May 22 and ended June 4 and in 1936 the first emerged May 21 and the last soon after June 6. That is, under one tree the new generation emerged within the same two calendar weeks as did their parents. The daily emergence has some connection with the sun, it takes place as light fades in the gloaming and on to nine o'clock and later—very rarely in early morning hours.

We cannot suppose the fading light acts on the cicada within its chamber but the dwindling heat of

day may do so and thus give a signal. At all events there is some connection between emergence and temperature for in chilly nights of 40°F. the flow may near cease, to resume when normal temperatures return. However, even if the temperature is one of the signals that makes the cicada act, the chief background for this seasonal emergence is to be sought in the slow changes of seventeen years, leading up to a certain ripeness for response to the sun's behavior.

Just as there are exceptional individuals that emerge a little before the general rush-night and also a few belated ones; so there are some few cicadas that emerge a whole year ahead of or behind the general clan. Yet the unanimity of action on the part of these creatures is most remarkable. And yet each has no communication with its fellows. Each works for itself and emerges upon its own responsibility and not at the bidding of another. Examination and calculation show that where 5000 came from beneath one tree there remained just about as many holes; that is each makes its own hole. Evidently no reformer has convinced the cicada that it were better for a few (others) to build community shafts to be used in common, by those who did not work! At present each does his own work for himself. Hence the holes in the earth are very numerous and lasting on for months after the emergence of their makers, they form a new factor in the soil; as a means of modifying the in and outgo of air, the letting in of rain water, increasing dryness, furnishing shelter and other features for the very numerous little people that one sees wandering through the miniature jungle fashioned by the grass beneath the tree—that is ants, beetles, crickets and other insects along with spiders, mites, isopods, centipedes, snails and earthworms. That these holes may have important rôles to play is suggested by their numbers and long persistence.

Under and near an apple tree we made the following enumeration of these shafts used by the emerging cicadas a month ago. In an area four feet by four feet ten inches were counted 601 holes, a quarter to a half-inch in diameter; about thirty to the square foot. But the distribution was far from uniform; there was a space of six by six in which were no holes while again holes were so crowded that there was but an inch or even half-inch from center to center of closely adjacent holes. Scarcely any were found to open in common and these were separate just below the surface.

Another area of four feet by eight feet two inches yielded 834 holes, or an average of 24 to the square foot. But this included a bare space of ten to twelve inches with no holes, but also in it, close to the tree, where the holes were very large, up to five-eights of an inch, they were crowded to a density of 55 and even 61 to the square foot. Thus may the ground be converted into a veritable sieve by the strenuous work of these creatures seeking escape from below.

This astonishing riddling of the earth is one of the marked aspects of cicada phenomena and early attracted the attention of the white man who knew nothing of the like kind in Europe. Thus in an account made in Virginia in 1675, as recorded in *Insect Life*, vol. 2, pp. 161–2, we read of insects like swarms of flies about an inch long and big as the top of a man's little finger rising out of "spigot holes" in the earth, to eat the new sprouted leaves from the tips of the trees, without other harm: "and in a month left us."

TREK TO THE TREE

The long strenuous labors of the cicadas making shafts is not as easily seen as wondered at, but once the cicadas emerge anyone standing near may easily see another wonder, though it may not be readily understood.

Standing in the region of the emergence we are enveloped with the wave of migration to the tree. Far from the tree cicadas come crawling as fast as they can toward the tree trunk, at first scattered and few, but ever more crowded as they gain recruits and coming into the smaller circles near the tree trunk they finally reach and climb up. On the trunk the noise made by the innumerable feet of the climbing hordes as they grasp the bark is plainly

heard, like the pattering of rain drops. The obstinate persistence of each cicada in making for the tree is most striking: pick it up and tumble it about however much, it does not become confused, but resumes its solemn march to the tree. Take one from the north of the tree where it was going south, and place it south of the tree, then it goes now north, always toward the tree. All about the tree each goes on its own radius, but taken to any other radius it proceeds along that one just as well. It seems as if each cicada were pulled to the tree, as iron to a magnet. The following experience illustrates this strikingly. We stand pointing at the tree with a cicada on our hand and the cicada walks toward our fingers and so continues as we walk all about the tree, aiming itself at the tree as we point at it: but let us face about, with back to the tree, and then the cicada turns through 180 degrees and walks away from our fingers and up our arm; ever toward the tree which is now behind us.

If we watch an area between two large trees we see some go toward the one tree and others toward the other tree as if each knew its own tree. Here again each persists in its own direction no matter how picked up and reversed. What may be the way in which the tree serves as center of attraction for these many cicadas just out of the earth? Is it possible that in seventeen years of experience the cicada gains and holds a sense of where the roots center at the trunk? A simple experiment weakens this supposition. Take cicadas from their home tree, that is the one they have lived with so long, and carry them far away to some tree they have presumably had no knowledge of and they go toward this new tree though they may be going farther away from their own home tree.

These plump cicada nymphs are but clumsy walkers, but each seems most intent upon hurrying as fast as possible, in dead earnest, to get to the tree in time, without however any reference to the others. While to be sure, when hundreds march in open formation toward the tree in droves there is some resemblance to the common action of a swarm of bees entering a new hive, yet here there is no suggestion of these cicadas being led by odors or sounds made by their fellows. That it is really the large red eyes that aid the cicada in finding the tree is a supposition readily made and easily proved to be true. While the cicadas still continue to show good orientation to the tree even when faint light lasts on in the evening, in the darkness of midnight we find them wandering about and going nowhere. However such late walkers are unusual and it might be thought that they were not normal. Cicadas emerging under a maple tree in the midst of dense growths of periwinkle plants, even though they had chimneys, wandered and seldom found the tree, as if they could not see through the dense mass of leaves.

Better indication of the use of the eyes is the following. Cicadas placed on the bottom of an overturned wooded box some five inches high and three feet long and wide, walked across the box toward the distant tree they had not dealt with before and coming to the edge fell off and went on in the grass. When, however, a bushel basket was placed over the cicadas on the box they no longer oriented to the tree but walked off to all sides of the basket. Now human eyes beneath the basket found it possible to see some branches, but not the whole tree, through some slits in the basket, but the eyes of the cicadas failed to be influenced by these part views.

Convincing evidence that the eyes served the cicadas to reach the tree was had when we painted thick red watercolor paint over their eyes as they were marching for the tree. It may be remarked that the insect eye is covered with a hard, inert, shell so that paint on it is like paint on a human finger nail, causing no suffering, unless to the onlooker. Such blinded cicadas ceased to go toward the tree but wandered and went nowhere in particular, but when the paint was

washed off they went again toward the tree with no evident lasting effect of the paint. When only one eye was covered the cicada was still able to orient to the tree very well. On top of the head cicadas have three little simple eyes and whether these were painted, or not painted, seemed to make no difference in the march toward the tree; and this is in harmony with the common impression that such simple eyes are of use only in connection with objects near at hand. If then, the tree enters the cicada through its eyes what do we suppose the tree looks like to the cicada—is it like the tree of the botanist, or of the artist, or what? The distant tree and the tree overhead should seem different, the one being a mass, light or dark, against the evening sky and the other a shadow about a trunk that is darker or lighter. That these cicadas do react to a tree very distant as compared to the one inch length of the insect, was observed in 1919 when they went toward a tall tree seventy feet away, proceeding both upon the grass and upon a table that was turned about to demonstrate the persistence of the cicada in going toward the tree, however often reversed. In 1936 cicadas were seen to start off for a large tree distant 150 feet, under the following circumstances. A piece of plowed ground, 100 by 200 feet surrounded by trees and amongst them one very large tulip poplar in the southwest corner, had in its middle a strip 12 by 40 feet raked and rolled for the cicadas to walk on. At each end of this smooth area there was set up, to represent a tree trunk, a furnace pipe, a foot wide and four feet high, covered with burlap. The one at the north end of the area bore a superstructure of black cloth four feet wide and six high to suggest something of the effect of foliage of a tree above the trunk. The cicadas taken emerging or else already crawling up trees, were taken hundreds of feet to this area and there dumped down in the middle, by fifties and by hundreds.

In several trials May 26 and 27 most all the cicadas walked directly in the direction of the far distant tree and at first few paid any attention to the imitation tree trunks, though so near, but many having started the long trek to the distant tree turned aside and went to the tree stump image, reached it, and climbed it, though others lost themselves in the plowed ground. Again when dumped upon a piece of compo board four by eight feet in size the cicadas at once started for that distant tree and when then the board was turned 180 degrees, with all the cicadas on it, they at once faced about to continue on toward that tree. Then the compo board was set upon end so as to screen the tree from human eye close to the earth, yet when the cicadas were dumped three feet from the board they again went toward the tree and toward the screen that hid it. However, the moon now shone and the board appeared as a shade and this may have influenced the cicadas to move toward it. Strange to say when this board was set up to the north near the tree with imitation foliage and the cicadas dumped three feet in front of it they again went toward the board though this was now bright in the moonlight and the imitation tree was darker. Here, however, we are dealing with responses to nearby objects, and light and shadow near at hand belong with the problem of how the cicada reacts to nearby trees they have actually reached. To simulate such a close-up tree there was set up in place of the above two effigies, May 31 and June 1, one similar trunk surmounted by a horizontal lattice of wood, four by eight feet, and covered with compo board so as to cast a dense shade all about the imitation trunk. When cicadas were put near the edge of this shade many of them paid no attention to it, but marched off toward the big tree 150 feet away. This is similar to what happened in natural conditions when cicadas, emerging under a hedge, left the shade to cross bright grass to reach a distant tree. However other results were got in the rather good daylight from six to seven before the moon was bright, for then often many cicadas preferred the nearby imitation tree to the far distant one, went along all radii to the trunk under the horizontal slats, reached the trunk, climbed up and then out on the slats to transform so successfully that in the morning many shells were found attached to both "trunk" and "branches" as in a real tree, though here were no leaves.

Apparently the intensity of the light is an important factor in orientation. When responding to a real tree cicadas pass from the lighted field into the shadows of the tree and there is then presented to them the trunk, which they reach as if they saw it, for when a large tree was illuminated on one side by a strong street-lamp, cicadas on the light side went to the bright trunk and cicadas on the dark side went to the dark trunk, though in the one case the trunk was bright against a dark background and in the other dark against a bright background.

This same ability to proceed toward a tree whether dark or light is seen when they react to distant trees, that is when they are placed between two large trees each twenty or more feet distant; some go

toward one tree and others toward the other tree, though the one is bright against the darker eastern sky and the other dark against the sunset sky. We infer the tree is to the cicada at least a contrast to the background.

If cicadas are proceeding toward a tree and we turn them clockwise through 180 degrees, each cicada turns itself counter clockwise; and the reverse. This may imply the movement of some sort of image in the eye of the cicada.

The tree influences the cicada through its eyes in such a complex way that we are prone to infer that the tree exists in the cicada somewhat as it does through the human eye; but what the experiments actually show is that the cicada responds to the tree as an area of greater or less light contrasted with the background.

TRANSFORMATION

The long years of slow creeping come to an end up in the tree when transformation sends each cicada forth as a winged creature. The horizontal journey to the tree trunk is followed by adept vertical climbing whether the tree trunk be smooth or rough, but when branches are met there is again horizontal progress, but now upside down along the under face of the branch. Having walked centripetally on the earth the cicada now walks centrifugally from the trunk outward and may attain the leaves. Here a striking change of direction often takes place for the cicada faces about to point toward the base of the leaf, and so again rather toward the trunk. Shedding seems to have some internal drive like parturition in mammals. When the time comes each cicada stops short, may be on the trunk, on the branch or on the leaf, and anchors its claws carefully, to remain as if petrified. This may happen prematurely down in the grass, but this is not the ideal place sought for so eagerly. On the earth the cicada marched toward the tree trunk so intently there seemed dire need for it to overcome all obstacles. If it encountered a man lying down, it surmounted him and walked on over him to the tree, but if the man were standing up beneath the tree the cicada might climb up him and even transform fastened to his clothing, without reaching the tree. Again when a fence was put up close to the tree trunk to keep the cicadas off, this was but of little avail as the cicadas climbed the fence and though some did transform on it, others went to the tree. There was found, however, a sure means of keeping the cicadas from the trunk of the tree, namely by surrounding the trunk with sheet tin, or some other smooth surface, that the cicadas could not attach their claws to. Despite the strength of the urge to reach the tree many are overcome on the way by the other urge, that to climb. Thus in 1919 cicadas walking 60 feet toward a tall tree were arrested near it by chickenwire net and tall orchard grass stalks and climbing up transformed without ever touching a tree. In the same spot in 1936 the cicadas did not reach the tree but finding the tall grass-stalks managed to climb up and having accomplished this very difficult stunt found themselves, like the boy upon the masthead on "old ironsides," unable to come down. Yet in this most unusual condition they managed to anchor themselves and actually transformed waving on the slender stalks. This substitution of grass for tree does not mean that the cicada did not see the tree but only that, not being an instrument of only one string, the urge to climb came to expression out of its usual time—that following the reaching of the tree.

As we have seen the cicada uses its eyes to find the tree but after that climbing

and subsequent transformation may be carried through without use of the eyes, for when we covered the eyes of cicada nymphs walking toward the tree with black asphalt they were lost and did not find the tree, but when we put them to the base of the tree they took hold, climbed up and out on branches and in the night transformed from blinded nymphs to flying adults, leaving behind as witness their old brown shells which were translucent except for the two blackened horny spectacles that had kept the light out of the eyes of the marching nymph.

Such brown cast-off shells remain everywhere in cicada years as witness of the transformations that took place and when found months afterwards still fixed, in sheltered places, are well worth study as they show not only the exact form and size of the nymph that crawled, but also when moistened reveal something of the marvels of the escape of the winged creature from its nymph case. For within the brown husk we see the long white threads that were the linings of the breathing tubes, also part of the lining of the intestine then cast off, also the large tendons to which muscles were attached, both in the chest and in the claws; as in the shell of a common crab.

The transformation itself is but the last of the half dozen sheddings the creature has experienced in its long life but as it launches the stumbling crawler into the air as a creature of strong and beautiful wings it is an astonishing transformation. As the cicada stands anchored, a crack begins to open up on the back near the head and reveals the white "undies" the creature has been wearing. In its inner white suit the creature swells and bulges more and more out of its old brown suit. Oozing as it were out of the rent, the animal shows more and more of its soft whiteness. There are from the first, however, two striking squarish black areas on the back—later after longer exposure to the air much of the white will turn black—but these two areas are black to begin with. Fancifully they may be compared to the large "false eyes" seen upon some insects and suggesting a possible use as frightening away enemies; however, a real explanation for the early blackening of these two spots is yet to be found. Before long the animal manages to pull its head out of the old shell; at least the normal cicada does so, but some poor wretches never accomplish this and perish. The head bears the large colored eyes wide apart. Against the light we note that the hind end of the old brown shell is empty and we infer that some of the pressure that forces the animal out through the back of its old coat arises from the creeping motions of the tail end drawing itself away more and more from its encasement. After the head the legs pull out and the body stands out so that only the tail end remains sticking within the old shell, holding on, we surmise, to the large tendons in the thorax or chest, that will remain inside the cast-off shell. All the while the animal seems dead and as if forced out of its shell much as a crab swells out of its shell to become a soft crab. A strange thing is that the cicada stands upright in its old shell as if an esquimo were to stand up in a kayak, for the tail end has moved to the middle of the shell and most of the body sticks out into space. In fact we fear that the creature surely will fall since everything is upside down and nothing but the grip of the tail end prevents dropping to the ground. Thus standing bolt upright, we see them all about, perfectly still, with something of the gruesome suggestion of little shrouded corpses standing up, each in the middle of its coffin.

But watch! Each suddenly comes to life, bends its back strongly forward and with the new soft feet seizes hold of the old shell and moving forward along it pulls out its tail-end and is at last quite outside the old shell. The cicada once

out, walks forward from its old shell onto the tree and we now see an advantage in the position taken on leaves with the head toward the stem, as now the creature can walk straight ahead and not have to reverse, with danger of falling off, as would be the case if it had not cleverly turned about-face on the leaf before anchoring itself.

On the tree the new birth must remain quietly till its skin changes to black, brown and yellow in place of white and till it has pumped its wing pads full of air and let them dry. Then in the morning flight comes easy. Henceforth the cicadas know the ground no more, unless by accident thrown down, and then they revert to the old habit and progress along the ground to climb some tree from which to take off again in the air.

When the wings were still soft and there was no power to fly the cicadas when put upon the grass crawled along to the tree again and went up it as they had done before transforming. Many there are that fail to expand their wings properly and such cripples clutter the earth and exhaust themselves walking and climbing and falling.

LIFE ON THE TREE

While the tree serves well as the place for the transformation into the winged state this is by no means the final part it plays in the drama of cicada life. It serves also as mating place and as savings bank, into which are deposited the eggs, that alone secure the continuance of the cicada tribe after all these adults have passed away. Once endowed with wings the cicada is strong and active but does not choose to fly far from its home acres but remains within a few hundred feet of the region it long lived in and at this season there are few gales to drive it far afield.

Actually as it flits from branch to branch, day after day, the cicada traverses much space, but it does not migrate to new regions as may the true locust. We observe them fly far out from branches and then curving return to the same or nearby trees. At first they seldom alight upon buildings, but may circle about a chimney without alighting and then seek some tree where they generally alight upon the tips of branches and leaves.

Flitting about they have every appearance of seeing the trees they fly to and when we toss one up into the air it rises high and soon lights upon some tree as if selected from a distance.

Here the eyes are made use of as in crawling toward a tree.

Thus when we paint both eyes with asphalt and throw the cicada into the air it flies rather weakly and falls to earth, or else, may be chances to strike against the observer and then seizes hold, though blind. When all the little eyes on top of the cicada's head were blackened their flight seemed to be quite normal.

A very interesting sight, recalling the circling of pigeons, was furnished whenever we painted one of the large eyes, right or left, and tossed the cicada into the air. Then the insect rose in circles, first smaller then larger, that is in an increasing spiral. Remarkably soon, after sometimes but two, though usually five or six circles, the cicada recovered power to fly straight and flew to a tree, as if seeing it. In these circling flights the insect turned toward the side that was blinded. When the eye was not well covered flight was but modified in part. However, when the front half or the hind half of both eyes was covered, there was no flying in vertical circles and there was good direct flight to the tree.

Care was taken not to paint the feelers as when this is done the insect tries to clean them, as is habitual, with its front feet and when the feelers were stuck to the front of the face the insect still continued to make passes alternately with right and left foot, though not finding the feeler in its accustomed place. However, even when the feelers were thus plastered down to the face, the cicada did fly well and find trees as if normal.

The best exhibitions of flying were in the warm sunshine. As the cool of eve-

ning comes on cicadas fly less and when then taken and tossed into the air they do not rise but soon come to earth. Bright light and warmth favors, and darkness and cool retards, active flight. It is chiefly in the hot sunshine that the cicada is an active flyer, at night it returns to the crawling phase.

Much time is spent actively searching about, crawling upon the branches and leaves. On the tree they are devoted to four things; getting a drink, finding a mate, making a noise, or else laying eggs.

All this lasts but a few weeks then the flying ceases, many fall to the ground, are weak and die, even if not attacked by the strange fungus that fills the bodies of so many. In this region the duration of flying life for the cicada clan was, roughly, in 1919 the three weeks from May 28 to June 20 and in 1936 the four weeks from May 21 to June 23, but it was not determined just how long one individual remains capable of flying. The first mentioned use of the tree, that of supplying food or drink is the least important and has often been overlooked or regarded as nonexistent. However, this year there was abundant evidence that the cicadas did drink juice from trees; both males and females gained something from apple, pear, Washington thorn, rose of Sharon, and apparently basswood, tulip and pecan. At times, especially afternoons, we see dozens of cicadas standing on the bark of a small tree with heads all upward and may be in rows, seeming asleep but rather quickly breaking away when disturbed. Cautiously raising one from its crouching position with elbows down and beak shoved against the bark, we note that as the beak is pulled away the dark inner tools come out from far within through a definite hole in the bark. Also round about and below this hole the bark may be wet with juice that is sweet and attracts passing ants to lap it up. The amount of juice thus withdrawn is not known but in hot dry weather the active cicadas do not dry up but on the contrary they are full and have to spare, so that under a tree crowded with cicadas there is a little shower of droplets they give off and when they are disturbed they cast off liquid in jerks as if for protection; like to the protective discharges of some of their relatives, bugs that give out honey-dew to the benefit of ants and other insects.

SOUNDS AND MATING

The tree serves also for meeting, mating, song and egg-laying; in this first and last reunion of all the cicada clan.

When the cicadas have taken on wings it is evident that *about* every other one is a male, as easily recognized by the shape of the hind-body with its large hooks or claspers, and by the fact that only on the male do we find the organ of sound as two large drum-heads under the middle of the body. The female on the other hand lacks any sound organ and bears upon the hind body, beneath, the very evident boring apparatus for the piercing of wood and the laying of eggs.

When first flying there is no noise for several days, it may be four or five, and then we hear the distinctive sounds of these seventeen year "locusts," not to be confounded with anything else, difficult to describe or to recall in detail. When hundreds sing together the sound is phenomenal: some compare it to the noise of a fire engine siren, far away; or it may be likened to the distant rumbling noise of a freight train to which some singing of wheels is added. Heard early in the morning when robins are singing the cicada chorus suggests a mob of angry crows, yet it is not so harsh and has something musical in it, like the ringing of multitudes of sleigh bells all together. It is primal, and

quite unlike the buzzing and rasping of other insects, suggesting rather some wind instrument or even the vocal efforts of wolf or owl. The peculiarities of the sound are connected with the fact that it is not made by scraping hard surfaces together as are the late summer sounds we hear from crickets, ivory crickets, cone heads and katydids, but arises from a stiff membrane pulled out of place by powerful muscles and let spring back into place. The sound is reinforced by large air spaces and membranes that act as sounding boards.

The males give forth their normal cry* when upon a tree or some object and not when flying. As the sound issues the hind body is held up and we see the air spaces above the drums, but then the body falls and the sound ends with a fall in the note as the apparatus is closed up and no longer worked. The sounds last three to four seconds and are followed by a pause. To us the sound seems like e-e-e- or, we-we-we- or, better more of a growl, as ur-ur-ur- continued for about three seconds and then dropping abruptly at the end as ow, or wow, or row, or yow.

The characteristic abrupt drop at the end was expressed in the common belief that the insect said "Pha-r-r—aoh," being of course one of the locusts that threatened Pharaoh, long since. If this is the cicadas usual chant it has other sounds to make when frightened by seizure or when sitting quietly and dreamily whispering to itself.

As in other insects the summation of sounds of individuals makes indescribable complex totals when all the multitude is active at once. Often the chorus arises and falls but with no steady rhythm. When heavy rain falls all are silent and in the night time sound arises only at intervals in very warm weather. In the daytime it begins before the birds sing and lasts on to the cool of evening. In the early dawn it may start far away and approach as others take up the refrain, spreading along like the pattering of multitudinous drops as the shower spreads over the wooded mountain. Be it noted, however, that there is another song different from the above; in fact there are two songs and two kinds of seventeen year cicadas, associated together hereabouts, two species known to the entomologist as *Tibicina septendecim* (Linn.) and *Tibicina cassinii* (Fisher). The sounds made by the latter, the smaller, are more harsh and rasping, yet the organ of sound is of the same nature as in the larger, lighter-colored species.

The tune of the little black fellow is a few chirps and then a buzzing for three to four seconds: that is it seems to say, zig—zig—— z-z-z-z-. Thus singing we noted that many keep step with one another at times so that there was a chorus of great volume swelling for about four seconds and then followed by silence for about three seconds. This takes place in a sort of dance when the small ones congregate together on tips of twigs, sing together and then fly off together in silence—a short distance, to return again: and so on. In this dance they are silent on the wing but sing as they sit, and very many fly together and remain sitting together the same periods.

While both these two kinds of cicadas occur mixed together on trees and bushes, we often found more of the smaller sort congregating upon smaller trees and more of the larger sort upon the tops of larger trees, where however they could not be well seen. The waves of sound in the treetops, starting as early as three-thirty in the morning, were chiefly the utterances of the larger form, while the chorus singing in small trees in the heat of afternoon was the work of the small forms.

To connect the terrific amount of noise with the mating habits of these insects is not as easy as one might expect. In the first place it is difficult to find cicadas in the right phase, or stage, of maturity. To be sure, they are all about seventeen

years of age but they did not actually all emerge the same day but through a period of weeks, and the start some got may be kept by them: at least some sing before others do, some pair sooner than others, some lay eggs before others and some die sooner. They are all not doing exactly the same thing on any given day. This increases the difficulties of following the life of any one individual that may be in amongst others that are doing various things in their allotted cycle. Very many are singing and few mating. In general singing precedes mating; so we search for a sequence, but in vain.

In the cricket it is proven that the female walks toward the singing male till they meet and mate and we look for something of this sort in cicadas, but we are disappointed. When many males were kept in separate cages over twigs of trees there was no congregation of the other sex as if attracted. In the aerial dance about the twig ends, do the males somehow attract the females there and do the males flitting here and there search for the females? Mating takes place on the branches and lasts long so we easily see males and females that have met, always large sort with large and small sort with small, but when we watch males and females on the branches, though near together we see no indications that they are aware of one another. A silent male may some half-hour later be found pairing with the near-by female, or in other cases have flown away.

The sounds are heard in June so that every seventeen years this comes to us a noisy cicada month. Some begin before the crowd, and some linger on after the crowd is silent: while for a few days we may hear none at all, yet some last songster later surprises us. By a strange coincidence the last singer recorded in 1919 was heard June 28 and again in 1936, after a few days of silence, the last singer was heard on June 28!

We are told that as yet no organ of hearing has been discovered in the female cicada and the sounds we hear may play but small part in bringing the sexes together in pairs, as apposed to bringing them together in droves. The actions of males toward other males and females full of fungus suggests that contact-stimuli may be potent in actual mating. Awaiting better evidence for associating the great cicada chorus with mating we are thrown back upon other attempted explanations for this monstrous volume of sound. Fabre concluded that his French cicadas sung for "the joy of life." Meyers thinks that the community song serves a useful purpose for the species, though it has but a secondary value as aid toward mating. He points out that while the cicadas possess powerful wings and muscles yet they do not migrate like locusts but remain pretty much in the same spot, generation after generation, and the terrible racket the males keep up in home trees may help to keep the clan together and prevent stragglers from wandering away.

And we will add that the holding together of the clan in one small region may have somewhat of the same advantage as accompanies long life underground. That is, coming forth after their enemies have forgotten them, though they may be decimated by all the enemies at hand, yet more survive from the whole flock, than if they came forth every year, or if they came forth widely scattered amongst widespread enemies. The enemies do not concentrate in these spots of emergence and those there become satiated before the cicada army is annihilated.

THE EGG IS LAID

As the male is specialized to spend much muscular labor in singing (which we hope is some use to the race) so the female is specialized to spend much muscular labor in depositing eggs, of great use to the race. Usually for laying the females select the small branches of trees, or of large bushes, and crawl along the under sides. Only some aberrant cicadas lay eggs in the soft annual growths of such plants as tomatoes and dahlias though down in Mississippi cicadas were credited with laying eggs in blackberries and according to a note in *Insect Life*, 6, 1894, p. 378, those who refused to pick the berries said "Them singin locusses done pizen em with thir aigs."

On the trees the cicadas commonly work upon twigs of about the thickness of a lead pencil and often of last year's growth. When they use smaller twigs this frequently works disaster as the twig breaks off and hangs with dead leaves, or falls to earth so that oaks and many other trees long look as if the twigs had been scorched by fire. But the eggs laid in such small twigs that fall are found dried up and are not successful. On stouter twigs the female stands upside down and with the same sort of firm grip that was employed when anchored for transformation forcefully shoves against the overhead twig, just behind herself, the specialized set of boring tools that penetrate through the bark into the wood, however tough. On the right and on the left she drills, or rasps, or saws, two chambers that meet as a V with one common opening. As the chamber is made on either side it is filled with a number of long slender eggs slid down the drilling apparatus into place to stand close to its fellows, like sardines in a box. As the eggs are long spindles they look something like cigars in a box, but stand at 45 degrees, with what will be the head-end toward the common opening. Having made and filled one pair of chambers the female moves forward a bit and makes a second and a third and so on to finish with often a long series before walking elsewhere to resume laying. Each has several hundred eggs to lay and these are distributed here and there on other twigs. Other females coming by lay also and soon the eggs of various females and of various ages are found in long series of chevrons along the under face of the twigs. It requires strenuous work for the female to penetrate the wood of oak or maple or other trees and the very visible play of the muscles forms a "danse du ventre" that has a very practical value in the cicada life-history. Not only do the powerful tools penetrate the wood and implant the eggs but they so rasp up the fibres that these project out of the common opening of the two chambers as a veritable brush of woody fibres. Long rows of such brushes may be felt along the under sides of twigs, even when it is too dark to see that any cicadas had been there. The eggs laid, the mother soon passes out of life and leaves the eggs to develop by themselves.

THE EGG DEVELOPS AND THE YOUNG GO BACK TO EARTH

Two by two, side by side, with a dozen or so brother or sister eggs in a closed chamber guarded at the exit by a brush of fibers of wood, each egg is well protected and kept from drying by the moisture of passing sap. It will run through its development in the month of July, for as June is song month so is July development month for these insects.

It may be reiterated that the eggs are so placed that young will form within them without further care from the parents, in fact the parents are now all dead and the young will never see them nor did the parents ever see their potential offspring. As being so common and abundant no special study of all the details of cicada making in the egg has been published, but we may take for granted that the general rules followed in other insects hold here also. First the egg looking like a diminutive rice grain has the value of a single cell from the female, and secondly there was added to it the essential part of a cell of the male. From these two parental components are gradually made the thousands of cells that arise and contribute to the making of nerves, muscles, digestive organs and reproductive organs, male or female as the case may be, that are properly assembled to fashion a diminutive model of a cicada of this style.

In some weeks of time such little cicada

is fashioned within the egg shell, unless some accident befalls, as for instance, the intrusion of the egg of some parasitic ichneumon fly that will hatch and destroy the cicada egg, or the encroachment of the wood of the tree itself as it strives to heal over the wounds made by the laying cicada. In fact most of the trees injured by cicadas do grow new wood along the sides of the punctured areas and these side swellings do sometimes entomb the eggs, but generally the young escape before the wound heals over and the tuft of fibers seems a rather good thing for the cicada, as it sticks out and can be overgrown only with difficulty and long lapse of time.

When the young is ripe for hatching it must first break through its tough egg shell and then travel a bit to get to the exit, before it can walk on the outside of the under face of the twig. To see something of all this we take twigs from different trees and place them in milk bottles of water to keep the stems from drying out. Then we find that the hatching, like all other events of the crowd, takes place not all at once but strung out over some weeks time on different twigs and often upon one twig where several had laid. When hatching has recently taken place we note a peculiar addition to the region of the brush of wood-fibres that guards the entrance to each pair of egg chambers, that is, a tuft of faint white threads, as if some fungus. Under the lens this proves to be more like a dozen or so minute cups of glass upon long twisted white stems, in fact they rather suggest champagne glasses, with long crooked stems stuck together and without bases, fastened to the fibre brush at the exit; empty glasses left behind after the celebration of the escape of the young! Then digging into the wood we find the clear but tough leathery egg shells standing just as they were laid but now empty and each split open along its end toward the exit. Inferentially the young did move each its own length to get out of the long egg shell and then move some little distance more, to get to the exit and vanish, after leaving the empty "champagne glass." To find out how the young escape we cut open twigs that have not yet displayed the little glasses on threads and in various twigs find embryos not yet escaped and in various steps of escape.

Within its shell each embryo is so made that it is not free against the shell, but is enveloped in a sort of caul, or clear membrane, fitting over each limb all in one suit. The movements we see it make are not walking but creeping movements. The body is long and worm-like and the first movements seen are those of a creeping worm rather than of a creature that will do so much walking and finally fly. As the young nymph leaves its shell it is not free, but still within this inner suit. In this we see it slowly advance toward the exit of its chamber. We see waves of contraction from the tail end forward along the rings of the worm-like creature. Thus the tail is shortened and the head pushed forward, little by little, ring following after ring, till the entire animal is farther and farther from the egg shell. These crawlers from the egg shell will crawl even on glass, like maggots, to which humble creatures they have resemblance in their locomotion.

We assume that the creeping-like movements forced the narrow head of the creature against the egg shell till it ruptured and let the inmate, still in its inner casement, crawl slowly forth, like an inner tube bulging from a casing. Watching the nymph after it leaves the egg we see it shove its shovel-like head along between the wood and the overarching fibres and note that besides the worm-movements there are also some slight motions of the legs. But these are not yet free, but as it were in bags and can move through but a short arc, being bent down toward the body and never as yet raised far from the position they were built in along the under side of the body, like unto the blades of a pen knife when but slightly opened. Likely these confined legs serve to hold what is gained by creeping and prevent retrograde motion. Thus slowly creeping the nymph finally goes a few lengths and attains the exit opening along with some of its fellows and pushes out above the wood fibres. Its continued worm-like motions thrust its body out into the air as if it might fall from the twig. Hanging thus it makes violent bending of the body, bending down its head and rearing up its tail, swaying violently.

And lo! the case that impeded its motions breaks open at the back of the head, as will other nymph skins in later life as they shed and grow till the final transformation shedding. As the animal withdraws its tail end from this first larval skin this collapses and shrivels and as a shrunken tube looks like a mere crooked thread. In truth, the white strings, left sticking to the wood fibres at the exit are but the accumulating cast-off first larval skins of the young that hatched and crept to the common exit. Each string is hollow throughout as it was the covering of the long worm-shaped larva and from it stick out remnants of the tubular covers of the limbs, that could not be moved well till these were cast off. The head

part of the larval case remains open as the glassy cup and from this there sticks out, very often, a minute thread like a straw from a goblet, and this is hollow and as it comes up from the tail end of the larval skin we regard it as the cast off lining of the intestine comparable to what we find within the old husk the last nymph leaves on the tree when ready to expand its wings.

Greater magnification of these cast-off larval skins of the first stage shows that though they appear to be but crooked strings they present on the outsides of the rings that compose them many crosswise rows of fine projections, like the teeth of a rasp, and so pointed toward the tail end that they should aid the forward motion of the crawling creature in its first, but very important, journey in the world, and enable it to crawl even upon glass. Thus is the infant provided with a good anti-skid set of tires, for its first brief but difficult trip.

Returning again to the actual casting off of this glassy skin, we saw that when the head stuck out the feelers that had been built bent down along the face, sprang up and were long and well formed. Then the legs came out also into the air, requiring work as each is within a skimpy covering which tends to be pulled inside out. When once free the limbs are used, bent into place and stretched forth to anchor to any roughness, so that the animal walks out of its case and away from the exit. Though the limbs move with some trembling and tottering the infant does not pass from crawling through a stage of falling but walks as knowing how and coördinates the motions of its six legs.

The cast-off clothing is sticky and adheres to others and to the wood fibres till some puff of wind carries all away. To repeat, the creature made within the egg crawls out of its egg and crawls a bit of a journey to the exit, encased in swaddling clothes that hamper free motion, like tight skirts. Here it meets the light and violently bursts out of its temporary larval suit. As Fabre points out, free legs and walking would be difficult in the confined chamber and its encroaching fibres, while the insinuating crawling is better to bring the creature safely past the obstacles into the open where it can walk free with its wide spread legs.

In trying to picture the little one that takes its first steps we may think of it as like the old nymph, but also very much smaller, slenderized, pale and long drawn out, suggesting, perhaps, with its great claws, the wraith of some Lilliput lobster. The creature is all white except some small points of yellow-brown and the dark of the small eyes. Even within the egg and inside its first larval skin we saw the dark-red black of the eyes most clearly, eyes that now are (though compound) very simple, as compared with what they will gradually become as years go by, though their use seems non-existent most of the time. At that time also, within the egg shell, there were yellow-brown spines at the ends of the legs and on the claws and also fine brown bristles or hairs scattered over the body and held down flat till the larval skin was cast off, and then they jumped upright. The head is now rather a wide shovel and blunt and bears already a small sucking tube beneath it. The body still shows worm-like rings and the three of the chest are larger, especially the first that bears the big claws.

These cicada claws are fashioned by growing spines out from the second and the third parts of the leg, counting from the tip, so that the spines form a sort of pair of forceps for picking up objects and also for scratching or picking earth. This leaves the outermost part of the leg of less apparent use and Marlatt discovered in later years it will almost disappear. The animal stands high upon its other four legs and walks ahead like a quadruped but helped along by the pulling of its first heavy claws and their tips.

These active little fellows run restlessly about on the twig, and one was observed to stop as if trying to draw juice from the bark. Indoors they soon fall off and outdoors they cannot be found on the twigs. In this region the period of time through which cicadas are coming forth is about two weeks, that is, in 1919 they were hatching from August 8 to 23 and in 1936 from July 29 to August 14: this year's early beginning may have been due to hot weather. In other words, all the thousands of cicadas in this spot after seventeen years of concealment crowded their emergence into about two weeks of time and their offspring all came out

within about the same half-monthly lapse of time.

We wonder not only at the great stretch of time taken to finish the life cycle of these cicadas, but at the conservatism with which the cicada clan adheres to family traditions.

Excerpts from the diary of a youthful cicada of Baltimore might read "Our parents when children entered this land in the trek of August 8 to 23, 1919, worked hard at mining and the sugar business till 1936, when they migrated with the clan to this green paradise from May 21 to June 6 and here they sang and labored for our good a brief three weeks and then passed on, leaving us snugly cradled in the swaying boughs. Let us then so live that we may follow in their footsteps and celebrate those dates in 1953."

Looking for the young ones is like looking for the needle in the haystack: a few were found under objects upon the ground where they could not dig, but when kept in receptacles the young dropping from the twigs soon conceal themselves, if possible, and in 1919 we inferred that they were light-shy and crawled away from the light.

However, we now find as did Snodgrass then, that the young at hatching are positive to light under indoor conditions.

In a dish they walk strongly toward a window and reverse when the dish is turned about. This is as persistent a trend as is that of the nymph going toward a tree and here also when turned to the right it turns to its left and resumes its way toward the light. However, some few there were that walked away from the light and these seemed shorter and less white, as if older. When a lot of young, hatched less than a day, were put upon a black paper two and a half feet wide and five feet long toward the window, there followed a veritable cicada marathon toward the window. Some few finally strayed off the paper on the sides but most all went directly forward and as fast as they could walk toward the window, in open formation and not in files, each to itself. One soon getting a foot ahead of the others finished the five feet in eight minutes. Many did it in ten, some in twenty and a few had not finished in twenty-five, but these wandered. Three went only three feet and then wandered about. Two went away from the window light and when one of these was put near the window end it started directly away from it, but after going a foot in five minutes it wandered about. Those that finished were left walking in a dish for half an hour and then started over again. Most all the thirteen started direct for the window again though a few first wandered and then went toward the window. One that went away from the window was put near it and still walked from the light. Another was also negative to light. Of the eleven positive some finished in thirteen to twenty minutes but others changed to negative, at times, and wandered about. Seven that finished were tried again but they went off the sides or advanced with reversals and wanderings, except one that finished in twenty minutes, having gone fifteen feet plus the walking in the dish. Apparently it had been striving toward the light for some eight hours at the rate of twenty feet, or so, per hour; this should bring it somewhere.

So much for the performance of the walking cicada indoors. When we dumped a hundred that had been hatched some hours on the grass and earth beneath a tree we saw something more complex. These did not walk off toward the setting sun but scattered widely in different directions over the rough surface. Some fifty are seen wandering over about two inches square of soil amidst grass and soon all are vanished. Some walked a few inches and were lost in the grass, some came upon holes made by their parents and went in, where some climbed down the sides and others fell down—not to return for seventeen years, we assume. Here the young appear to profit by the work of the older generation, if they are thus led more readily to the roots they need. Two larvae were carried off bodily by ants, one of which was smaller than the cicada. But most of the vanished cicadas were still about where they were thrown down, but had managed to squeeze into crevices, or cracks or under lumps in the soil. Though the gait of these walk-

ing infant cicadas is rather tottering, as compared with the native occupants of the grass, yet we found they could actually dig in. One seen within a hole as wide as its outstretched limbs was rather staggering about, yet after a time it emerged and then disappeared again. With pocket lens it was seen to back out bearing in its big claws a particle of earth, then turning to one side thrust the burden from it and shoved it against the side of the little pit. While its motions were vague and weak as compared with those of an expert ant, its methods were those of a born miner. Others, put upon earth under a binocular microscope, were seen to dig in slowly but surely and so to vanish below.

In 1919 the Japanese student Yasushi Ibara stated that these young cicadas attach themselves to the roots of grass, but when we now try to see them do so we fail. Many were kept several days in glass receptacles with grass roots, but though the cicadas continued to scramble about amidst the grass roots we did not succeed in finding any that were attached to the roots or sucking juice from them. If it be the cicada's habit to feed upon grass roots on the way down, cicadas in suburban tracts where both trees and lawn abound would seem to have some advantage over the primitives of the woodlands. We do find some of the most populous assemblages of cicadas upon such tree strewn lawns.

DAMAGE DONE

"Before them there were no such locusts as they, neither after them shall be such,—they did eat every herb of the land and all the fruit of the trees—and there remained not any green thing in the trees, nor in the herbs of the field, through all the land of Egypt." *Exodus* X, 14–15.

Having once connected our seventeen year cicadas with the locusts of Pharaoh's plagues, we expect to find great damage done here, as in the orient. Yet, as a matter of fact, we find little of ill can be laid to the cicada hosts. The government maps show broods of seventeen and thirteen year cicada issuing almost every year from one part or another of the United States but there is no such wide spread continuing devastation as would call for large expenditures in attempted control.

All these broods are in restricted areas, there is no general outpouring over vast areas. In the maps of Marlatt are some thirty broods chiefly over the states to the east of the Mississippi River but with some few to the west, where other kinds of cicadas occur. Some of these broods occupy but a very small part of a state though others, like our present brood X, reach over parts of several states. But of course cicadas are no respecters of state lines and a brood appears over an area that has no boundaries that are understood as causes for limitation. While the thirteen year variety is more prevalent in warmer states they overlap the more northern variety and yet indicate some climatic factor involved in the distribution.

In general, history shows that these cicadas were more abundant in virgin forests and have receded like many wild things with the advance of cultivation. Yet some wooded areas still hold much of their former strength of population, as at Plymouth, Mass. where the brood of 1634, coming down through sixteen generations of seventeen years each, recurred in strength in 1906.

What damage is done is in these mapped areas and in them it is only the wooded parts that are affected, not the large regions of fields and crops. Moreover, not even all wooded areas or regions with trees are occupied by cicadas. When at its height the noise of the cicada is but

very local and one can easily escape it by short journeys in any direction. The regions of sound are scattered here and there amidst the larger regions of silence.

As we motor through Maryland months after the cicadas are gone we can still recognize infected areas by the dead twigs and leaves dangling from forest trees, or encumbering the ground as an added nuisance to the man who tends his lawns.

Were one to designate by red dots such infected areas in a state or county or district there might well result an instructive chart of strangely scattered and discontinuous distribution of these insects. Very large areas that seem fit for cicadas do not exhibit any and we are led to infer that there is no such ready means for cicadas to be distributed all through the land as there is for many a weed; more the pity.

The damage done is then very much a local affair; and it is not the destruction of trees but only their too severe pruning. The tips of the branches are injured, many leaves are killed. But these leaves had already served the tree for a period and the tree may spare them. The greatest loss is the foot or two of terminal twig with its many buds for next year's growth. The cicada prunes off some of this year's growth and so retards the tree. In our little fruit trees and smaller shrubs this may be of great importance, but it is as nothing compared with the damage done by many other kinds of insects that work in silence. What is of import is the fact that the twigs are damaged far below where they break off and far more injuries remain upon the living parts of the tree than are suggested by the shrivelled twigs. Inches, or feet, of the undersides of living twigs may show the scars resulting from the egg laying operations. Though the tree rapidly heals over the scars yet there is here but half a twig, the lower part being mostly dead. These are weak points and in storms, for years to come, the branches are prone to break off where the cicadas injured them. Long before healing over, various fungi, beetles and aphids gain access to the living parts of the tree through these wounds. Scars are recognizable even after the lapse of seventeen years, in some cases. Thus the cicada pruning continues to have results and the tree is hampered more than at first sight seems to be the case.

To be sure only half of the insect horde is guilty of wounding the trees, only the females of the species are to blame, yet the male shares in the guilt of taking sap from the trees. To our puny house plants the attacks of sucking insects, especially when they not only draw out sap but inject saliva, may be most serious, but the sucking of sap from vigorous tree or shrub in the open, whether by bird, aphis or cicada may be a venial sin.

Yet when we see a young pear tree with its bark wet with the sap wasted by twenty sucking cicadas, we wonder if the tree were not better without this hypodermic treatment. What shall we say of the long years of sap sucking done by cicadas on the roots of trees? Such stealing of sap cannot be of benefit to the trees and may be some harm, but evidence appears to be lacking. If the cicadas hold back the growth of trees by taking juice the years they are underground then the tree may grow better the year or two the cicadas are not feeding and are coming forth and it is even possible that careful study of the annual rings of old trees infected through several generations of cicadas might reveal some wave of periodicity approaching seventeen years.

At all events the cicada is not convicted of ill to the plants man most depends on and he need scarcely consider how to exterminate the cicada Let him rather

take steps to conserve broods in forest reservations for the wonderment of future generations, who else might not have so good a link with the forest life as the Indian saw it.

If in any region we desire to curb the injuries inflicted upon plants we like, and upon our jazz-vexed eardrums, emphasis should be laid upon the most vulnerable link in the chain of cicada activities, the period of emergence from the earth. Down below the cicada is safe from attack unless we venture to pour down carbon disulphid, as has been suggested, while up in the trees it would be expensive to reach them with poison spray. However, when the cicadas march along the earth to the tree, in the gloaming, then they are easily captured. And that is the period when the ancients deemed them a delicacy for the table. In fact they are then full of fat and are nutritious and to be compared with soft crabs. Yet we may relegate the eating to pigs, ducks and fowl and all our song birds, as well as squirrels and other cicada enemies. One drawback to this plan is that the common poultry go so early to rest that they miss much of the flow of emerging cicadas. However, bands of smooth material, as tin, about the trunks of trees will keep the cicadas down till they can be gathered for the creatures' breakfasts.

This food value of the cicada hosts may some day be made more use of. Should someone collect them and manufacture them into fish food for the aquariums, then the entrance of our cicadas into the land of the dodo, great auk and passenger pigeon would surely be hastened.

But let us rather endeavor to keep some of them with us as fellow travelers in this our common world of earth and air, mindful that the words of the poet apply not exclusively to the cicada:

> "Short thy allotted space of sunny hours
> Until thy issue sinks beneath the ground."
>
> Latrobe Weston, 1936

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A CRITIQUE OF PLANT SEROLOGY (*Concluded*)

PART III. PHYTOSEROLOGY IN MEDICINE AND GENERAL BIOLOGY. BIBLIOGRAPHY

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VI. CERTAIN MEDICAL ASPECTS OF PLANT SEROLOGY

SEROLOGY *of plants pathogenic to animals*. The pathogenic yeasts and lower fungi responsible for disease in man and other animals have frequently been the subject of serological investigations. In the limited space of the present paper it is not possible to consider all such investigations, and accordingly the following discussion is designed to afford only an illustrative and suggestive view of this phase of plant serology.

While the degree of immunity resulting from infection by pathogenic fungi is relatively low as compared with that of immunity to the bacteria and viruses, it appears that in some cases a significant acquirement of immunity does follow fungus infection or immunization. As early as 1896, Sanfelice (305, 306) showed that the guinea pig, dog, cat, and rabbit inoculated with attenuated *Saccharomyces neoformans* show a decided resistance to tumor formation caused by virulent strains of the same fungus. Michel in 1917 was able to immunize guinea pigs strongly against *Monilia psilosis* by injections of the killed fungus (224), and similarly *Actinomyces gypsoides* extracts produce an active immunity in the rabbit (252). In cases of *Trichophyton* (56) and *Monilia albicans* (177), however, no general immunity followed the inoculation of dead fungus, although in the former case a decrease in severity of symptoms was noted in the inoculated animals.

The property of conferring passive protection in the serum of animals vaccinated against or recovered from attacks of zoö-pathogenic fungi is apparently rarely met with. For example, Citron (56) has shown that, while precipitating sera may be obtained in rabbits inoculated with *Trichophyton*, these sera have neither protective nor curative value for mice inoculated with *Trichophyton*. Nelson and Henrici (252), however, claim that if rabbits be immunized with an endotoxin from *Actinomyces gypsoides* their sera partially protect guinea pigs against actinomycosis.

In contrast to these relatively weak or negative findings regarding immunity to the zoö-pathogenic fungi *in vivo*, numerous experiments have shown that the sera of animals inoculated with living or dead cultures of virulent or attenuate zoö-pathogenic fungi regularly exhibit demonstrable serological effects on the fungi concerned, *in vitro*.

Although a number of non-pathogenic yeasts had early been shown to yield precipitins of a rather low order of specificity (321–324), the precipitin test has not proven entirely satisfactory in all cases with the pathogenic yeast-like fungi.

Positive results were claimed by Citron in 1905 with a pathogenic *Trichophyton* (56), but the earlier attempts with the precipitin method often failed because of lack of a suitable test antigen (e.g., 224). In 1925 Balls (16) reported successful precipitin tests with pathogenic yeasts by the use of autolyzed yeast as a test antigen, and shortly afterward the development of the use of the water-soluble specific carbohydrate of pneumococcus led to an application of the same technique with pathogenic yeasts, particularly by Kurotchkin and his associates (166, 167, 178) who obtained high-titered precipitin reactions between the sera of inoculated animals or infected humans and purified preparations of the water-soluble specific carbohydrates of *Monilia tropicalis*, *Trichophyton violaceus*, *T. cerebriforme*, and *Saccharomyces* species. This work has been continued by Kesten and Mott (in 168a). Lamb and Lamb (168a), by the use of a precipitin absorption test, obtained clear cut results separating the species of *Monilia* tested into three well defined groups. Fermentation tests confirmed these serological results.

Agglutinins for non-pathogenic yeasts were observed as early as 1901 by Bissérié (30) and Schütze (321) in the serum of inoculated rabbits. In 1917 Michel found that in severe cases of sprue the *Monilia* causing the disease is agglutinated by the sera of infected patients (224). Similarly, fungus agglutinins have been shown to occur in the serum of rabbits inoculated with the pathogenic *Aspergillus fumigatoides* (307), *Monilia psilosis* (126), other pathogenic *Monilias* (207), *Saccharomyces neoformans* (192), and a strain of yeast inducing sore throat (270), as well as in the serum of a patient dying from infection with *Monilia pinoyi* (270). They were not found in normal human sera or in the sera of cancerous humans (37). While the titers of such agglutinins are not usually high, in some cases positive reactions have been reported with sera diluted 1:5120 (207). It seems evident from the work of Defalle (64) and others that it is the wall substance of the yeast cell and not the protoplast which acts as antigen.

Positive complement fixation reactions have been reported with the sera of laboratory animals inoculated with pathogenic species of *Monilia* (224, 167, 207, 178) and *Saccharomyces* (270, 306, 178), and with the sera of cases naturally infected with *Actinomyces* (252) and *Monilia* species (224, 83, 167). They were not obtained with normal or cancerous human sera (37) nor with sera of unprotected laboratory animals which subsequently died of yeast infection (306). The water-soluble specific carbohydrates of *Monilia* and *Saccharomyces* likewise give complement fixation reactions with immune sera, showing titers even as high as 1:25,000,000 (167, 178). On the whole, the complement fixation test seems the most satisfactory of all procedures yet used in the serological study of zoö-pathogenic fungi. Killed fungus extracts, fungus culture filtrates, and specific carbohydrate fractions have all been used with success as test antigens.

Certain other serological reactions have also been sought in the study of zoöpathogenic fungi. It seems evident from the works of Skchiwan (329), Citron (56), and Peckham (270) that the inoculation of pathogenic species of *Trichophyton* and *Saccharomyces* is quickly followed by phagocytosis of the fungus *in vivo*. The opsonic index shows an increase before either the precipitin or complement fixation reactions appear. In intraperitoneal inoculations of *Saccharomyces subcutaneous tumefaciens* into the rabbit the yeast responds by the production of a thick, resistant wall and is not killed; on the

other hand, intravenous inoculations result in the rapid destruction of the yeast. No lysis of peritoneally inoculated yeasts has been observed in the cases of *Trichophyton* and *Saccharomyces* species, and the serum of inoculated animals fails to exert any lethal effect on the growth of pathogenic species of *Saccharomyces* (193). Davidson, Gregory, and Birt (63) have described a diagnostic test for dermatomycotic fungi, the "trichophytin test." The "trichophytin" is a protein extract of *Trichophyton gypseum*. Of this .1 cc. is inoculated into the skin of the arm and readings are made after 24 and 48 hours. The test is claimed to be diagnostic of suppurative ringworm, since 15 definite cases gave positive tests, while 55 patients with other types of dermatomycosis all gave negative or very weakly positive tests. The serological mechanism is anaphylactic, analogous to the tuberculin test of cattle or to local skin tests in diagnosis of hay fever. Axamit (11) and Nakayama (247) have claimed that pathogenic fungi (a dermatomycotic Torula-like yeast and an *Actinomyces*, respectively) are anaphylactogenic. The reactions obtained were highly atypical for anaphylaxis, however, since if two doses of the fungus were given at 5–7 day intervals death ensued, while if the interval was three weeks (the customary interval for anaphylaxis) there was no reaction. Since the weight curves showed loss in weight during the first week after inoculation, which was subsequently recovered, it is probable that in this case the reactions observed were not anaphylactic but rather due to the cumulative effect of two sublethal doses of a toxin present in the inoculum. Rosenau and Anderson, however, have reported reliable anaphylaxis tests with yeast (297).

A question of particular interest in the study of the serology of the zoö-pathogenic fungi regards the specificity of the reactions obtained. On the whole, it may be said that there is little or no species-specificity apparent in the reactions noted above, and that even on a broader scale it is rather difficult to distinguish the pathogenic yeasts and yeast-like fungi with success. Thus the various species of *Trichophyton* have been shown to be very close to one another according to the precipitin test (56, 166), and the *Monilias* often cannot be separated from one another by the agglutination, complement fixation, or precipitin tests (126, 207, 178). The yeasts of sprue and other blastomycoses have been shown to be very closely related serologically (126). It has been possible to demonstrate a serological difference between the pathogenic and non-pathogenic yeasts, on the other hand (207), and between the non-acid-fast, pathogenic *Actinomyces bovis* and *A. Madurae*, and other, acid-fast, non-pathogenic *Actinomyces* species (252). Balls (16) has claimed an excellent differentiation of yeast sub-species by the precipitin test, but this finding has not been confirmed by other workers. A curious case of heterogenetic specificity has been observed in using the water-soluble specific carbohydrates of yeast in precipitin testing, where Sugg and Neill (344) have shown that anti-yeast serum not only precipitates in the presence of the carbohydrate of pneumococcus Type II, but that it protects mice against these pneumococci, in spite of the very wide systemic separation of the yeasts and the pneumocócci. This phenomenon is apparently to be explained in terms of the occurrence of a common, semi-antigenic, non-proteinaceous substance in the capsules of these very distinct organisms.

Hay fever, asthma bronchiale, urticaria, satinwood dermatitis. One of the most outstanding links between medicine and phytoserology concerns hay fever, asthma

bronchiale, and urticaria, all known to be due to anaphylactic reactions in man toward the proteins of many species of plants. The modern conceptions of the etiology of hay fever date back to the studies of Dunbar in the first decade of the present century (68, 69). Dunbar was able to reproduce the clinical picture of hay fever in susceptible patients by the administration of dried pollens of the *Gramineae* and of *Convallaria*. The reactive constituent of the pollen was found to be insoluble in alcohol, ether, and alcohol-ether, non-carbohydrate, highly sensitive to heat and to phenol, precipitable by ammonium sulfate and recoverable from such precipitate, and hence presumably protein in nature. Immunized animals yielded sera which neutralized the toxin efficiently. Dunbar further showed that precipitin and complement fixation reactions are demonstrable between the pollen extracts and the serum of hay fever patients, but not with the serum of normal individuals. Susceptibility may be passively transferred.

These findings of Dunbar have been repeatedly confirmed until the anaphylactic nature of hay fever and asthma are today accepted as proven facts. As practical applications of these findings may be cited the well-known skin treatments for the identification of the particular plant responsible for the condition in a given patient and the efficacy of the serum treatment once the sensitizing species of plant is known (57). Therapeutic treatment is successful in about 75 per cent of treated cases (186a).

In 1910 Wechselmann (360) described an erysipelas-like skin reaction in workers with the satinwood (*Fagara flava* = *Xanthoxylon cribrosum* of the Rutaceae) which he attributed to anaphylactic sensitization, since in non-sensitized workers an application of the wood caused only a minimal burning, while sensitized workmen showed a severe dermatitis a few hours after the wood powder was rubbed into the skin.

Many other plant substances are known to be responsible for allergic reactions naturally occurring in man. These include dried seaweed (*Zostera*), kapok, and jute used as stuffing materials, all grasses, flax seed, ipecacuanha, *Lycopodium*, violet root, *Camellia*, boxwood, rose, cocoa, teak, numerous fungi, coffee and tea, etc. Extensive data concerning these substances, as well as those of animal origin, together with their chemistry and biology, are given in Maass' collection of tables on allergy (186a).

Rhus dermatitis. The serology of the toxic element of *Rhus* and the therapeutic use of *Rhus* extracts in treatment of ivy poisoning have been discussed previously (page 188).

Determination of cause of death when due to plant toxins. The phytotoxalbumins discussed in a previous section not infrequently are responsible for accidental death in livestock, since a number of common weeds contain such toxins, and occasionally cases occur in which human poisoning is attributed to such plant toxins. It would, therefore, be of considerable value if it were possible to prove, in suspected cases, the cause of death.

In 1902 Ottolenghi (267) showed that ingested egg albumin may be detected serologically in the contents of the digestive tract. Schern in 1910 (314), on the basis of such findings, attempted to demonstrate serologically the presence of ricin in the stomach contents of poisoned stock, but unsuccessfully. The following year, however, Müller (243) contributed a technique which detects ricin in the stomach and small intestine after death due to this poison, although the protein had been so much broken down or absorbed by the time it reached the large intestine that it was no longer serologically recognizable. Hiki (125) has added

the instructive observation that plant proteins which are ingested retain their ability to react serologically even after passing into the blood, but that this serological reactivity is lost once the blood passes through the liver by way of the hepatic portal system. It would, therefore, seem evident that under favorable circumstances the diagnosis of death due to poisoning with plant toxins is possible by serological methods and the specificity of the reactions obtained (cf. Part II) would indicate the reliability of the results obtained.

VII. APPLICATION OF PHYTOSEROLOGY IN THE STUDY OF CERTAIN BASIC BIOLOGICAL PROBLEMS

Movement of proteins in plants. The possibility of the specific detection of plant proteins by serological methods opens a new approach to the study of protein metabolism in the living plant, and indeed the difficulties involved in the detection of proteins are such that it is doubtful whether any other approach yet devised could afford similar aid. Moritz in Kiel is chiefly responsible for the little work which has as yet been done in this field. In 1932 (237) he raised the question of whether plant proteins can migrate in the plant sufficiently to permit an immunological sensitization *in the plant* comparable to that which obtains in an immunized animal. It had previously been claimed by Kraus, Portheim, and Yamanouchi (159) that animal antibodies can penetrate the roots of uninjured bean plants. Moritz and vom Berg extended this work, watering *Vicia* plants with ovalbumin solutions and later testing the aerial parts of the plants for the presence of ovalbumin by means of the Schultz-Dale anaphylactic technique. The encouraging preliminary results of this first work were extended and confirmed in a later study (237) and it was shown that the penetration of ovalbumin into the plant and its passage up to the leaves may be demonstrated either by sensitizing the guinea pig with such leaf material and testing against pure ovalbumin or reciprocally by sensitizing the pig against pure ovalbumin and then testing against the leaves of ovalbumin-fed plants. While these results are contrary to a general opinion that substances of such large molecular size cannot freely pass through the living plant, Moritz and vom Berg's work gives every evidence of having been carefully planned and executed, and should be subjected to further investigation because of its importance in an interpretation of such biological problems as that of the physiological basis for immunological sensitization in the plant, and the debated question of the nutritional relationships between the members of symbiotic partnerships, in particular of the mycorrhizal symbioses.

In connection with the serological approach to the study of the movement of proteins in the living plant should be mentioned the work of Matsumoto and Somazawa (204, 205) in demonstrating the movement of tobacco mosaic virus in the plant by means of the precipitin test. By the use of these tests, the Japanese workers were able to follow the virus in its occupation of the tissues of the inoculated host, and to show that the advance of virus as indicated by the precipitin test is correlated with the advance of symptoms due to the virus. They have recently described a micro-method for facilitating this work (201c). Gratia (110) made the interesting observation that, while extracts of the pericarps of mosaic tobacco plants are reactive with tobacco mosaic serum, extracts of the seeds of the same plants are not, in accordance with the fact that tobacco mosaic is not seed-

transmissible. With tobacco mosaic virus it so happens that there are other methods for virus detection more sensitive than the precipitin test, and accordingly this particular case might be better attacked by the use of the local lesion method of Holmes; however, with other viruses in which convenient and relatively accurate infection methods of detecting virus are not available, the serological tests might be used to greater advantage in the study of the movement of virus in the inoculated host.

Investigation of hybrids. Another application of serological methods in fundamental botany is in the investigation of the constitution of hybrids. In 1914 Zade (385) first attempted such a study, and by means of the precipitin test he was able to show that, while *Trifolium pratense* and *T. repens* are related but serologically distinct, their hybrid, *T. hybridum*, reacts so strongly with both as to demonstrate clearly its hybrid nature. Furthermore, known mutants were shown to be serologically indistinguishable from their parent forms.

Moritz and vom Berg have made an extensive contribution in this field in recent years (232, 236–239). In a *Berberis* hybridization it was possible by means of the Schultz-Dale method to detect proteins in the hybrid peculiar to each parent.

Among the grains, in several hybridizations of *Triticum*, *Secale*, and *Aegilops* species, the hybrids were likewise shown to be serologically intermediate between the parents. In a *Triticum-Secale* cross, for example, *Triticum* partially saturated an anti-*Secale* system, and *Secale* partially saturated an anti-*Triticum* system, while the hybrid saturated both but not those of other species of *Triticum*. In a hybrid between *Aegilops* and *Triticum* the hybrid also fully saturated each parent, although the parents were serologically distinct. Finally, in the disputed hybrid *Vicia Leganyi* ("*Lens esculenta* x *Vicia sativa*"), the protein complex of the hybrid (represented as $a + b + d$) was shown to be intermediate between the protein complexes of the parents ($a + b + c$ and $b + d$, respectively).

The protein constitutions of the hybrids are hence found serologically to represent combinations of all or part of the proteins of the parents without the occurrence of specifically new proteins as a result of hybridization (238).

It is thus evident that in the field of genetics phytoserology may contribute instructive data, affording evidence of the genetic distribution of the specific protein, the chemical basis of life and of the secondary manifestations regularly used as criteria in genetical studies.

Contributions to the plant virus problems. The work of Matsumoto and Somazawa in following serologically the movement of virus in an inoculated plant (204, 205) has already been mentioned in another connection (p. 298). Serology has also given indications of affording help in a number of other ways in the problems of virus research. It has been pointed out (pp. 179–181) that instructive aid may be obtained by means of the precipitin, complement fixation, and neutralization reactions in identifying and in determining the systematic relationships of plant viruses.

A third contribution of serology to plant virus research is in the use of serological techniques in the quantitative determination of the amount of virus present in a given sample extract, a line of work developed in particular by Beale with reference to tobacco mosaic virus (282, 283). In Beale's experience, as in the writer's (51), it is possible under some conditions to arrive at a fairly accurate estimation of the relative amounts of virus in two comparable samples by means of the precipitin test, and the development of serology in this connection gives promise of affording useful evidence in the cases

of some viruses which have not as yet yielded to any other quantitative method.

Gratia and Manil (111) have made the valuable suggestion that the neutralization reaction might be used in the purification of virus mixtures. The most frequent and troublesome contaminants of virus materials are the highly resistant and infective viruses, such as tobacco mosaic and the latent virus of potato. Using customary techniques these can be removed from mixtures with less stable viruses only with great difficulty. Yet it is just these frequent offenders which are most highly antigenic and it seems reasonable to expect that the treatment of extracts of two mixed viruses with serum antagonistic to one of the components may yield pure inoculum for the other component. Indeed, recent preliminary experiments of the writer have shown that the serological approach affords a practical solution of this problem.

The specificity of the plant virus seric reactions (50, 281), and the fact that the earlier experiments showed a close correlation between virus infectivity and seric activity (51) led to an assumption that in these reactions it is the viruses themselves which are the antigens concerned. Very recently, however, it has been shown that preparations of the viruses of tobacco mosaic (332b) and latent potato mosaic (20a, 20b) may be treated with formaldehyde or other agents in such a way as to eliminate their infectivity without affecting their seric reactions. Two interpretations present themselves: (1) that the seric reactions are due to virus which is so affected by certain chemical agents that it loses its infective power without losing its specific antigenic character, or (2) that the reactions are due to a specific by-product of the virus, from which admixed virus is removed by the chemical treatments. Bacteria in culture may produce specific, filterable by-products, regardless of the culture medium.

The crystalline protein recently isolated from the juice of mosaic tobacco and tomato plants by Stanley (332, 332a) shows a high precipitin titer with virus-immune serum. This crystalline material appears to consist largely of virus-antigen, although it also contains protein of the virus-free tobacco plant (53). The writer has prepared what appears to be the same crystalline protein by precipitating virus-containing phlox juice with tobacco-virus antiserum and then digesting the precipitate with pepsin. The high yield of crystalline protein obtained indicates that the protein is either virus or specific virus by-product, since there are no serologically common proteins in healthy tobacco and phlox (53).

Although it appears from the foregoing facts that the crystalline tobacco mosaic protein and the antigen responsible for the virus-specific precipitin tests are the same thing, the question remains open whether or not this antigen is the virus responsible for the disease. From the practical standpoint, however, this problem does not seriously affect the utility of the virus seric reactions. The empirical evidence indicates that the virus antigens are specific for each type of virus disease and that usually, if not always, there is a direct relationship between amount of virus and amount of antigen in a given preparation. From this point of view, serology has served in the past, and probably has much to offer in the future, in the study of virus diseases of plants and of the nature and action of their causal agencies.

It is thus evident from these illustrations that phytoserology gives promise of being an instructive and valuable objective aid

in plant virus research, as in research on other types of fundamental biological problems.

VIII. THE POSSIBILITIES AND LIMITATIONS OF PHYTOSEROLOGY

It has been the purpose of the present paper thus far to describe the methods and results of serology as applied to plant materials, and to indicate the directions in which serological studies are proving of value in relation to other botanical disciplines. While, however, the results thus far obtained incline one to be rather optimistic as to the value of phytoserology, it must be borne in mind that the serological methods are beset by numerous difficulties both in practice and in interpretation. Great strides have been made in the understanding of serology since the early experiments of Kraus, Tschistovitch, Bordet, and Ehrlich at the end of the 19th century, but in spite of this much is yet to be learned of the principles of general serology. A number of questions of basic importance are unanswered or controversial. For 30 years it was held as unequivocal that proteins were the antigens *par excellence*, yet the recent works of Sachs, Heidelberger, and others tend to make it increasingly evident that lipoids and carbohydrates also have an important part to play in serological reactions. The site of antibody formation is unknown; the relative serological importance of various parts of the antigenic molecule is not wholly clear. Plant serology naturally looks to the older serology of animal and bacterial materials for guidance, and in many cases the older science is not yet able to supply adequate answers to the questions regarding the principles of general serology. The limitations of plant serology are, then, primarily the limitations of all serology, limitations inherent in the growth of any comparatively young science.

In addition, the plant serologist faces many technical and theoretical problems peculiar to plant materials and conditioned by the complexity of these. There are present in plants many substances which disrupt the performance of serological tests. Some of these, e.g. lipoids, are also impediments in animal serology; others, e.g. tannins, plant organic acids, plant alkaloids and glucosides, are peculiar to plants, and, as has been pointed out, these may produce artefact reactions of such intensity as seriously to interfere with the prosecution of serological tests. More and more stress has been laid on the elimination of these in recent years, as is witnessed by the studies of the Königsberg, Münster, and Kiel investigators. But at the present time one can proceed only with the utmost caution in regarding as significant serological reactions in which there is always the possibility of distorting artefact phenomena. Complete control systems must be rigidly adhered to, and experiments in which any of the controls are faulty must be rigorously discarded. Even with careful experimentation a large percentage of experiments may fall into this category.

One does not find in phytoserology a panacea for the troubles encountered as a result of the subjectivity of certain other botanical disciplines. But the handicaps of serology are of types which give promise of yielding more and more to the gradual improvements in technique and advances in interpretation. The only apparently insuperable difficulty lies in the probable inability to immunize animals with plant antigens which are so unstable that they break down in the experimental animal before there is time for them to reach the site of antibody

formation and stimulate the production of antibodies. Certain plant viruses, for example, are highly unstable at mammalian blood temperature, being destroyed in a few minutes at 40°C. Whether or not such may be eventually used with success as antigens remains to be seen. Possibly the substitution of a cold-blooded animal or the treatment of the antigen in such a way as to render it more stable without altering its specificity may serve to eliminate this difficulty. It is not unlikely that many other plant and animal proteins besides the viruses show a similar lack of stability, and remain hitherto undetected because of this fact. It is certain that, in the preparation of plant antigens by the extraction of dried tissues or expression of juices, there must inevitably result extensive and basic changes in the chemical composition of the products originally present in the plant, although future researches may aid materially in lessening these changes.

A question which has frequently arisen in the phytoserological literature is whether the plant proteins are more or less diverse, serologically, than the proteins of animal origin. It seems to the writer that this is a question of much less moment than would be expected from its discussion in the literature. Using blood serum as an antigen it is frequently possible to distinguish species of the same genus. The same is sometimes true of plant serology, using tissue extracts as antigens, but not always. Using such distinct species as many of the Solanaceae, separations are entirely practicable; with such closely related species as many of the fungi, on the other hand, the separation is accomplished with much more difficulty or not at all. This is by no means to be deplored,—in fact, this difference gives us an insight into the relative significance of the concept "species" in the two cases mentioned. Even with the fungi Link has shown that among some fungi, species and even varieties are readily distinguishable, while among other fungus groups it is difficult or impossible even to separate genera of the same family. Blakeslee and Gortner in 1913 (33) even reported that there were able to distinguish + and − strains of *Mucor* species by the precipitin test, although this has never been confirmed. Our natural classification, so-called, is at best an artificial system, the distinctions between species depending not so much on the phylogenetic distance of the forms concerned as on the *differences in morphological structure produced by a given phylogenetic trend.* It may very well be that varietal differences in the Solanaceae represent phylogenetic divergences fully as great as are intimated by specific differences in the genus *Fusarium.* If so, it is well to have serology as an arbitrary guide as to what constitutes a significant phylogenetic departure. An immediate objection to this view is that the protein differences as demonstrated by serological techniques are but a single phase of phylogenetic development. This is true, but there is reason to believe that protein divergences are at the basis of all morphological divergences. As Wells (362) has pointed out, the proteins represent the only chemical substances which could be responsible for the great diversity in life. It is by no means certain that serological reactions give a true picture of the relationships of the proteins responsible for specific, varietal, and individual differences in animals and plants, but it is self-evident that a study of protein relationships comes closer to the fundamental species-specific characteristics of species than the evolutionarily convergent morphological characteristics, such as flower structure and leaf dentition, on which our present systems of plant phylogeny are

based. Assuredly, if one could obtain a precise serological technique for the differentiation of the proteins of the genes, this would afford a more accurate approach to the question of phylogeny than any yet adduced. We cannot be sure how much if any of the serologically active protein of plant antigenic extracts is karyosomal in direct origin. It is significant that the weight of the evidence, however, favors the view that all tissues of a plant have serologically common properties, which would imply a fundamental unity of active material. It is not possible at the present time to do more than suggest the viewpoint that the serological reactions in plants are due in part at least, to materials closely related to or consisting of the karyoprotein, but such a view is favored by the experimental evidence in hand, and such affords excellent reason for regarding the serological reactions as fundamental in the study of the constitutions and relationships of plant species.

Granting such difficulties as those mentioned, the empirical results of phytoserology have shown that in spite of the handicaps the method has proven its usefulness as an adjunct discipline of very different character from those heretofore employed in botanical studies. Chief of its advantages are its objective nature and its fundamental character, objective in the sense that a serological test is positive or negative, opinion having little part to play in its prosecution and evaluation, fundamental in the sense that it is the proteins, the chemical basis of specific differences, which serve as the source of evidence. Because of these facts, it is concluded that in such studies as those of plant phylogeny and systematics, of protein metabolism, and of the detection and identification of plant products, the serological methods have already proven their usefulness and give promise of offering much aid in future investigations in these and related disciplines.

IX. RECAPITULATION

Part I

The present paper presents a digest of the information available regarding the serology of plants and plant products exclusive of bacteria. The techniques of phytoserological procedures are outlined, and then are considered the results of phytoserological investigations as applied in particular to the diagnosis of plants and plant products, the systematics of plants, plant toxicology, contributions of plant serology to the field of general serology and to certain aspects of medicine connected with plant pathogens and toxins, and such basic botanical problems as those of protein metabolism and movement in plants, the constitution of plant hybrids, and the nature and properties of plant viruses.

Rabbits are customarily used in the preparation of immune sera. Immunization- and test-antigens usually consist of solutions of dried, macerated plant tissues in physiological saline, water, phosphate buffers, or weak alkali. It has been found desirable to preëxtract such tissues preliminary to extraction, with ether, alcohol, benzol, acetone, etc., to remove non-specific precipitating substances. For certain purposes expressed saps, suspensions of whole or macerated plants, or purified protein or carbohydrate fractions may be used to advantage. Since many plants contain constituents which precipitate in the presence of normal serum, it is held desirable to eliminate such nonspecific reactions by thorough preëxtraction followed by an elimination of any residual normal serum reaction through dilution, absorption of the extracts with

normal serum, or the use of phosphate buffers. While in general all organs of a given plant contain common antigens, the differences in concentration of these and the possibility of the occurrence of organ-specific proteins makes it desirable to confine single experiments to a single type of organ. Chemical protein tests have often been found to bear so little relationship to the amount of serologically active protein present that their value in controlling serological reactions is very limited. A careful standardization of extraction technique is held to be more satisfactory than a subsequent adjustment of extracts to equal protein tests.

It is comparatively easy to immunize to plant toxins so that the animals will tolerate several or many lethal doses of the toxin. The blood of animals thus immunized *by feeding* does not yield precipitating, complement-fixing, or anaphylactic antibodies, however, hence the feeding of herbivorous animals should have little or no effect on the serological reactions of the blood of such animals after injection with plant antigens. Other details of the production of immune sera follow the techniques customarily employed in animal serology.

The artificial sera used by Mez and Ziegenspeck have failed to be satisfactory in the hands of numerous other responsible investigators. Although there is both theoretical and empirical basis for their use, they have not, at least thus far, proven sufficiently reliable to serve in routine phytoserological work.

The precipitin test, the most extensively used technique in phytoserology, may be desirably carried out either by overlaying antigen and serum (ring test) or by mixing the two (flocculation test). The latter test is somewhat more sensitive, the former more convenient. With plant antigens and especially in plant relationship studies, it is highly desirable to use an extensive and complete system of controls, preferably after the order of those used in the laboratory of Mez and described in Part I. Various methods have been used for the determination of strength of reaction, but it is felt that unless reactions are sufficiently sharp that essentially the same results are obtained by any approved method, the results are of doubtful significance.

The so-called "conglutinin reaction" of Mez, better designated as "Mez' reaction," represents a methodological variation of the precipitin reaction, perhaps somewhat more sensitive than the latter, and serving as a useful adjunct to the customary precipitin technique, although looked upon as somewhat less reliable because of the addition of another variable to an already complex system.

The anaphylaxis reaction in various forms has been applied to plant materials with considerable success. It is exceedingly delicate and specific in the form of the Schultz-Dale modification and in this form has yielded valuable evidence as to the antigenic compositions of plant extracts. Toxic plant materials may be studied by this method if the extracts are first dialyzed.

Many plant extracts, particularly plant toxins, are hemolytic, often powerfully so. Immunization frequently results in the production of anti-hemolysins in the blood. Lysis of particulate plant antigens by immune serum has not proven a satisfactory phytoserological procedure, nor has the Abderhalden lytic reaction, but the complement fixation test, which is described in detail, has been found to be the most delicate and specific of all immunological reactions employed in plant serology. Similarly, many plant extracts are hemagglutinating and their use as immunization antigens results in the

production of anti-agglutinative sera. Likewise normal serum not infrequently non-specifically agglutinates particulate plant antigens. The acquired agglutination of such antigens has proven of particular use in the study of unicellular algae and fungi or fungus spores. There is some evidence to support the view that this reaction is due to the antigenic actions of substances in the cell capsule, which may be relatively non-specific, and not to the protoplast itself.

The immunization of animals to plant toxins either by injection or *per os* usually results in a greater or less toleration of such toxins. After repeated injection the blood customarily contains neutralizing antibodies specific for the toxin, and passive protection is possible with such blood. Similarly, a number of the plant viruses if injected into animals induce the production of specific neutralizing antibodies, although the viruses are not pathogenic to the animals used. Other serological tests which have been used with plant antigens without, or with indifferent, success comprise phagocytosis and the opsonic index, the effect of immune serum on the germination of seeds and the growth of plants, decoloration of plant dyes by dye-immune serum, Pfeiffer's test, and antidiastatic power of anti-yeast serum.

Part II

Serology has shown itself highly suitable for purposes of identification of plant products. An important practical application of this is in the identification of contaminants or adulterants of flours, meals, fodders, drugs, etc., especially when the contaminant is toxic or present in illegal quantity. Ricin, field mustard, ergot, corn-cockle, plantain, and potato flour as adulterants or contaminants have been detected by this method.

In seed testing serology has been of less value principally because here varietal differences and provenience of seed samples are of paramount importance, while generic and specific differences are usually easily recognized by the morphology of the seeds. It is often very difficult to differentiate varieties of plants serologically, although in a few cases this has been claimed. Several factors are involved in this problem of specificity.

It is manifest that the botanical family is an arbitrary unit, based on conceptions of homogeneity which may be very different among different orders of plants. It necessarily follows that some families are more compact in an evolutionary sense and consequently more compact serologically. Thus, in some families it has been found that differentiation of genera is impossible serologically, in others genera are distinct but species indistinguishable, until finally species, varieties, and even biological races and sexes may occasionally be separated serologically. The genus or species usually represents the limits of separation, however.

It has become recognized that the plant antigens customarily used represent complexes of many individual antigens (protenoms), and the resulting reactions are mosaics of numerous individual reactions. In two closely related forms the greater number of individual antigens are in common, only a few being distinct. This makes possible the relationship reaction which may be looked on as a group of more or fewer identity reactions according as the number of antibodies corresponding to the several antigen units (protenes) is greater or less. Relationship reactions are, then, the summation of many or few identity reactions with the antigenic units in the protein complex of a plant extract.

The success of Nuttall in deriving animal

relationships by serological methods led to a number of tentative attempts to do the same with plant extracts during the years 1900–1912. This work, chiefly with legumes and grains, was sufficiently successful to warrant the development of the subject on a broad scale. This was done by Mez and his colleagues in Königsberg in the years 1912 to the present. The whole plant kingdom has been studied serologically, and from the results a serological phylogenetic tree, the "Königsberg Stammbaum," has been derived. On the whole this agrees very well with existing systematic conceptions, and in great part has been well received by systematists. The tree, besides bringing out the relationships of the many families involved, exemplifies the unity of life and the absence of convergence in protein evolution.

In 1926 Gilg and Schürhoff in Berlin and their students began a series of papers strongly criticizing the Königsberg studies. The failure of the Berlin school in repeating the Königsberg work is interpreted by the fact that the Berlin techniques differ in a number of important respects from those of the Königsberg school, lacking in certain instances the exactness and sensitivity of the Königsberg methods. A number of independent investigators in Germany, Japan, Holland, and Finland have carefully repeated the Königsberg procedures with selected plant groups and have obtained results confirming the Königsberg Stammbaum and bringing out the fallacies of the Berlin criticism.

In recent years a number of more specialized and restricted sero-diagnostic studies have been undertaken in various other laboratories, the results of which have shown the service of serology in correlation with systematics particularly with regard to such problems as grafting compatibilities, cross-inoculability of legumes with nodule bacteria, disease susceptibility and resistance, possibilities of hybridization, and systematics of such difficult groups as the algae and fungi.

A study of the possible serological reactions of plant non-proteins has brought out the following points: The "water-soluble specific carbohydrates" of yeasts and other fungi are highly active as test antigens but do not themselves induce antibody formation, as a rule. Their specificity is problematical; in some cases, at least, entirely heterogenetic reactions are obtained with them. Certain plant glucosides, notably the toxins of *Amanita*, and poison ivy, appear to induce toleration in animals. Plant alkaloids, starches, chlorophyll and xanthophyll, hematoxylin, and tannins have not in general been found to act satisfactorily as antigens, although they may often be studied serologically by means of their protein contaminations. Pure plant lipoids are non-antigenic in the sense of their inducing antibody-formation, unless bound to proteins, in which case antibodies are formed which react with the pure lipoids *in vitro*. Practically, the plant lipoids may be conveniently studied serologically because of their characteristic protein contaminants.

The yeasts and yeast-like fungi pathogenic to animals give agglutinin, precipitin, and complement fixation tests with the sera of infected or immunized animals, although these forms are often so closely related that differentiation and relationship study are impractical. Hay fever, asthma bronchiale, urticaria, and a dermatitis in workers with satinwood are all due to anaphylactic reactions to plant antigens, and their diagnosis and treatment proceeds logically and effectively from this standpoint. Death in animals when due to antigenic plant toxins may be

accurately diagnosed by serological tests of the toxin residues in stomach and intestinal contents.

As regards certain basic biological problems, serology has been used with success in studying the movement of proteins in plants, although the possibilities in the study of protein metabolism, especially as regards graft-metabolism, mycorrhizal relationships, and acquired immunity in plants, have hardly been sounded. Moritz has shown that the Schultz-Dale anaphylactic test affords valuable evidence as to the nature and constitution of plant hybrids, in comparison serologically with their parents. In the field of plant virus research serology is of continually increasing benefit in connection with the problems of virus relationships and classification, estimation of relative and absolute virus concentration and purity, detection and diagnosis of virus diseases and virus types concerned, movement of virus in the plant, and the nature of the virus particle. Although in this work it has been assumed that it is the virus itself which engenders the reactions, the possibility that the reactions may be due to some specific byproduct of virus activity has not been excluded.

The possibilities and limitations of phytoserology are briefly discussed. The main advantages of the method are its sensitivity, its specificity, and its objective character. It is limited at present by a number of inadequacies in technique, which, however, are slowly being minimized or eliminated. The method is significant only when skillfully employed and when the reactions are very rigidly controlled. Given such prerequisites, however, and judging pragmatically, the serological reactions have already served in the solution of a number of important botanical problems, and give promise of far-reaching value as an adjunct to many phases of botany.

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DETERMINATION OF LIMB-AXES

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INTRODUCTION

The various problems of development and growth ultimately may be shown to have similar answers and, although speculation concerning them is of consuming interest, we are rather completely occupied at present with preliminary studies on the many phases of the subject and the end is far from being in sight. Differentiation of tissues, organs and organ systems is being investigated from many angles, a small portion of one of which has been chosen for discussion. The restricted field of polarity fixation in the limb-axes has been selected chiefly because of a peculiar concentration of personal interest. Direct reference to the problems of causality and of terminology, discussed in part by Harrison ('33), is omitted from consideration here. It is proposed merely to set forth a simplified review of some of the more firmly established observations together with suggestions for future investigation.

Most of the experimental work upon limb development has been carried out upon embryos of various species of amphibia although the chick has been used to some extent. It happens that the phase to be discussed has involved more particularly the urodele embryo and has been a direct outgrowth of the pioneer studies which are too well known to need comment at this time.

THE LIMB RUDIMENT AND ITS AXES

At the stage of development (Fig. 1) most used for the earlier experiments upon the limb, the cells whose prospective significance lies in that direction are situated (Harrison, '18) in a flattened disc just ventral to the pronephros and cover an area roughly corresponding to the width of three somites (3, 4 and 5). For the purpose of the present discussion not the least important results were derived from Harrison's ('21) experiments which indicated that axial differentiation in the limb was inherent in the intimate protoplasmic structure of the rudiment. Subsequent experiments have provided no evidence to the contrary and the stereodiagrams presented at that time to show the "hypothetical progressive differentiation of the structural units" (Fig. 136, p. 89) may be reproduced here (Fig. 2) as a framework upon which to hang the work to be described. The first diagram represents an isotropic condition with no axial polarization, the second has one axis polarized, the third has two, the fourth and fifth have three—the last two figures representing a mirror imaged pair. The condition of isotropy (Fig. 2, *1*) has not been demonstrated to obtain in any normally developing limb rudiment. If it exists at all, it must do so most fleetingly and in a stage of development somewhat earlier than is convenient to use for experimental removal of the anlage.

This is indicated by Detwiler's ('33) observation that limb-forming cells, transplanted from embryos

with large to medium sized yolk-plugs, already have a polarized anteroposterior axis. Milojevič's ('23, '24) studies indicated that conditions may be somewhat different in regenerating limb blastemas and that they, in early stages, may be composed of cells which are quite indifferent so far as polarization is concerned. However, similar experiments carried out by Lodyženskaja ('28, '29) upon limb regeneration in the axolotl do not support those obtained by Milojevič upon *Triton*.

The conditions of these experiments, especially the retrograde changes undergone by some of these buds after transplantation, make it doubtful whether the

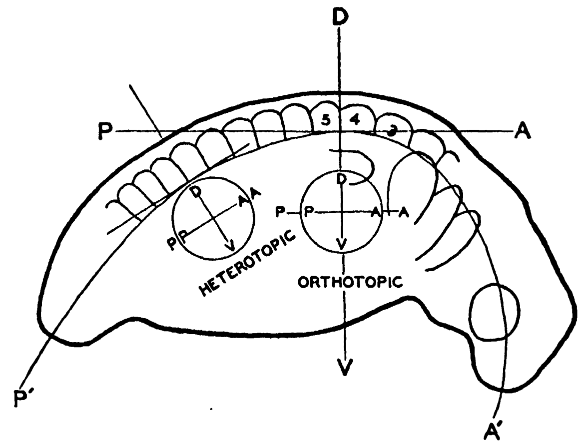

FIG. 1. DIAGRAM OF A. PUNCTATUM EMBRYO IN STAGE 29 (MODIFIED FROM HARRISON, '25, FIG. 10, P. 483)
The circles represent limb rudiments, in normal location (orthotopic) and in a flank position (heterotopic), after transplantation. The letters within the circles represent cardinal points of the limb-axes; A = anterior, P = posterior, D = dorsal, V = ventral. Similar letters outside the circles represent the axes of the embryo body—relative to the line A'—P' drawn to correspond to the body curvature. The three myotomes of the limb area are numbered—3, 4, 5. Magn. approx. × 16.

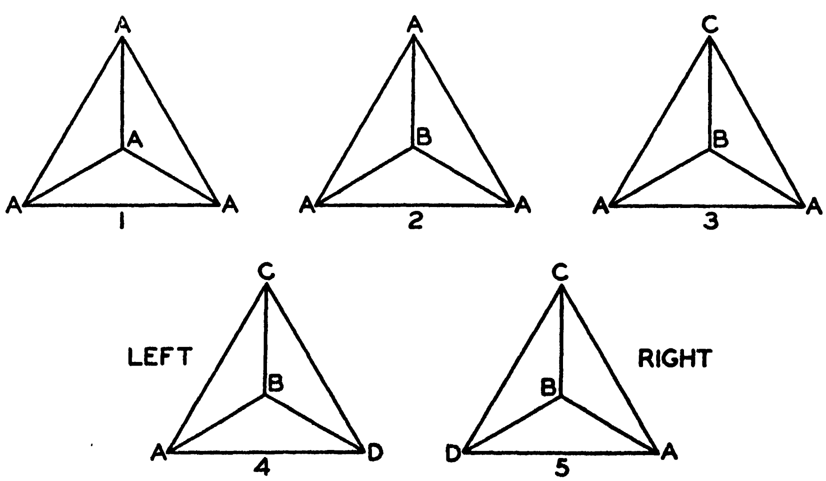

FIG. 2. DIAGRAMS TO SHOW HYPOTHETICAL PROGRESSIVE DIFFERENTIATION OF THE STRUCTURAL UNITS
(*1*) Condition of isotropy; (*2*) polarization with reference to one axis; (*3*) bilateral symmetry (two axes differentiated); (*4* and *5*) condition of complete asymmetry (three axes differentiated), giving right and left enantiomorphs (from Harrison, '21, Fig. 136, p. 89).

results with regenerating blastemas should be compared directly with those from embryonic rudiments.

THE ANTEROPOSTERIOR AXIS

In *Amblystoma punctatum* the condition of a single polarized axis (Fig. 2, *2*) persists through several days of development (Fig. 3) thereby giving ample opportunity, in point of developmental time, for studies upon the behavior of a limb rudiment with only one of its three axes determined. Observations carried out within this period have been too numerous to review at this time and will be mentioned only as they bear directly upon the topic at hand.

Some results of experimental reversal

Harrison's ('21, '25) experiments (Fig. 4) showed very clearly that normal orientation of this anteroposterior axis with relation to the body wall of the embryo (A) resulted in the production of a harmonic limb (C); also that reversal of this axis at operation (D) produced a disharmonic limb (F). The orientation, whether normal or abnormal, of the other two axes of the limb made no difference in the results, nor did the side of origin or location of the graft (Fig. 4, A.D.). In this instance, disharmony was occasioned by the presence of a limb belonging to the other side of the body, i.e. one whose radio-ulnar axis faced in the wrong direction. Furthermore, this imposed change in orientation of the anteroposterior axis accomplished a marked difference in the direction of original adduction (Fig. 4, B, E). The difference did not correspond exactly to the 180° disorientation of the embryonic axis but was often as much as 120°. Operative reversal of no other axis,

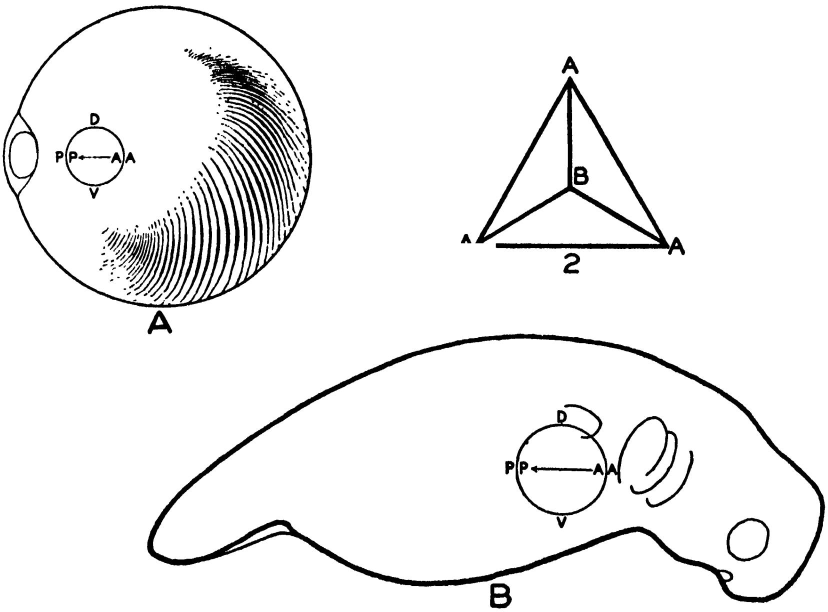

Fig. 3. Diagrams of A. punctatum Embryos

(A) In medium sized yolk-plug stage (modified from Detwiler, '33, Fig. 2, p. 407), and (B) in stage 32; to show the duration of the developmental period characterized by one determined limb-axis. This axis, the anteroposterior, is represented in A and B by the arrows. The stereodiagram *2* corresponding to this period is reproduced from Fig. 2.

even after it becomes determined, brings about such a marked alteration. This question of the direction of limb growth, in the normal embryo as well as after transplantation, has been fully discussed by Harrison ('25). It is of interest that such an important feature of development

Determination of the anteroposterior axis

More or less as a by-product of experiments carried out on embryos belonging to the period of development under consideration, certain interesting suggestions concerning fixation of the anteroposterior

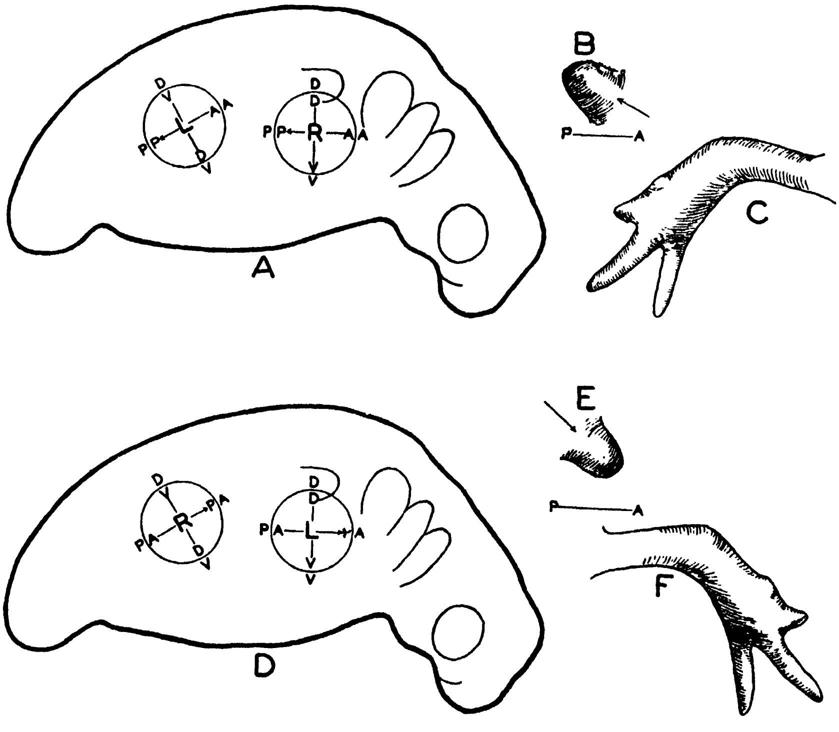

FIGURE 4

A. Diagram of embryo to show harmonic combinations of the antero-posterior limb-axis with the embryo body, using a right rudiment (R) as orthotopic graft and a left rudiment (L) heterotopically. B. Showing the posterodorsal direction of growth (when projected on the median plane of the embryo) of the limb bud when the embryo-graft combination is harmonic. C. Outline of the harmonic primary limb which arises from such a graft. D. Diagram to show, in comparison with A, a right (heterotopic) and left (orthotopic) rudiment grafted with anteroposterior axes out of harmony with corresponding axes of embryo body. E. Showing the anteroventral direction of growth (when projected on the median plane of the embryo) of the limb bud from such a disharmonic graft. F. Outline of the disharmonic primary limb which arises from such a graft.

as this should become established along with determination of the first of the axes and probably, therefore, at the time of segregation of limb-forming material. If, or perhaps more optimistically when, we secure our isotropic limb anlage the degree of correspondence between these two characters should be investigated.

limb-axis have appeared. Harrison has pointed out that regulation of posture in transplanted limbs may take place either by rotation or by reduplication with subsequent resorption of a primary, potentially disharmonic bud. Nicholas ('24) has investigated the question of rotational recovery of posture, and the phenomenon

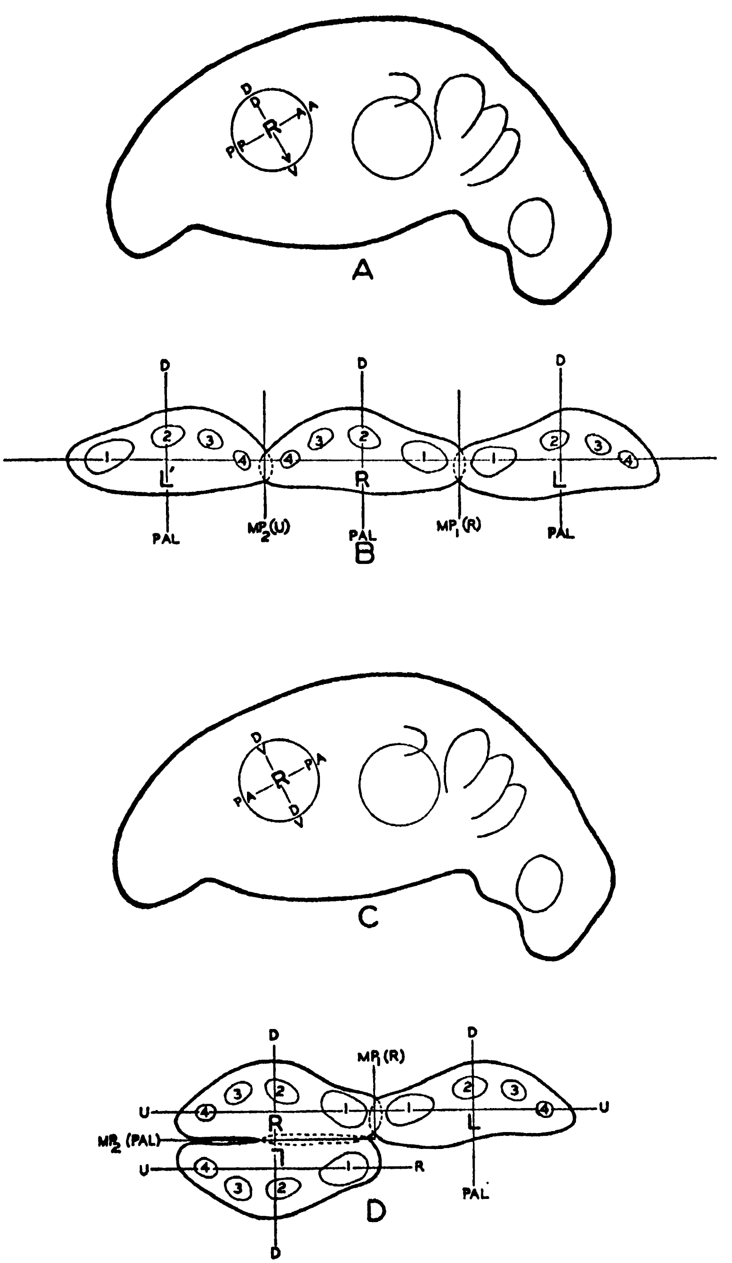

FIGURE 5

A. Diagram of embryo with a harmonic, heterotopic limb graft. B. Showing mirror relationships between the primary (R) limb from such a graft and a possible radial (L) and (or) ulnar (L′) supernumerary. D = dorsal; PAL = palmar; $MP_1(R)$ = first (radial) mirror plane; $MP_2(U)$ = second (ulnar) mirror plane; 1, 2, 3, 4 = digits from radial to ulnar side of limb. (Modified from Harrison, '21, Fig. 4, p. 14.) C. Diagram of embryo with disharmonic limb graft. D. Showing mirror relationship between the disharmonic (L) limb from such a graft and its radial supernumerary (R); also the mirror relationships between the latter and a possible palmar supernumerary (ꓤ). $MP_2(PAL)$ = second (palmar) mirror plane. (Other abbr. and acknowledgment as in B. above.)

of reduplication has been the subject of a considerable number of studies. A few observations which are helpful in the present discussion, have come out of these studies. Reversal of asymmetry in the extra limb, a reversal causing it to stand as the mirror image of the primary member, must be due to the establishment of polarity in its anteroposterior axis opposite to that of the corresponding axis of the original limb. This follows from the indifferent condition of the dorsoventral axis and is further demonstrated by observations upon the development of certain limbs which underwent both reduplication and rotation (Swett, '24, '26). We have no crucial experiment upon the exact time at which polarization takes place in the anteroposterior axis of the supernumerary member. However, there is some more or less circumstantial evidence, derived from experiments in which young double buds were transplanted (Swett, '28a), that it occurs very early. Apparently, in the secondary as well as in the primary member, the anteroposterior axis undergoes determination at the time the rudiment is segregated, in the primary with reference to the corresponding axis of the body of the embryo and in the supernumerary with a reversed relationship to the corresponding axis of the primary. Figure 5 presents (A) a harmonic orientation of a transplanted rudiment and (B) the symmetry relations which result when such a graft produces an anterior (L) and (or) a posterior (L') supernumerary. In the case of transplanted limbs, the primary member may or may not be harmonic in the definitive condition but the secondary member apparently has its anteroposterior axis polarized oppositely in either case.

Naturally the above can be applied only to a supernumerary limb which has arisen from the radial or the ulnar border of the primary. In the relatively rare dorsal or palmar reduplications (Fig. 5, D), the definitive conditions of symmetry are due to determination of the dorsoventral axes —the anteroposterior axes being parallel.

THE DORSOVENTRAL AXIS

Owing to studies carried out particularly by Harrison and by Nicholas we have certain definite information on the dorsoventral limb-axis prior to its determination, i.e. during the period of differentiation characterized by only one polarized limb-axis. In the case of *Amblystoma* at least, all the early experiments showed very clearly that the asymmetry of a limb derived from a transplanted rudiment was, in large measure, independent of the orientation imposed upon the dorsoventral axis of the embryonic rudiment. However, in the course of his experments on recovery of posture by rotation, Nicholas found that rotation, under the influence of an operatively rotated ring of surrounding tissue, could be made to take place toward a posture abnormal with respect to the body as a whole. Further, by using larger grafts, with a diameter equal to the width of five somites, it was possible to produce inverted limbs by the same operative orientation which resulted in normally postured members when smaller grafts were used. This finding is contained in an unpublished study, the manuscript of which Dr. Nicholas was kind enough to send to me. The rôle of tissue immediately surrounding the rudiment thus was shown to be most important while factors operating in the body of the animal as a whole had a more indirect effect.

Determination of the dorsoventral axis

Determination of the dorsoventral limb-axis, in the embryonic limb rudiment of urodeles, has been shown to occur at a later period of development than the deter-

mination of the anteroposterior. This is somewhat different from the condition in skaja, '28, '29) in which these first two axes appear to undergo simultaneous deter-

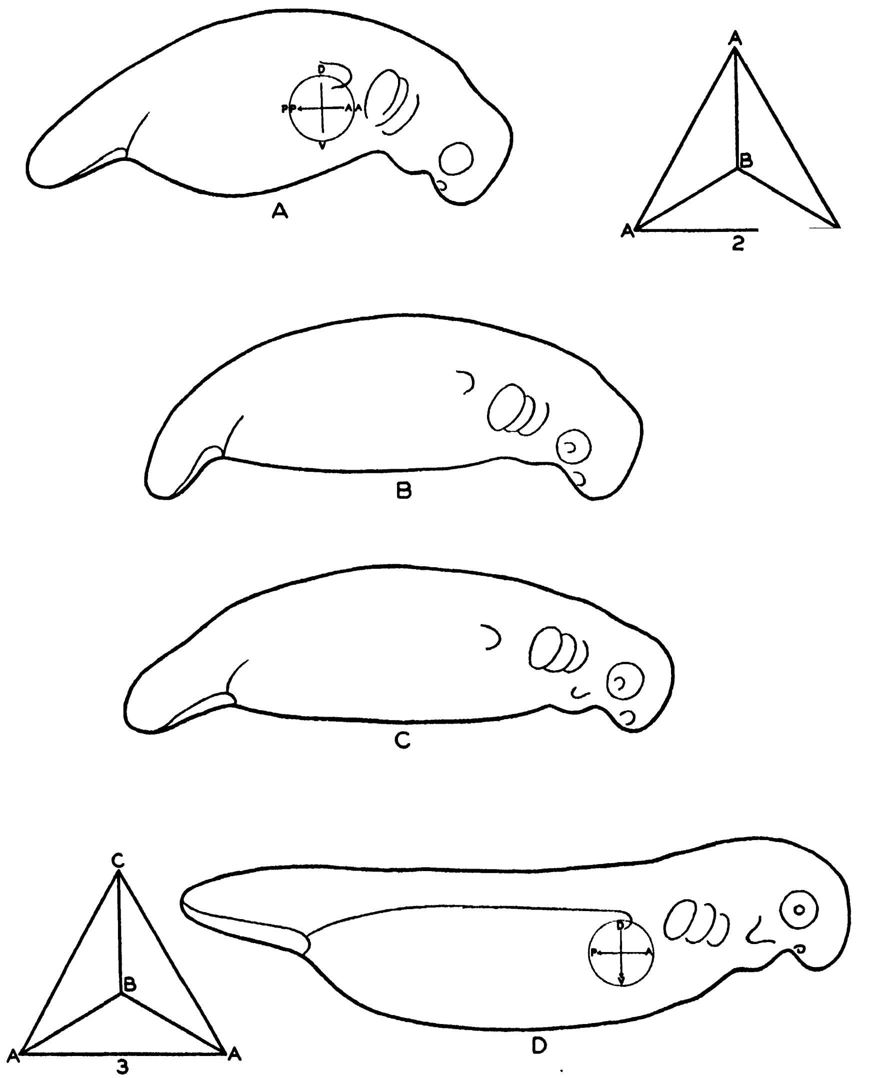

FIG. 6. A SERIES OF DIAGRAMS TO SHOW THE RELATIVE DEGREE OF DEVELOPMENT OF EMBRYOS (A) BEFORE, (B, C) DURING AND (D) AFTER THE DIFFERENTIATION CHANGES LEADING TO DETERMINATION OF THE DORSOVENTRAL LIMB AXIS

A. Embryo stage 32, one limb-axis determined, (2) the corresponding stereodiagram from Fig. 2. B. Embryo stage 33, transitional period. C. Embryo stage 34, transitional period. D. Embryo stage 35, two limb-axes determined, (3) the corresponding stereodiagram from Fig. 2.

anurans (Graper, '22, '23, '24, '25, '27) and in the regenerating limb blastemas of urodeles (Milojevič, '23, '24; Lodyženskaja, '28, '29) ... mination. Figure 6 indicates stages of *A. punctatum*, (A) just before polarization of the second limb axis, (B, C) during the

transitional period and (D) after determination has been accomplished.

The period at which this axis becomes determined has been investigated in *Triton* by Brandt ('24), in *Amblystoma tigrinum* by Ruud ('26) and Hollinshead ('36), in the axolotl (*A. mexicanum*) by Ruud ('31) and in *A. punctatum* by Swett ('27). Some species variation seemed to have been apparent but a recent study by Hollinshead has indicated that the difference may not be so great as was at first thought. He repeated certain of Ruud's experiments on *A. tigrinum* and paralleled the series with another using smaller grafts. The results showed that the axis in question does not become determined so early in *tigrinum* as in *punctatum*. His findings were somewhat at variance with Ruud's conclusions but the discrepancies appeared to be due chiefly to differences in graft size.

Apparently the periods of dorsoventral-axis determination in the two species are really quite close together, especially when they are compared on the basis of the relative degree of limb development rather than with regard to the stage of development exhibited by the embryo as a whole.

Perhaps the most characteristic finding in limbs grafted after determination of the dorsoventral axis is the inverted posture of the primary limb which results from operative inversion of the rudiment. That it actually develops upside down, instead of being drawn into that position secondarily, has been shown by careful study of the developmental stages in a considerable number of experimental cases. Such a limb does not undergo the reversal of prospective asymmetry shown by limbs developed from younger inverted grafts. Furthermore, the initial direction of growth after inversion is shifted only about sixty degrees from the normal and the asymmetry of the limb is dependent upon the orientation, not of one, but of both determined axes with respect to the body wall. No detailed studies have been made upon rotational recovery of posture in these older stages. The small amount of information at hand has indicated that they behave much like the younger stages in *Amblystoma*, although conditions in *Triton* appear (Brandt, '24) to be different.

Determination apparently not reversible

During the past several years a series of investigations has been under way to test some of the conditions affecting axial determination. The dorsoventral axis was chosen in preference to the others because of the ease with which it can be disoriented, both before and after it has become determined. Certain experiments (Swett, '30), in which inverted rudiments with determined dorsoventral axes were transplanted to brachial and to flank locations on younger embryos, have indicated determination of this axis to be absolute. Although implantation was accomplished some time before the limb rudiments of the recipient underwent determination of the corresponding axis, there was no observable effect upon the graft. It developed into an inverted limb just as it would have done had there been no age discrepancy.

Can determination be delayed?

Preliminary experiments designed to find out whether the process of determination could be accelerated or delayed have not been conclusive, but they do offer a few points of interest. Attempts to delay the process have been completely negative (Swett, '37). Limb rudiments from embryos (Fig. 7, A) just about to enter upon the transitional period in which the dorsoventral limb-axis becomes polarized, were transplanted to younger embryos (Fig. 7, B) without change of orientation. After the donor had reached a stage (Fig. 7, A′) beyond the transitional period and the recipient had grown to a stage (Fig. 7, B′) just before the transitional period, these grafts were removed and implanted upside down on the opposite flank of another

embryo in the same stage of development (Fig. 7, C′). Heterotopic positions were used to avoid complicating factors possibly resident in the normal brachial region. The definitive limbs which developed showed very clearly that there had been no change in the rudiment which fluences brought to bear upon it by the donor. When different orientations were employed at the first or the second operation, the results showed that the relationship of the anteroposterior, or of the dorsoventral, axis to the body wall made no difference.

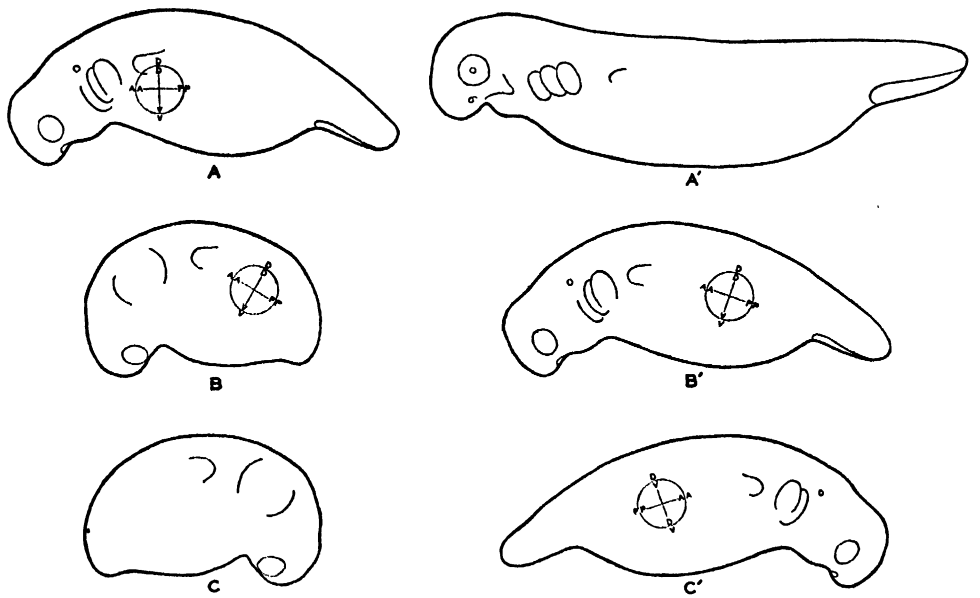

FIG. 7. DIAGRAMS OF EMBRYOS USED IN EXPERIMENTS ON DELAYED DETERMINATION OF THE DORSOVENTRAL LIMB-AXIS

A, B, C. The three embryos chosen in advance of the first operation. A. The donor, stage 32, of the left limb rudiment used for transplantation. B. The recipient, stage 25, of the graft (heterotopic, homopleural, dorsodorsal). C. Extra embryo of same age as B.

A′, B′, C′. The same three embryos at the time of the second operation. A′. Original donor A, now in stage 35. B′. Original recipient B, now in stage 32. C′. Final recipient C, now in stage. (Graft heterotopic, heteropleural, dorsoventral.)

could be ascribed in any way to possible factors in the younger body. In spite of its changed environment the limb rudiment proceeded to undergo dorsoventral-axis determination at approximately the normal rate, as indicated by its development into an inverted limb. The nature of the experiment permitted no conclusions as to whether this step in differentiation was accomplished under the influence of factors in the younger body or as a carry-over from possible pre-transplantation in-

Can determination be accelerated?

Judging from several preliminary series in which attempts were made to expedite the process of determination, transplantation of younger limb rudiments to older embryos may result in some alterations in the process of development. At present, however, there is some doubt whether the effect is a direct one upon the cells of the graft. In these experiments a young rudiment (Fig. 8, A) was transplanted, without change of orientation, to the

body wall of an embryo (Fig. 8, B) just about to enter upon the transitional period of dorsoventral axis determination, and left there for the duration of this period. When the donor had reached a stage (Fig. 8, A′) just before the beginning of the transitional period, the graft was removed from the original recipient (Fig. 8, B′) to the opposite side of the body of another limbs were invariably produced when a ring of surrounding tissue was carried over with the graft at the time of the second operation. However, when no extraneous tissue was included the results were quite inconstant. Although all the possible complicating factors have not been ruled out, nor have the results reached a state from which generalizations can be made, it

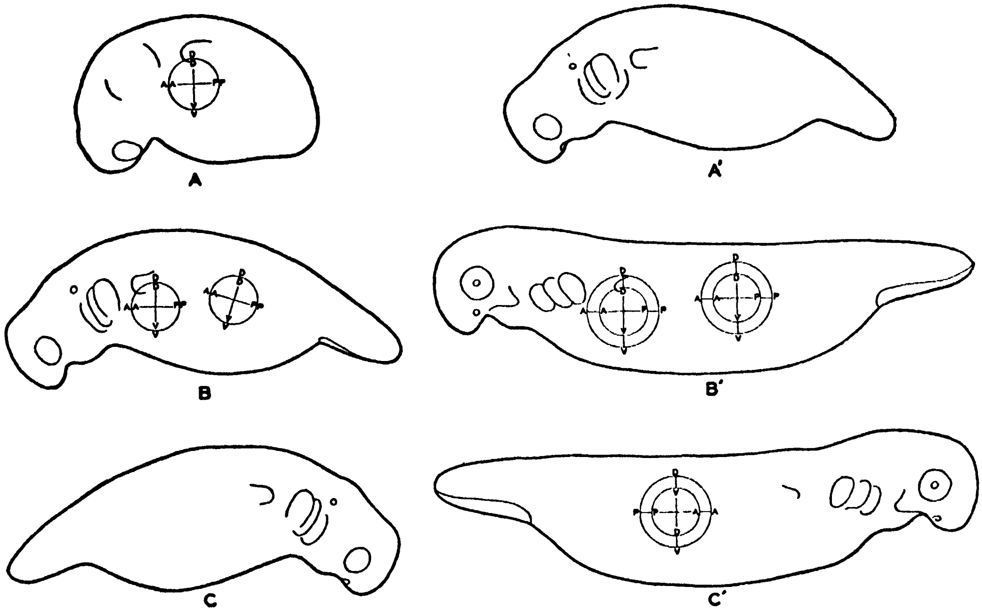

Fig. 8. Diagrams of Embryos Used in Experiments on the Acceleration of Determination of the Dorso-ventral Limb-Axis

A, B, C. The three embryos chosen in advance of the first operation. A. The donor, stage 25, of the left limb rudiment used for transplantation in normal orientation to left brachial region or flank. B. The recipient, stage 32, of the graft (homopleural, dorsodorsal, orthotopic or heterotopic). C. Extra embryo of same stage as B.

A′, B′, C′. The same three embryos at the time of the second operation. The larger circle around the limb rudiment represents the margin of the "surrounding tissue" included with the rudiment in some of these second grafts. A′. Original donor A, now in stage 32. B′. Original recipient B, now in stage 35. C′. Final recipient C, now in stage 35 (graft heterotopic, heteropleural, dorsoventral).

embryo in the same stage of development (Fig. 8, C′). As in the previous series these grafts were inverted at the second operation. Both brachial and flank locations were employed for reception of the first graft (Fig. 8, B) and the second graft was varied as to size (Fig. 8, B′). As might have been expected from the results of Nicholas and of Milojevič, inverted appears that the cells of the rudiment originally grafted were essentially unaffected. If this proves to be true, as the result of subsequent experimentation, we must conclude that the factors resident in the tissue surrounding a younger grafted rudiment have little or no power to fix the polarity of its dorsoventral axis prematurely; although they are, during the

same period, effective in the process by which the embryo's own limb undergoes determination of this axis. It follows that the timing factor in the determination process is at least partially resident in the rudiment itself. Whether this can be tested experimentally remains to be seen.

Rôle of adjacent tissue in the process

It seems appropriate to mention at this time another angle from which this problem is being investigated. Inasmuch as Nicholas' experiments had shown the dorsoventral limb-axis to be determined with reference to, or by, the tissue immediately surrounding its base, it became of interest to discover what portion of this ring produced the effect. Consequently, experiments were made with grafts composed (Fig. 9, A) of the limb rudiment together with tissue anterior and posterior, or dorsal and ventral, to it. The

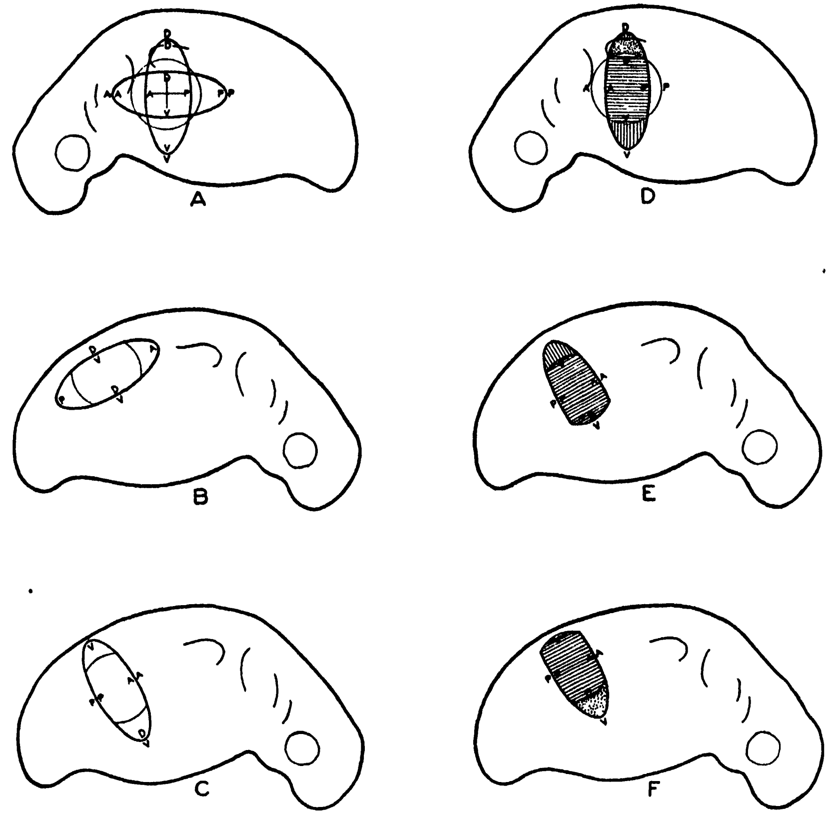

Fig. 9. Diagrams to Show Origin and Placement of Grafts Composed of a Portion of the Limb Rudiment together with Various Fragments of the Tissue Surrounding It

(The orientation of each graft is heteropleural dorsoventral.)

A. Showing the outlines of horizontal and vertical grafts (heavy lines) with respect to the left rudiment of the donor embryo. The margin of presumptive limb-forming material is indicated by light lines within the graft. B. Showing location of the horizontal graft from A on the right flank of another embryo. C. Showing location of vertical graft from A on the right flank of another embryo. D. Donor embryo for grafts including a portion of the limb rudiment (horizontal stripes) together with tissue dorsal (stippled) or ventral (vertical stripes) to it. E. Recipient embryo showing location of graft, from D, composed of limb-forming cells together with the small ventral fragment of surrounding tissue. F. Recipient embryo showing location of graft, from D, composed of limb-forming cells together with the small dorsal fragment of surrounding tissue.

results consisted of reversed limbs when the inverted anterior and posterior extra-brachial areas accompanied the limb graft (Fig. 9, B), and inverted limbs when the dorsal and ventral areas were used (Fig. 9, C). It thus appeared that the factors effective in dorsoventral limb-axis determination might be resident in regions dorsal or ventral to the limb rudiment but not in regions anterior or posterior to it. Then small areas either ventral or dorsal to the anlage (Fig. 9, D) were grafted with it in an inverted orientation to see if either, by itself, was effective. No indication of inversion appeared in the limbs produced when the rudiment was related only to the ventrally situated fragment (Fig. 9, E). The positive cases in the other category (Fig. 9, F) produced inverted limbs. From these observations it appears that the area dorsal to the limb rudiment may play the major rôle in dorsoventral-axis determination. Here again, additional data are necessary before we can offer an opinion as to whether certain neighboring fragments are effective because of their position, original or final, relative to the transplanted rudiment, or because of their constitution. All of these unfinished experiments are presented with considerable hesitancy and with the hope that, if they have any value, they may suggest ways and means for a further attack upon the problem.

THE MEDIOLATERAL AXIS

The next stage of limb-axis differentiation is characterized by polarization of the third and last axis—the mediolateral. Only after this has been accomplished is the asymmetry of the limb really established on a permanent basis. Harrison ('25) showed very clearly that operative reversal of this axis during the so-called tail bud stages of *Amblystoma*, made no difference in the asymmetry or posture of the limb produced. The axis became determined with reference to its surroundings and the limb developed as though no disorientation had been imposed. In the course of experiments upon later stages (Fig. 10, A, E-G), results (Fig. 10, D) duplicating those of Harrison were secured (Swett, '27, '28b) until the embryos reached a stage somewhat more advanced than that at which the dorsoventral axis had been shown to become determined. As in the experiments upon the dorsoventral axis, a transitional period (Fig. 10, E, F.) was shown to exist in the differentiation process leading to determination of the third axis. Beyond this stage of development its operative reversal, with the other axes maintaining a normal relationship with the body (Fig. 10, H, I), did not result in a reversal of prospective asymmetry. The limbs obtained (Fig. 10, J) from this experiment were lefts instead of rights (Fig. 10, D) and their posture was rather peculiar. They grew posteriorly close to the body wall, thus diverging about 90° from the usual posture of a left limb on the right side of the body (Fig. 4, F). The factors involved in this assumption of posture (see Swett, '28b) have not been made clear but the apparent failure of growth from the proximal surfaces in these grafts, coupled with the persistence of the original prospective asymmetry, has taken the developmental question out of the category of the "Bruchdreifachbildung." The explanation may be that these limbs, with their asymmetry already determined, continued to grow in the same general direction as before they were grafted—only shifting enough to avoid growing into the body of the embryo. At all events, their behavior has indicated that after a certain stage (Fig. 10, G) had been reached, the final axis had become irreversibly polarized (Fig. 10, *4*, *5*).

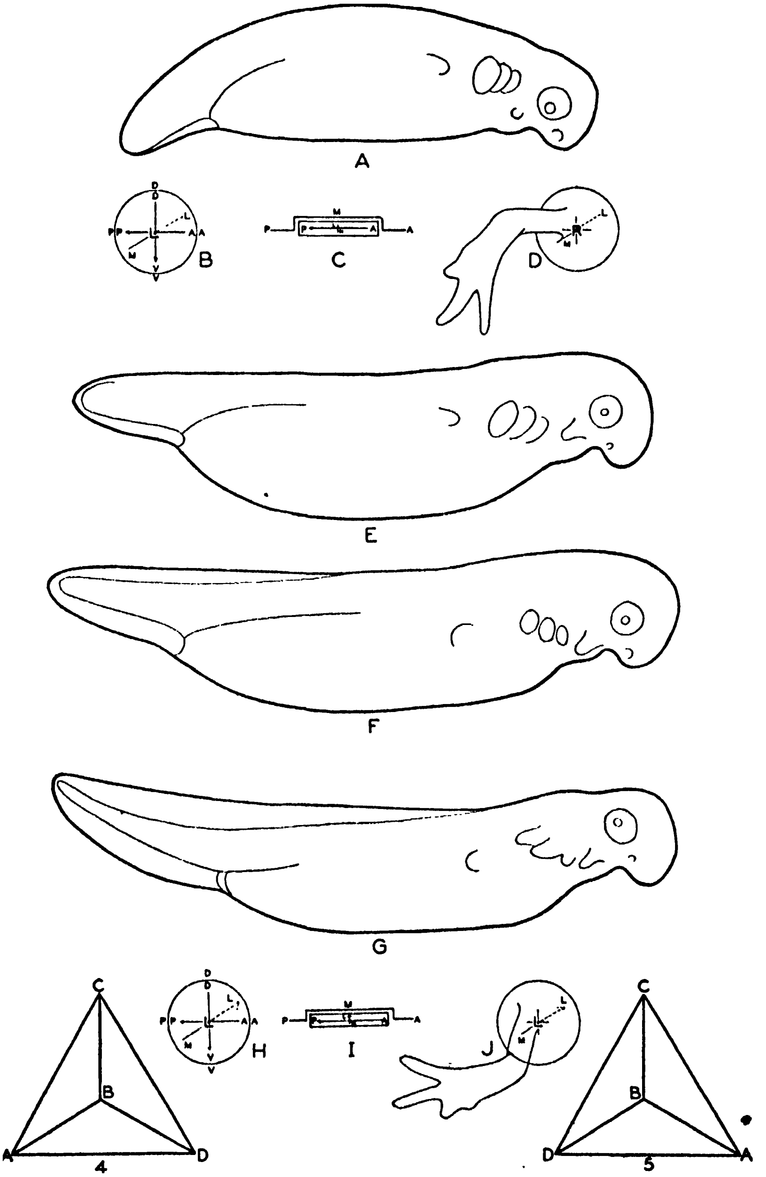

FIG. 10. A SERIES OF DIAGRAMS TO SHOW THE RELATIVE DEGREE OF DEVELOPMENT OF EMBRYOS (A) BEFORE, (E, F) DURING AND (G) AFTER THE DIFFERENTIATION CHANGES LEADING TO DETERMINATION OF THE MEDIOLATERAL LIMB-AXIS

A. Embryo stage 34, mediolateral axis not determined. B. Diagram showing, in lateral view, orientation of grafted limb rudiment with the change of operative orientation affecting only the mediolateral limb-axis (M — L). C. The same condition as in B diagrammed as though viewed from above. D. Outline of the harmonic limb produced by such a graft (B, C) carried out before determination of the mediolateral axis has taken place. E. Embryo stage 35, transitional period. F. Embryo stage 36, transitional period. G. Embryo stage 37, mediolateral axis determined. H, I. Diagrams corresponding to B, C above, same operative orientation but with determination of all the axes indicated by the arrow tips on the three lines corresponding to them. J. Outline of the disharmonic limb produced (contrast with D) when the above graft is made after the third axis has been determined. *4, 5* Right and left stereodiagrams from Fig. 2 illustrating the axial condition attained by the elements composing the limb rudiment at this stage.

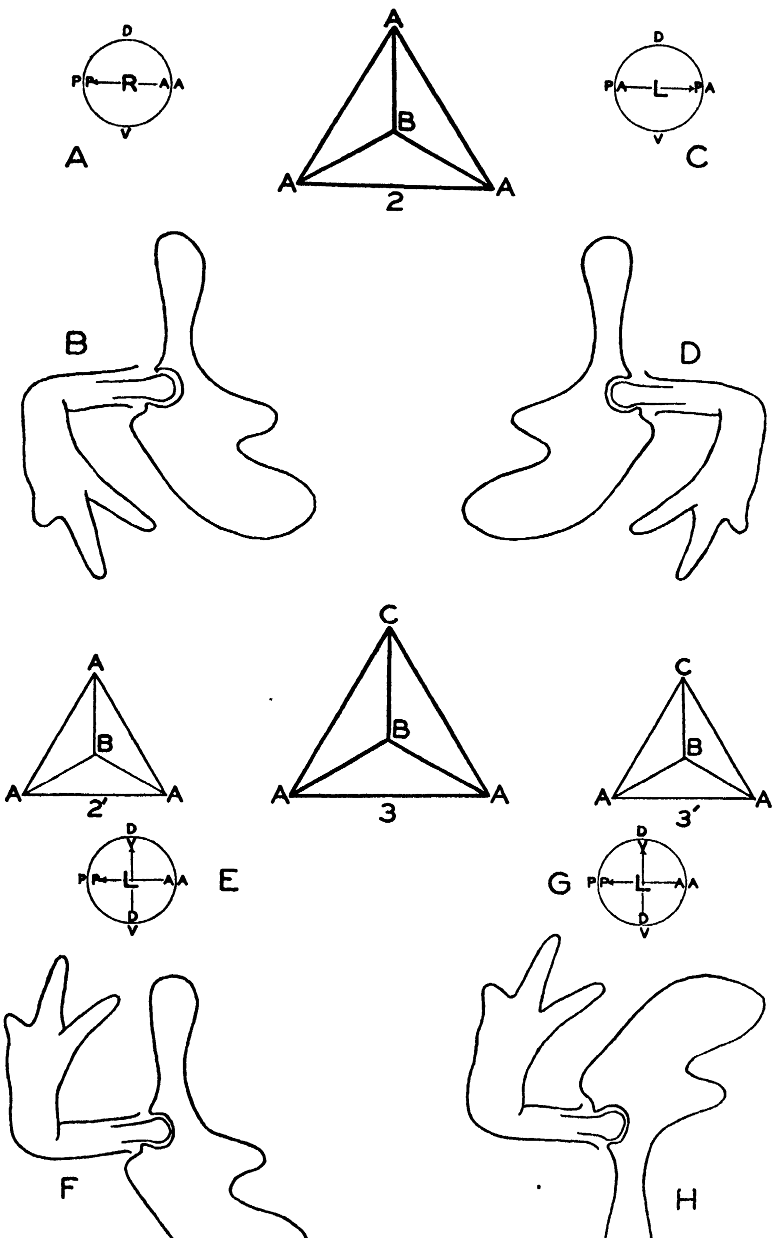

Fig. 11. Diagrams Showing Harmonic and Disharmonic Limb-Girdle Relationships with Relation to Determination of the Anteroposterior and the Dorsoventral Axes of the Two Rudiments

A–D The free limb and shoulder girdle rudiments have only one axis, the anteroposterior, determined (2 from Fig. 2). A. Diagram indicating harmonic orientation of a limb graft. B. A harmonic limb and a harmonic girdle. C. Diagram indicating disharmonic orientation of a limb graft. D. A disharmonic limb and disharmonic girdle. E. Indicating heteropleural dorsoventral orientation of grafted limb rudiment after determination of dorsoventral axis of the free limb (3 from Fig. 2) and before determination of corresponding axis of the shoulder girdle (2′). F. Disharmonic limb-girdle combination composed of a right girdle and an inverted left limb. G. Same orientation and conditions as in E. except dorsoventral axis of girdle (3′) has undergone determination. H. Harmonic limb-girdle combination composed of inverted left elements.

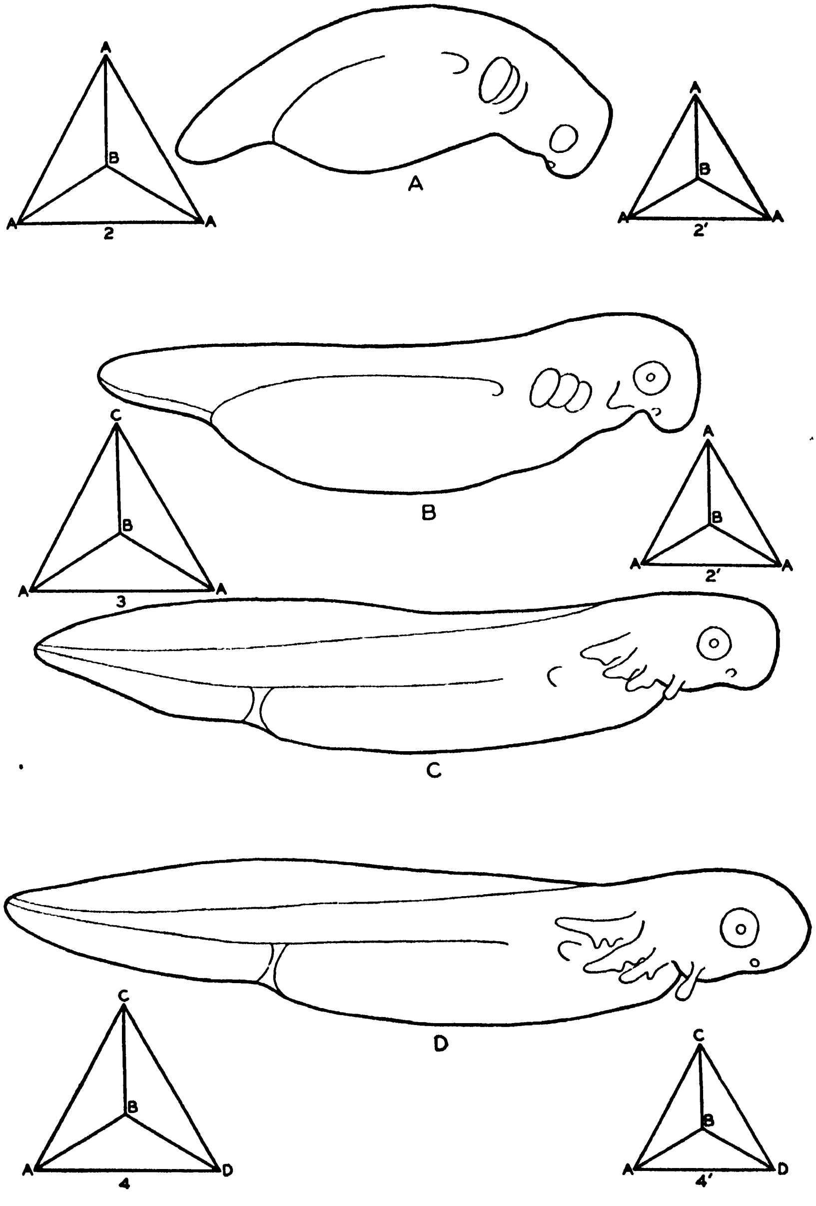

FIG. 12. DIAGRAMS SHOWING STAGES OF DEVELOPMENT DURING WHICH HETEROPLEURAL DORSOVENTRAL LIMB GRAFTS PRODUCE (A) HARMONIC LIMB-GIRDLE COMBINATIONS OF THE TYPE SHOWN IN FIG. 11, B, D; (B, C) DISHARMONIC COMBINATIONS (FIG. 11, F); AND (D) HARMONIC COMBINATIONS LIKE THAT SHOWN IN FIG. 11, H

The corresponding stereodiagrams (*2*, *3*, *4*) at the left apply to the axial condition in the free limb; those at the right (*2'*, *2'*, *4'*) to the axial condition in the girdle.

A. Embryo, stage 32, no dorsoventral axis determination in limb or girdle. B. Embryo, stage 35. At this stage, also in stages 36, 37 and 38, the dorsoventral limb-axis (*3* from Fig. 2) is determined, but the corresponding shoulder-girdle axis (*2'*) is not. C. Embryo, stage 38. D. Embryo, stage 39, dorsoventral axis of shoulder girdle has undergone determination.

LIMB-GIRDLE RELATIONSHIPS

A further point which should be discussed briefly is the relationship between the free limb and the girdle with respect to axis determination. The correspondence between these parts of the appendage in harmonic and disharmonic grafts (Fig. 11, A-D) was noted by Harrison ('21), Nicholas ('24), Brandt ('27) and others. The conclusion seems justified that the anteroposterior axis of each of these rudiments becomes polarized at about the same time. After operations on older stages, certain rather peculiar limb-girdle relationships led to a further study of the question (Swett, '28c). In some of the experiments with inverted limb grafts (Fig. 11, E) the free limb showed an inverted posture while the girdle remained normally oriented (Fig. 11, F). In others, both of these elements were upside down (Fig. 11, H). These findings indicated that the dorsoventral axis of the free limb was determined (Fig. 11, *3*) in each instance but that the corresponding axis of the girdle rudiment was labile in the one case (Fig. 11, *2'*) and determined in the other (Fig. 11, *3'*). These observations do not fit in especially well with Detwiler's ('18) conclusions that the girdle rudiment constitutes a mosaic in the younger stages. If this were strictly true, every inverted graft should result in an inverted girdle. However, apparently this condition has been reported, in *Amblystoma*, only in the case of girdles developed from grafts made at relatively late stages. The final settlement of the problem must await further study.

A tabulation of cases on the basis of the stage used for operation resulted in the recognition of three developmental periods. Operations during the first (ending at about the stage shown in Fig. 12, A) and third (beginning with the stage shown in Fig. 12, D) of these periods produced harmonic limb-girdle combinations when the dorsoventral axes were inverted. Similar operations during the second period (Fig. 12, B, C) resulted in the production of disharmonic limb-girdle combinations.

Although the anteroposterior axes of both the free limb and the girdle apparently become determined simultaneously, there seems to be quite a gap between the determination periods of the two dorsoventral axes. In view of more recent experiments (Nicholas, unpublished; Hollinshead, '36; Swett, unpublished) in which larger grafts were used, it is possible that a re-study of the dorsoventral axis of the girdle might narrow this gap slightly. The relationships between the two mediolateral axes have not been studied. Without going further into the possible relationships between these two parts of the appendage, what has been given will suffice to show that they are in some respects independent of each other developmentally, in spite of their intimate structural association.

SUMMARY

In conclusion we may point to some progress in the study of limb-axis determination, particularly with respect to the periods of development involved in the process. Study of the form, asymmetry and posture of limbs from rudiments grafted during each of these periods has called attention to the part played by extrabrachial tissue, possibly a particular portion of it, in the determination of the limb-axis. There is some evidence, at present insufficiently controlled, for assuming that the speed of axis differentiation is not altered materially when the limb rudiment is grafted to an older or to a younger embryo. If established by future experiments, this may necessitate a revision of our present ideas concerning the impor-

tance of the rôle played by the surrounding tissue. The complexity of possible interactions between graft and surroundings renders doubtful a prompt solution of the problem and necessitates the greatest care in establishing adequate control experiments. No one questions the necessity of a better understanding of tissue interactions in development, nor is anyone sanguine enough to believe that the problem can be settled by one method of investigation. The fundamental changes involved do not lend themselves readily to direct observation and must be studied from as many angles as possible. The limitation of this discussion to one such angle should not be construed as a reflection upon the importance of the others.

For the careful execution of the figures accompanying this paper, the writer is indebted to Mr. Elon H. Clark of the Department of Illustration.

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SEX HORMONES, CARCINOGENICS AND STEROLS

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THE gradually accumulating proof of the molecular structure of such substances as cholesterol and other sterols, the genital gland hormones, vitamin D and of organic chemicals having estrogenic or carcinogenic action indicates that these materials have a close chemical and biological relationship.

An exhaustive consideration of one of the major groups mentioned in the title would require volumes for even an imperfect understanding.

The purpose of this presentation is to indicate and discuss some of the more important features of this large and inclusive field.

THE SEX HORMONES

A complete account of the research work done on the male sex hormone would be difficult, but Berthold's (1) transplantation of testes into capons with the production of the now familiar comb and wattles can be cited as an example of the early work and from this we can proceed to the reports of Brown-Sequard (2), to Pezard's (3) work with extracts and to the studies of McGee (4) who obtained an active dealcoholized extract from bull testes. The very microscopic yields obtained from testes extracts precluded accurate chemical investigation of the active lipin-like extracts. The discovery by Loewe *et al.* (5) that the hormone was present in the urine stimulated the chemical attack and Butenandt (6) obtained an active crystalline material from which several derivatives have been made. The molecular structure was shown to be very similar to that of estrin and some sterols. Ruzicka *et al.* (7) have recently prepared active material from cholesterin; this substance gives biological and chemical tests similar to those obtained with the crystals isolated from the testes. Koch *et al.* (8) have reported that the male sex hormone may be specifically adsorbed from urine on a siliceous earth preparation. The formula now accepted for one male sex hormone, known as androsteron, is given in the diagrams. The molecular configuration of an active agent obtained from testes (testerosterone) has not been proven as yet.

While the nomenclature of the female sex hormones is just now being clarified, it may be stated that there are at present two well known hormones obtainable from the ovary, namely the follicle hormone estrin, and the corpus luteum hormone luterosterone. Estriol a third substance is apparently a hydrate of estrin. Luteosterone is obtainable from the corpus luteum, while estrin and estriol may be extracted from the urine of pregnant women.

Allen and Doisy (9) induced the various stages of estrus in castrated animals by injections of an alcoholic extract of ovaries. An impetus to the purification studies resulted from the observation of Ascheim and Zondek (10) that the estrus-producing hormone could be obtained

from the urine of pregnant animals. They also noted the presence of a water soluble estrogenic agent (prolan) in such urines and developed from this observation a diagnostic test for pregnancy. The crystallization of estrin was announced by Doisy *et al.* (11) and by Butenandt (12). The accepted formula evolved from results of work in several laboratories is given in the diagrams.

Chemical studies are being conducted on luteosterone and the formula for this substance has already been postulated. A similar substance called equilenin has been prepared from the urine of mares, Girard (13).

It is apparent from a study of the chemical formulas that these various substances differ but slightly in the character of their polar groups, side chains and the degree of aromatization. All have the phenanthrene nucleus and, as will be shown later, this characteristic is common although not absolutely essential for the various estrogenic substances existing naturally or prepared by artificial means.

Partial hydrogenation of the follicular hormone, estrin, by the addition of two atoms of hydrogen, increases its activity about four fold, the compound resulting is known as dihydrofolliculin. Another peculiar change resulting from this hydrogenation is that the dihydro form is more active orally. If the aromatic ring of this same hormone be completely hydrogenated by the further addition of six more atoms of hydrogen, the resulting substance does not have the property of producing estrus but does assume the properties of the male sex hormone in causing comb growth in capons and it is also capable of causing enlargement of the seminal vesicles in laboratory animals. Trihydroxy-estrin can be converted into ketohydroxy-estrin by vacuum distillation over potassium bisulphate. Several of these chemical relationships and probable derivatives are shown in the diagrams.

The extremely complex molecular make-up, as well as physical and chemical difficulties such as stereoisomerism, will perhaps delay for some time the basic synthesis of these compounds although the sterols from various sources may be used as a chemical starting point in the production of active preparations, namely; estrin can be obtained from the palm nut, estriol from willow catkins, soy beans can serve as a source of the corpus luteum hormone and cholesterin may be utilized as a basis in the preparation of the male sex hormone androsteron.

Synthetic estrogenic compounds have been studied by Cook, Dodds *et al.* (14) and some of these resemble the original estrin in that they contain the phenanthrene nucleus, while others of the anthracene type and not containing the phenanthrene nucleus have been shown to have some definite estrogenic activity when tested on rats, mice and capons. It appears that there are but certain parts of the estrin molecule essential to this biological activity. The important components of artificial estrogenic substances seem to be a condensed carbon ring system with polar groups containing or comprised of oxygen. The phenanthrene nucleus is always present in the more active compounds.

Some synthetic estrogenic compounds are derivatives of rather potent carcinogenic agents, while conversely some carcinogenic agents themselves possess estrogenic activity. Of the latter group we have 1:2 benzpyrene and 5:6 cyclo penteno 1:2 benzanthracene. The chief pharmacological action of these compounds is that of carcinogenesis, the estrogenic action being feeble although definite and produced with rather high dosages as compared to the dosage required for

carcinogenesis. The dibenzanthracene compound is estrogenic when in a diol form and suitably hydrogenated, while in the aromatic state it is carcinogenic. This is illustrated on Chart 3.

Ergosterol, neo-ergosterol and calciferol also cause estrus when given to laboratory animals in considerable dosages, namely about one hundred milligrams. This dosage is many times the amount essental to protect the experimental rat against rickets. This pharmacological action may be a factor in the new therapy for arthritis, which employs excessively high dosages of these and allied substances.

While the excessive cellular proliferation occurring in the early stages of estrus bears a superficial resemblance to certain phases of carcinoma, it is more likely that the chemical skeleton key comparison of Dodds (*loc. cit.*) for these related phenomena and causal compounds fits the facts better. At least the specificity of hormone action may now be severely questioned. Of interest here is the fact that certain types of cancer in the male, Owen and Cutler (15) and Fortner and Owen (16), are associated with excessive urinary outputs of water soluble estrogenic substances. It is not uncommon that reactions as given by Ascheim and Zondek (*loc. cit.*) for the diagnosis of pregnancy in the normal female, may be obtained from the urine from males suffering with teratoma or chorionepithelioma testis.

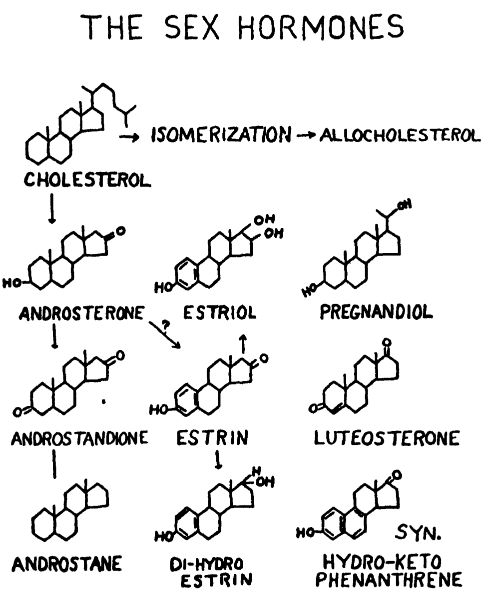

CHART 1

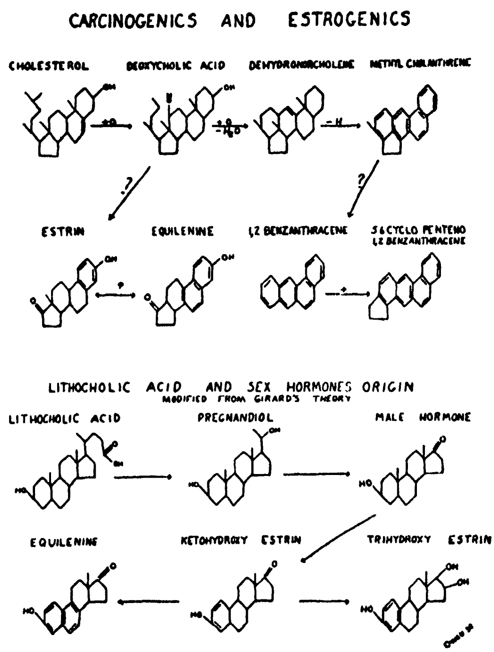

CHART 2

In the course of development and study of these hormones the work has proceded from transplants of glands, injections of extracts, chemical purification and formula determination and the artificial production of agents of known chemical formula.

CARCINOGENIC SUBSTANCES

With these materials a quite similar development period exists, dating from the observation of Yamagiwa and Ichigawa (17) that the experimental applica-

tion of tars produced carcinoid changes in laboratory animals. The fractionation and chemical isolation of the constituents led to the discovery that hydrocarbons of the polycyclic aromatic type were likely to be carcinogenic. Kennaway (18) by the heating of isoprene and acetylene in hydrogen obtained a mixture of compounds some of which were carcinogenic. Synthesis of a series of these compounds by Cook *et al.* (19) resulted in the development of substances related to benzanthracene, which possesed more or less carcinogenic activity. The most important of these are 1:2:5:6 dibenzanthracene, 1:2 benzpyrene and 5:6 cyclo penteno 1:2 benzanthracene. As is shown in the diagrams of formulas, it will be noted that these compounds possess characteristics in common with the estrogenic hormones. Similarity in formula may be carried further if one compares the male sex hormone, ergosterol, cholesterol, several of the alkaloids and other substances such as toad poisons and bile acids.

Since the carcinogenics and estrogenics seem closely related chemically and because it was possible to use some carcinogenics as estrogenics, it was only natural that scientists would attempt to use the estrogenic substances in producing or enhancing the appearance of cancer in animals. Lathrop and Loeb (20) have shown that it is possible to decrease the percentage of naturally occurring cancer, in specific strains of mice that normally show cancer by early castration or ovariectomy. Reduction of the cancer incidence rate in similar mice by the prevention of breeding is also possible. These studies suggest the possibility that the sex hormone in the female animal, at least, coöperate with hereditary factors in causing early breast cancer. The follicular hormone is known to cause mammary proliferation in experimental animals, Loeb and Hasselberger (21) and this hormone has been used in attempts to induce cancer of the breast in animals. Lacassagne (22) used folliculin benzoate, which is slowly absorbed, and reported that mammary cancer occurred in male mice which were injected with this substance. Burrows (23) using keto-hydroxy estrin in benzene has noted one instance of mammary cancer in a male

THE CARCINOGENICS

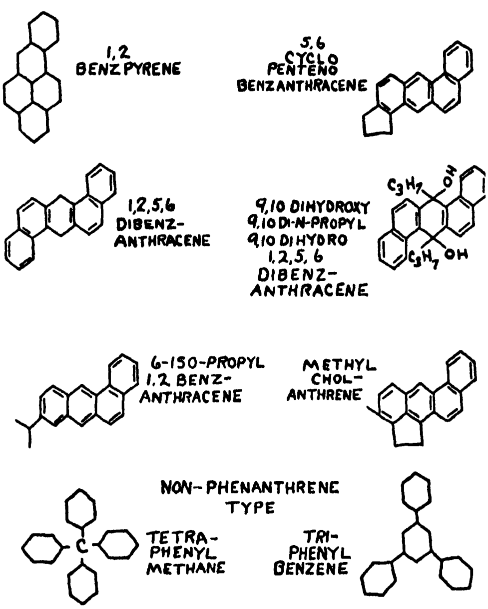

Chart 3

mouse apparently caused by repeated painting of the animal with the solution. He has also shown that prolonged administration of estrin to mice causes hyperplasia and metaplasia in the prostate gland. The latter observation has raised the question whether the benign enlargement of the prostate in elderly men may be due to the action of an estrogenic hormone, and whether this action might be reversed by appropriate balancing of

the hormones in the body by means of injections. It is well to remember that there is considerable similarity in the structure of the male and female sex hormones, and that in the female the germinal tissue produces estrin which inhibits the anterior pituitary gonadal stimulation. Androtin fails to give complete results when used in replacement therapy for castrates and here it has been noted also that a prostatic hypertrophy results following such therapy. It is apparent that benign prostatic hypertrophy may possibly be a matter of hormone imbalance, but a considerable amount of research will be essential in order to determine what corrective endocrine measures, if any, might be serviceable in this type of therapy. Preliminary findings in this laboratory indicate that patients with benign enlargement of the prostate as well as patients with a malignant prostatic condition or a hypernephroma occasionally do show an increase in the urinary output of lipoid soluble estrogenic substances. The amounts are not as exaggerated as are those of the water soluble estrogenic substances reported in cases of teratoma testis, nor does it appear probable that a simple diagnostic aid may be developed for prostatic carcinoma as has been used clinically in cases of teratoma testis, Cutler and Owen (24). Patients with benign or malignant prostatic conditions do not show an excessive urinary output of the prolans.

From a perusal of the diagrams we may postulate some reason for the occurrence of the female sex hormone in the male and vice versa. The production of estrin in male mammals and the existence of the male hormone in female tissues or fluids might be explained by assuming that estrin in the male is a transformation product of the male hormone which is produced in large quantities in the testes. Chemically a dehydrogenation of androsteron to estrin may take place, the estrin then being excreted. In females, the male hormone might be an initial state in the formation of estrogenic substances and some unconverted male hormone might be excreted. It is true that small amounts of the opposite sex hormone may be found in the urines of humans.

THE STEROLS

The complex chemical formulae of the sterols has necessitated a slower develop-

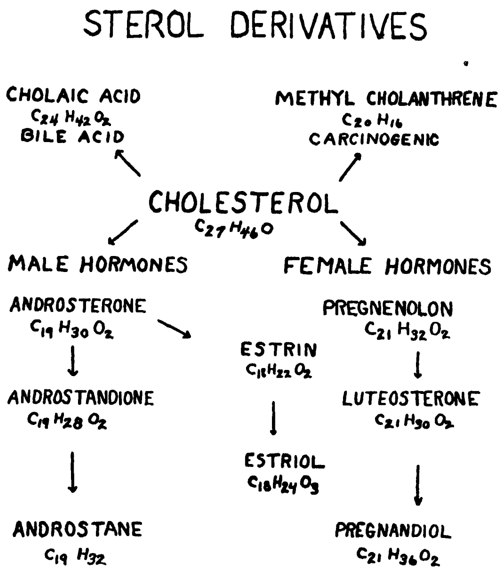

CHART 4

ment of knowledge regarding them. The relationship of sterols to hormones was first shown by Butenandt (25) who was able to produce 1:2 dimethyl phenanthrene from folliculin and from a stearin derivative. This observation at once connected these important hormones with matter common in all tissues, the sterols. Methyl cholanthrene, a highly active carcinogenic chemical can be obtained from sterols as is shown in the diagrams, thus the carcinogenics and the estrogenics may be obtained from a common chemical substance which

exists in body tissues. The reactions essential in forming the carcinogenic substances from cholesterol, for instance, constitute such chemical changes as are common in the body for other molecules. These changes are mainly ones of oxidation, reduction, dehydrogenation and dehydration. Whether the body metabolism can effect the conversion of a straight chain into a carbon ring compound as is illustrated for the cholesterol changes, is still questioned although Edisbury *et al.* (26) believe they have proof that a similar cyclization of a highly unsaturated compound does occur.

It seems entirely probable that the various sex hormones are prepared in the body from the sterols. It is also possible that the carcinogenic agents might be formed in the body cells by a degradation metabolism of the sterols. Here it may be of interest to point out that the cholesterol content of tumors is very high and in spite of a fairly high phospholipid content, the ratio of cholesterol to phospholipid is unusually large. Various workers have made extracts of tumors, more especially the lipomas for sex hormone content and have reported finding significant amounts of these endocrines. More controls are still needed.

DISCUSSION

Rather weak although definite carcinogenic effects have been noted for chemicals not possessing the phenanthrene nucleus, Morton *et al.* (27) who have shown that symmetrical triphenyl benzene and tetraphenyl methane will produce cancer in mice. Thus the field of carcinogenics may be more broad than hitherto supposed.

The assistance in the production of cancer, given by the sex hormones seems limited to the sex organs or to the secondary sex organs, and requires periods of application of some time. The carcinogenic material from tar or as prepared synthetically, is not specific as to field of action for cancerous changes are induced in the tissues which have long contact with the agent. These agents are also more certain in action than the hormones. For both types of material,the longer the stimulus is applied the greater is the incidence of cancer and the earlier the cancer will appear. There is a preparatory period for both types of chemicals after which the agent need not be applied as the cancer conditions will develop in the course of time although no such change may be apparent at the time of discontinuing the applications. Irritation and its concomitant inflammation are usually absent although it is believed that some gradual cellular changes must be induced in each instance.

The mechanism underlying the cancerous changes induced by the carcinogenics and estrogenics seem to depend upon proliferative processes extending over a long period of time but the final action apears to be indirect. One of the basic etiological factors may include an altered cellular metabolism for the sterol.

SUMMARY

1. A discussion of the carcinogenics and the estrogenics is presented from the chemical and biological angles.

2. The relationship of the sterols to the sex hormones and to the carcinogenics is illustrated.

3. Artificial sources of the hormones are given and these are compared to the sources of carcinogenic substances.

4. The possible rôle of sterol metabolism and cellular dysfunction in sterol degradation is presented as a probable source of carcinogenics and estrogenics in the body.

5. The common chemical nucleus of the sterols, carcinogenics, sex hormones,

vitamins, bile salts and acids and of some common alkaloids is illustrated, and discussed.

This paper is published with the permission of the medical director of the Veteran's Administration, who assumes no responsibility for the opinions expressed or conclusions drawn by the author.

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CONTROLLING FACTORS IN DROSOPHILA POPULATION GROWTH

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PEARL'S studies of *Drosophila* populations have established the fact that the logistic equation describes the growth of such populations with reasonable accuracy. He has further demonstrated that increasing density of population has definite effects on fecundity and fertility in *Drosophila*. It seems worth while to consider how far the facts and relations so far determined are adequate to account for the regulation of population numbers.

A few words as to the experimental technique are called for. The experiments are of three kinds. There are, first, experiments on population growth (1), in which a small initial population is placed in a bottle with standard food substrate (banana-agar seeded with yeast) and subsequent census counts taken. Second, there are experiments on mortality (2). In these adult flies are taken at emergence and placed in one-ounce vials. Thereafter deaths are noted as they occur; the flies are periodically transferred to fresh bottles. Third, there are fertility experiments. Of these there are two types, dealing with net fertility (imagoes produced per female-day) and with fecundity (eggs produced per female-day). In the net fertility experiments (3, 4) adult flies are taken at emergence and placed in milk bottles with the standard agar-yeast substrate, left a few days and transferred to new bottles. The progeny flies are counted on emergence. In the fecundity experiments (5), by one or another device it is provided that the flies shall oviposit on a surface which can be removed from the bottle, so that the eggs can be counted.

We may now attempt some analysis of these experimental results, with a view to determining how far the facts which have been established about fertility and mortality are adequate to account for the results obtained in the population growth experiments. The computations are necessarily rough, but should give satisfactory estimates of the orders of magnitude of the quantities involved.

We may consider first the growth of experimental populations of *Drosophila*. It appears that such populations grow along a sigmoid which may be fitted adequately with a logistic. The limiting population, in half-pint bottles on banana-agar, is of the order of 210 flies per bottle (Pearl, 1, p. 35). We do not know the sex-ratio; we shall assume, when necessary, that half of these are females. [From L'Héritier and Teissier's work (6) we might expect that more than half would be female.]

Consider now what can be said of this asymptotic density of 210 flies per bottle. First, what will be the egg production at this density? Using Pearl's (5) equation (1), and extrapolating to a density of 210, we obtain a predicted egg production of about 244 eggs per day. This figure is not more than an indication of order of magnitude, for several reasons. First, it is based on the first 13 days after emergence, and makes no allowance for the age composition of the actual population. Second, synthetic food, changed daily, was used. Third, the food area was about one-quarter of that in the population experiment. Fourth, the extrapolation involved is considerable. It would

not be surprising if the true figure differed from this by a factor of two or three.

We cannot use this figure of egg production directly, because we have no direct information on pre-imaginal mortality. We have, however, figures (Pearl and Parker, 3) which give us net fertility (i.e. adult flies emerging) as a function of population density. [We use these rather than the Pearl, Allen, and Penniman figures of 1926 (4) as being more closely comparable with the population growth experiments.] With these 1922 data, extrapolating to density 210 by Pearl's equation (xxxiii), we find a total of 20 imagoes per day emerging. This figure is probably as reasonable as we can get. It corresponds, in a stationary population of 210, to a mean imaginal duration of life of about 10.5 days.

We should now naturally inquire as to the expectation of life of *Drosophila* in half-pint bottles on banana-agar at a density of 210 flies per bottle. Unfortunately Pearl's life table data are all obtained using one-ounce vials, and one is in considerable doubt as to what sort of correspondence to establish with half-pint bottles. If we asume that flies per unit volume is the significant measure of density, then we should use the figures for density 30–35 flies in the one-ounce vials. If we assume that flies per bottle is controlling, we should use the figures for 210 flies per one-ounce vial. The truth probably lies between these extremes. If, however, we take some such figure as 100 flies per ounce vial, we find the further difficulty that we have no figures for constant density; all the data were obtained for a given initial density, which continuously decreased as flies died off. The best that we can say is that under these conditions, expectation of life lies somewhere between 40 days and 10 days. This is not too far from our previous figure of 10.5 days in the stationary population.

We next observe that there is a wide discrepancy between the figure of 244 eggs per day and 20 emerging adults per day. On their face, these figures indicate a pre-imaginal ("infant") mortality of around 90 per cent. If we scale down the eggs by a factor of three, we still have a mortality of around 70 per cent. Pretty clearly, there must be a large infant mortality, which, in any complete study of population dynamics, would need careful evaluation.

We can perhaps estimate the order of magnitude of this mortality more directly by a comparison of the density-net fertility figures with the density-fecundity figures. We use the figures of progeny per female-day on synthetic food (Pearl, *et al.*, 4) rather than the 1922 figures because the egg production figures are obtained with synthetic food; and the figures of eggs per female-day (Pearl, 5).

DENSITY (FLIES/BOTTLE)	PROGENY/ FEMALE-DAY	EGGS/FEMALE-DAY
2	34.4	19.4
4	25.1	17.4
8	11.1	14.2
16	4.9	13.5
32	1.9	8.2
64	1.0	4.4
128		2.8
256		2.8

It is evident that these figures are not directly comparable, since they purport to show more progeny appearing at low densities than there were eggs laid. However, we have to remember that for technical reasons (for which see Pearl, 5) the area available for oviposition in the egg-production series was only about one quarter that in the progeny series. Now according to Pearl (5) the significant measure of population density for egg-production is flies per unit food area. We may therefore reasonably compare a given density of egg production with a

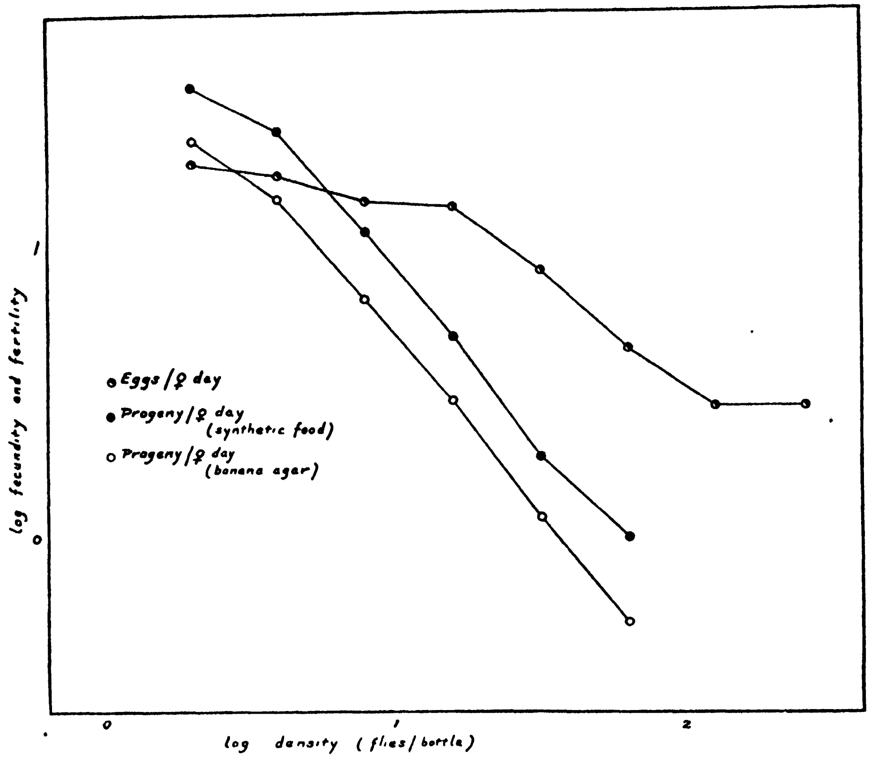

FIGURE 1

nominal density in the progeny series four times greater, and set up our comparison thus:

DENSITY (EGG PRODUCTION)	EGGS FEMALE-DAY	DENSITY (PROGENY SERIES)	PROGENY/ FEMALE-DAY	PRE-IMAGINAL MORTALITY
		2	34.4	
		4	25.1	
2	19.4	8	11.1	43%
4	17.4	16	4.9	72%
8	14.2	32	1.9	87%
16	13.5	64	1.0	93%
32	8.2			
64	4.4			
128	2.8			
256	2.8			

All these calculations are only fit to give orders of magnitude, if we remember the approximations which have been necessary. On the whole, however, they agree rather well together, and the results seem not too improbable. If they are at all near the truth they indicate (1) that there is a high infant mortality in *Drosophila* under conditions of crowding; and (2) that this mortality is markedly affected by density. It would appear that this is very probably an important factor in the control of *Drosophila* populations—very probably of more importance than imaginal death-rates as affected by density, or than fecundity rates as affected by density.

Another way of making the comparison is by plotting the density-fertility and density-fecundity results on double logarithmic paper. In such a plot the significant comparison is between slopes, not between absolute positions relative to

either the vertical or horizontal scales. It is at once clear that the density-net fertility curve is much steeper than the density-fecundity curve, indicating the existence of a component of infant mortality which increases with increasing density of population.

We may perhaps note also the results which L'Héritier and Teissier (6) have briefly reported. By a technique due to Pearl, *et al.* (2) *Drosophila* populations are maintained at saturation levels, with a constant supply of fresh food. In their experiments a saturation population of 3290 imagoes was reached. (Of these about two thirds were females, due to higher mortality among male larvae.) The rate of emergence of adults averaged 141 per day, giving a mean duration of life of 23.3 days. The pre-imaginal mortality was from 98 to 99 per cent. This indicates an egg production per female-day of somewhere from 3.2 to 6.4 eggs. Unfortunately the information as to the conditions of the experiment is insufficient to permit any real comparison with Pearl's results; it would appear, however, that there are no vital discrepancies.

To summarize: the data on fecundity and fertility relative to density of population in *Drosophila* indicate a pre-imaginal mortality varying markedly with density. This mortality may reach 90 to 99 per cent at high densities such as correspond to saturation levels of population. It seems highly probable that this mortality is a factor of major importance in the regulation of numbers.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

WHY BE AN APE? *Observations on Evolution. By A London Journalist. Preface by Admiral Sir George F. King-Hall. Illustrations by W. D. Ford. Marshall, Morgan and Scott, London; Zondervan Publishing House, Grand Rapids.* $1.00. $7\frac{1}{4} \times 4\frac{7}{8}$; 144; 1936.

The author of this work is one of those strange creatures that delight in trying to fan into flame the dying embers of a nearly forgotten conflict of bygone ages. There is no good reason why such a book should ever be written. Vituperative and intolerant language is never becoming to any author, least of all to one actuated by religious motives, for we naturally expect a more sympathetic attitude toward, and a greater toleration for, the religious beliefs of others from a follower of the Man of Galilee than from the crass materialist.

That modern thinkers are not unanimously agreed as to all details of evolution no one will deny. Had the author of this work (who for obvious reasons prefers to remain anonymous) been content to attack a single phase of the subject he might conceivably have made out a plausible case, but instead he undertakes to demolish the entire structure, and so draws heavily on every scientific writer of any note, selecting and isolating odd texts here and there, discarding what he cannot use, in order to forge an instrument wherewith to confute other writers. Onto his *index expurgatorius* go the technical works of Darwin, Weismann, DeVries, and MacBride; and also the more popular but not less scientific works of Fiske, Drummond, and LeConte. If he were consistent the book of Genesis would also have to be consigned thither willy-nilly, for the Elohistic narrative of creation is frankly evolutionary in declaring that man is the culmination of a long series of upward steps from the lowest forms of life.

To such an author it must be disconcerting to find passages in the Psalms, the books of Deutero-Isaiah and Jeremiah, and the Epistle to the Romans, which indicate how receptive to the evolutionary point of view the minds of the writers of these books were, and it must be even more so to remember that among the ecclesiasts of a later day who were philosophical evolutionists must be included Gregory of Nyssa, Augustine of Hippo, Albertus Magnus, Thomas Aquinas, Francis of Assisi, and Giordano Bruno. One wonders if he realizes that responsibility for the adoption by the Christian Church of the doctrine of special creation (a doctrine, incidentally, derived not from Christian or Hebrew sources, but from Plato) rests with Francisco Suarez, *S.J.*, a devoted disciple of the philosophy of Scholasticism with its insistence on the immutability of types, who died only three short centuries ago?

Lest it be thought that too much space

has been given to discussing what the author should have said, and too little to what he really has said, a brief quotation which seems to epitomize the whole thought of the book may appropriately be cited here: "Really, if believing in the ape ancestry of man is humility, and accepting the Bible's declaration of his Divine ancestry is snobbery, I'm for snobbery every time!" Perhaps we have here the explanation of how books like this get written. Perhaps they are the natural product of snobbish intellects that seek to conceal inferiority complexes by rushing in blindly and recklessly where angels are said to tread with humility and reverence.

If this work should win any considerable number of adherents—which is unthinkable—the great loss would be not to science but to religion.

Die Holarktis. *Ein Beitrag zur diluvialen und alluvialen Geschichte der zirkumpolaren Faunen- und Florengebiete.*

By W. F. Reinig. Gustav Fischer, Jena. RM. 7.50. 10 x $6\frac{3}{4}$; vii + 124; 1937 (paper).

The author has attempted to explain the relationship of the flora and fauna of the three continents of the Northern Hemisphere as residues of the Tertiary life brought southward from the Arctic regions with the glacial invasions. He recognizes two distinct regions of plains; one the inner-Asiatic steppes, and the other designated as the sonoral, in America. The four major wooded regions, namely the European-West-Asiatic, the Manchurian-Siberian, the American-Pacific, and the New-England-Canadian, he finds more closely related to each other. The formation of the present topography and vegetable and animal life of the arctic tundras he places in the Pliocene. Hofmann's statistical method of comparison of numbers of species from the same origin in the various regions, combined with an historical study of the movement of the ice-fields, were utilized in this study. A bibliography and author and subject indices are provided.

Jouannet. *Grand-Père de la Préhistoire.*

By André Cheynier. Imp. Chastrusse, Praudel et Cie, Brive. 15 francs. 10 x $6\frac{1}{2}$; 101 + 3 plates; 1936 (paper).

In this little book Doctor Cheynier fulfils the pious task of rescuing from undeserved oblivion an archeologist who in the generation before Boucher de Pesthes began his work had discovered and described a rich neolithic station with its pottery, polished axes and arrow heads; had discovered caves inhabited by paleolithic man; had recorded the association of fossil bones with chipped flints; had made experiments on the chipping of flints; had classed ancient times into ages of stone, of bronze and of iron; had divided the stone age into two periods; had demonstrated the origin of bronze axes in those of polished stone, and of bronze daggers in those of chipped stone; had discovered the first station of the Solutrean period; and had had chemical analyses made of fossil bones and of the bronze of the ancient axes. The book includes a biographical sketch of Jouannet, a reproduction of his works on prehistory and a bibliography of his writings.

GENETICS

Grundzüge der Vererbungslehre.

By Friedrich Alverdes. S. Hirzel, Leipzig. 5 marks. $8\frac{1}{2}$ x $5\frac{3}{4}$; viii + 143; 1935 (paper).

A clear and thorough introduction to genetics is here presented, in easy style. Three general chapters cover such topics as cells, cell division, multicellular organization, fertilization, continuity of the germ-plasm, somatoplasm, nature and nurture, definitions of race and species, etc. Four chapters on Mendelism complete almost half of the book. Further chapters are devoted to heredity and sex, variation of phenotypes, of genotypes, mutations, multiple factors. Two final chapters concern human heredity, with remarks on twinning. The rôle of environment is considered, but not in detail. The discussion is always generous and a large number of practical examples are cited for the main topics; but many

important topics are omitted, particularly those of recent acceptance. Forty-five diagrams and sketches of cells are included, also a 6-page index.

INHERITANCE AND EVOLUTION.

By W. F. Wheeler. With a Foreword by E. B. Ford. Methuen and Co., London. 3s. $7\frac{3}{8}$ x $4\frac{3}{4}$; xii + 116; 1936.

This is a brief but well-written summary of the relation of cytology to genetics; of the operation of Mendel's laws and their subsequent consequences; and of the rôle of inheritance in evolution. The material is essentially comparable to that studied by the college sophomore but has the virtue of a certain up-to-dateness and accuracy frequently lacking in general biological texts. Each chapter contains a short bibliography and the book boasts an adequate index and glossary. There are a few well-chosen diagrams in the text that aid in understanding the discussions. The book should prove useful to students wishing to review their elementary genetics and to the intelligent layman desiring orientation about heredity and its general significance.

PROBLEMS IN HUMAN HEREDITY. (*Learning by Thinking.*)

By Vincent W. Jackson. Vincent W. Jackson, University of Manitoba, Winnipeg, Canada. 50 cents. 10 x $6\frac{3}{4}$; 48; 1937 (paper).

A laboratory outline of genetics and eugenics with brief explanations and diagrams and numerous questions for the student to solve or discuss.

GENERAL BIOLOGY

TRAVAUX DE LA STATION BIOLOGIQUE DE SÉBASTOPOL, *Tome V.* Containing the following articles: *Beobachtungen über die pelagischen Eier der Fische des Schwarzen Meeres*, by W. Vodjanitzki; *Essai d'une Évaluation quantitative de la Végétation du Fond de la Mer Noire*, by N. Morosova-Vodjanitzkaja; *Phytobenthos des Karkinitzky Busens*, by N. Morosova-Vodjanitzkaja; *Die Algen der Umgebung der Biologischen Station Karadag*, by N. Morosova-Vodjanitzkaja; *Some data concerning the Development of Delphinus delphis L.*, by E. N. Malm and K. T. Trotzkaya; *De l'Origine bactérienne du Pigment rouge de l'Amphioxus lanceolatum, Cause de sa Mort et Destruction*, by J. Ravitch-Stcherbo; *A Review of the European Species of the Genus Trigla (Pisces, Triglidae)*, by A. N. Svetovidov.

Academy of Sciences SSSR, Moscow and Leningrad. $9\frac{3}{4}$ x $6\frac{1}{2}$; 349; 1936.

The first paper in this collection of contributions to the marine biology of the Black Sea is a study of about forty species of pelagic eggs including several not hitherto described. The second paper gives a quantitative evaluation of the vegetation on the bottom of the Sea, with a discussion of the seasonal changes and adaptation. This is the longest paper of the lot (175 pages). Fifty-five species of macrophytes and their relative densities in the Karkinitsky Bay are the subject of the third paper. The fourth article presents quantitative studies of the algae found on five different types of sea-bottom in the region of Karadag—gravel, rocks and stones of volcanic origin, rocks and stones of sedimentary origin, sea-sand, and sand and slime brought in from rivers. The development of *Delphinus delphis* L., one of the most widely distributed species of this sea, during foetal life and the first six months of post-natal life is the subject of the fifth paper. In the sixth study it was found that the red pigmentations that are produced by bacilli and cause the death of *Amphioxus lanceolatum* in aquaria were not observed on bacilli infested catches from the sea, and it was therefore concluded that these bacilli are only pathogenic under artificial conditions. The last paper describes seven species of *Trigla* found in the Mediterranean Sea. Three of these have also been observed as migrants in the Black Sea.

The papers are in Russian, with English, French or German summaries as indicated in the title above.

BIOLOGY. *A Study of the Principles of Life for the College Student.*

By U. A. Hauber with the collaboration of

M. Ellen O'Hanlon. F. S. Crofts and Co., New York. $3.90. 9 x 6; xii + 559; 1937.

The present trend in the teaching of biology is away from the technical and toward the cultural aspect of training for a scientific or professional career. In the preparation of this new text, the authors have very clearly and successfully emphasized this trend.

The course is built around the important biological principles rather than on the study of types, although due emphasis is placed on the fact that a principle may become a mere memorized formula unless the student himself observes its manifestations in a concrete organism. The book is designed primarily for a full year course for college freshmen with the assumption that laboratory work will be included. The many drawings and diagrams, the extensive glossary, the table of contents and the complete index will find favor with the student who is embarking on his first course in college biology.

PICTURING MIRACLES OF PLANT AND ANIMAL LIFE.

By Arthur C. Pillsbury. J. B. Lippincott Co., Philadelphia. $3.00. $8\frac{1}{4}$ x $5\frac{1}{2}$; 236; 1937.

Author Pillsbury is a professional photographer who became interested in biology as a result of trying to take action pictures of the side of a very sedentary mountain—at least so he says. It was for this purpose that he first developed the lapse-time motion picture camera to photograph the beautiful movement of shadows of clouds on a cliff of the Sierras in Yosemite National Park. Having developed the technique, he used it to speed up the opening of a rose with such good results that he has been taking motion pictures of slow plant and animal movements ever since. His present book is written in the style of an amateur bubbling over with enthusiasm for his favorite hobby and the spirit is readily transmitted to the reader. There is little technical description, the text being mainly a series of adventure stories with the author bragging pleasantly about his many accomplishments. He does tell in sufficient detail how each of his experiments may be repeated but unfortunately for those who would like to take up the sport themselves, the equipment costs anywhere from $1,000 up. His chapter on soil-less "chemical farming" by which 2,465 bushels of potatoes may be raised per acre is fascinating and sounds much more hopeful as a prospective hobby for even a city dweller, as little space is required and the necessary ingredients may be had for a dollar or less.

PLANKTONKUNDE FÜR JEDERMANN. *Eine erste methodische Einführung für den praktisch arbeitenden Hydrobiologen. Handbücher für die praktische naturwissenschaftliche Arbeit Band 26.*

By Willy Baumeister. Franckh'sche Verlagshandlung, Stuttgart. RM. 2.80 (in Germany); RM. 2.10 (outside of Germany). 10 x 7; 62; 1936.

Here we have a complete introduction to the study of the plankton. From it we may learn how to collect and observe living plankton; how to prepare specimens for microscopic study; how to use and care for the microscope and something about microscopic photography; and how to identify some of the commoner kinds of freshwater plankton of both kingdoms. There is also a chapter on freshwater plankton ecology in which twenty different kinds of environment are discussed. Perhaps to some this part of the work will seem too detailed, for there is a distinction drawn between fish ponds and goose ponds; and even roof gutters, burial vaults, and flax retting vats come in for their share of attention. The illustrations leave nothing to be desired, and there is an index of four pages and a bibliography of three. It is unfortunate that books similar to this are not produced on this continent.

HYDROGRAPHY OF MONTEREY BAY, CALIFORNIA. THERMAL CONDITIONS, 1929–1933. *Transactions of the American Philosophical Society. New Series, Volume XXIX.*

By Tage Skogsberg. University of Pennsylvania Press, Philadelphia. $3.00. 12 x 9¼; 152; 1936 (paper).

From the point of view of hydrography, the Monterey Bay region had been practically unexplored until this survey was started in 1929. This report, largely descriptive, is concerned with the first five years' survey of the thermal conditions. While all of the data obtained during the period has been analyzed, only a limited portion of the findings are presented at this time. (The original data are available at the Hopkins Marine Station, Pacific Grove, to those especially interested.) Sufficient tabular matter has been included, however, so that together with the 45 figures (graphs, map outlines, etc.) the reader obtains a clear and interesting picture of the five years' work.

PHILOSOPHIE DER NATURWISSENSCHAFTEN.

By Max Hartmann. Julius Springer, Berlin. RM. 3.60. 10 x 6½; 46; 1937 (paper).

This is a reprint, with a few annotations and a bibliography added, of an article published in a *Festschrift* in celebration of the twenty-fifth anniversary of the *Kaiser Wilhelm Gesellschaft zur Förderung der Wissenschaften*. The fundamental theme is that "in spite of all the differences between physics and biology, the same philosophical bases underly both, and the same methods of research are practicable."

URDEUTSCHLAND. *Deutschlands Naturschutzgebiete in Wort und Bild. Lieferungen 24.*

By Walther Schoenichen. J. Neumann, Neudamm. 2.50 marks. 10¾ x 8¼; 281–342 + 8 plates; 1937 (paper).

This is the last number of Volume II of the carefully written and beautifully illustrated work on the natural history of Germany. Notices of previous numbers have appeared in this REVIEW from time to time. The present number describes the more rare and beautiful flowers and birds native to Germany.

HUMAN BIOLOGY

AFTERMATH: *A Supplement to The Golden Bough.*

By Sir James G. Frazer. The Macmillan Company, New York. $3.00. 8½ x 5½; xx + 494; 1937.

So the literature of cultural anthropology grows new data on magic and its related subjects accumulate. Instead, however, of issuing a new edition of *The Golden Bough* Sir James Frazer has gathered this new material into a supplementary volume. On the whole he finds that the new data support the conclusions that he had already drawn.

> But now, as always, I hold all my theories very lightly, and am ever ready to modify or abandon them in the light of new evidence. If my writings should survive the writer, they will do so, I believe, less for the sake of the theories which they propound than for the sake of the facts which they record. They will live, if they live at all, as a picture or moving panorama of the vanished life of primitive man all over the world, from the Tropics to the Poles, groping and stumbling through the mists of ignorance and superstition in the eternal search after goodness and truth. When I first put pen to paper to write *The Golden Bough* I had no conception of the magnitude of the voyage on which I was embarking; I thought only to explain a single rule of an ancient Italian priesthood. But insensibly I was led on, step by step, into surveying, as from some specular height, some Pisgah of the mind, a great part of the human race; I was beguiled, as by some subtle enchanter, into inditing what I cannot but regard as a dark, a tragic chronicle of human error and folly, of fruitless endeavour, wasted time, and blighted hopes. At the best the chronicle may serve as a warning, as a sort of Ariadne's thread, to help the forlorn wayfarer to shun some of the snares and pitfalls into which his fellows have fallen before him in the labyrinth of life. Such as it is, with all its shortcomings, I now submit *The Golden Bough* in its completed form to the judgment of my contemporaries, and perhaps of posterity.

INTERRACIAL MARRIAGE IN HAWAII. *A Study of the Mutually Conditioned Processes of Acculturation and Amalgamation.*

By Romanzo Adams. The Macmillan Co., New York. $4.00. 8½ x 5¾; xvii + 353 + 11 plates; 1937.

No less than eleven racial classes of significant numerical proportions besides a group of a few hundred Negroes are distinguished in the census of Hawaii's population of about 350,000 people. These include whites of several nationalities,

some of whom have Negro and American Indian blood, Hawaiians, Filipinos, Chinese, Koreans, and Japanese, the latter being by far the most numerous. The greatest amount of racial mixture has taken place between the native Hawaiians and other groups, usually from marriages between Hawaiian women and foreign men, there being approximately 22,600 Caucasian-Hawaiians and 15,600 Asiatic-Hawaiians on the islands today. The latter are mostly Chinese-Hawaiian mixtures, the Japanese out-marrying less than the other groups. But all sorts of other combinations also occur. When the whites first discovered the islands, a number of sailors married native women of chiefly class, were accepted into the upper class, and their mixed breed children inherited large estates and in consequence high position from their mothers. As a result, for many years there was no social stigma attached to miscegenation, which was practiced freely as a result of the fact that the immigrant laborers who later arrived in large numbers brought very few women of their own race with them. The relative absence of sensitiveness on account of mixed blood makes it quite easy to collect data on the ancestry of individuals and as immigration has now almost stopped the islands are an almost perfect place to study the physiological, intellectual, and sociological effects of nearly every conceivable type of racial mixture. Mr. Adams has done a remarkably wonderful piece of work in analyzing the situation. The status of each important type of mixed and "pure" group is discussed from the standpoint of social-economic position, attitudes of the group, and attitudes of other groups toward it. In general, he finds that the hybrid individual is apt to be intermediate in every respect between the two races from which he comes.

Moslem Women Enter a New World. *Publications of the American University of Beirut Social Science Series No. 14.*

By Ruth F. Woodsmall. Round Table Press, Inc., New York. $3.00. $8\frac{1}{2} \times 5\frac{1}{2}$; 432 + 30 plates + 1 folding map; 1936.

This is an extraordinarily interesting book, that will serve as a useful source to sociologists and human biologists wanting to know what is going on in the Moslem world so rapidly changing today. The author is General Secretary of the World Y.W.C.A. Nine years' experience in Y.W.C.A. work in the Near East after the War gave her a solid background of experience upon which to build her treatise. She gathered her material for the book while holding a Traveling Fellowship of the Rockefeller Foundation. She proves herself to be a careful and acute observer. There is occurring now in Egypt, Palestine, Syria, Trans-Jordan, Turkey, Iraq, Iran, and India a social change of the greatest significance so far as concerns Moslem women. They are being rapidly freed of the crushing restrictions that have overwhelmed them for ages past. Educational facilities for women are everywhere developing, the old collective type of family life is being replaced by the individual home, polygamy is on the way out, and generally *incipit vita nova*.

The chief defect of the book is in the diffuseness of its style. There seems to be something in the conditioning and training of social workers that almost invariably makes them a bit on the gabby side when their efforts reach the stage of exposition. The kernel tends to hide itself in too thick a verbal husk. But in Miss Woodsmall's case the enormous interest of her material brings forgiveness for the sin of loquacity.

There is an adequate index, and a route map of her travels.

The Social Consequences of the Economic Depression. *Studies and Reports Series C (Employment and Unemployment) No. 21.*

By Wladimir Woytinsky. International Labour Office (Washington Branch), Washington, D.C. $2.00. $9\frac{1}{2} \times 6\frac{1}{4}$; xi + 364; 1936 (paper).

This study, based on official statistics of prices, unemployment, production, etc., describes the course of the economic depression and how it has affected industry, commerce and agriculture, industrial and agricultural countries, and the different economic classes of people. The high de-

gree of interdependence between countries and between the different economic classes of the same country is considered the most important element in the diffusion of the crisis. Thus, while conditions in industry were the immediate cause of the depression, agriculture was affected almost at once. Similarly, agricultural countries have borne a good portion of the loss sustained by the industrial nations, and in each country the net result of the economic crisis has been a general lowering of the standards of living. What classes have lost most because of the depression? Not the capitalists, nor the farmers, nor the small business men, but instead, the laboring classes and persons with small fixed incomes. In the author's opinion, since contraction of industrial production was the principal factor of this crisis, the means used by the governments to alleviate conditions have been in general useless because they have been mostly directed to aid agriculture. As he sees it, all efforts should have been made to increase industrial production. Notwithstanding the obvious limitations of the material, the author is able to draw conclusions consistent with well established facts. They are of interest and should serve as a starting point for further research.

MARRIAGE CONDITIONS IN A PALESTINIAN VILLAGE, II. *Societas Scientiarum Fennica. Commentationes Humanarum Litterarum, Tomus VI, Nr. 8.*

By Hilma Granquist. Akademische Buchhandlung, Helsingfors; Otto Harrassowitz, Leipzig. RM. 9.60. 9¼ x 6½; 366; 1935 (paper).

The author lived for some time in the Mohammedan village of Artas in Palestine. There she noted the customs of the people and having acquired friends among the natives is able to interpret her findings with unusual clearness and authority.

In this volume, the second of a series, the author describes the procedures of betrothal, the preparations and the festivals preceding a wedding, the marriage contract and ceremony. In the second part of the book she discusses the rights and obligations of the marital partners with regard to themselves and to their respective families; the position of the wife in cases of monogamous and polygamous marriages; problems of separation and divorce; and the status of the widowed. Illustrative cases are reported in detail. There is an extensive bibliography and the author also compares her observations with the reported customs of other villages and localities in Palestine.

This is without doubt a first-class ethnological study and should prove of interest to all biologists, whatever their specialty. The descriptions clearly show the fundamental similarities of certain patterns of human social behavior. For example, the threat: "I am going home to mother" (substitute father or brother for mother) is apparently just as effectively and frequently made by the Mohammedan bride as by her Christian sister.

THE BIOLOGICAL BACKGROUND OF HUMAN POPULATION THEORY.

By F. S. Bodenheimer. Palestine. 9 x 6; 157; 1936. [In Hebrew].

This volume on human population theory by the distinguished Professor of Biology in the University of Jerusalem merits translation into a more generally accessible language than Hebrew. It opens with a general introduction into the problems and achievements in the field of infra-human animal population theory, experimentation, and observation. It then summarizes and discusses the influence of environmental factors upon human beings, as individuals and as group civilizations. The work of Pearl on the problems of population, human and experimental, is then described at some length, and, following that author's lead, the data of the biology of sex and reproduction are brought into direct relationship to population problems and theories.

The latter half of the book is devoted to such matters as the criticism of Malthus's position from the viewpoint of the biologist and critical student of modern methods of population limitation; business cycles as viewed by the animal ecologist; and finally the problems of modern eugenics and "nature and nurture" in human life.

There is a short but well-selected bibliography, but no index.

A first-rate contribution.

MEN, MEDICINE AND FOOD IN THE U.S.S.R.
By F. Le Gros Clark and L. Noel Brinton. Lawrence and Wishart, London. 5s. net. $7\frac{1}{4} \times 4\frac{7}{8}$; v + 173; 1936.

This book we highly recommend to such of our readers as wish to get an objective picture of the life of the Russian people today, and what biologist does not? The authors are sympathetic, and even in some degree enthusiastic about the aims of the Soviet leaders. This occasionally, but only occasionally, leads them to make absurd statements, such as that "the Soviet system has at last brought to birth the science of preventive medicine." But in general the book is a calm and dispassionate account of what the authors (one of whom speaks Russian) found out regarding the sort of things the biologist would like to know about—food, health, the practical details of the organization of scientific work in a country where people and state have a deep and pervasive respect for science. A basic article of faith in their social philosophy is that "Science will mould the world into a place worthy of human habitation and will mould and remould it again past all our present imaginings. It will also fashion a humanity worthy of the new world. Thus the scientist today, as scientist, embodies to him the sound wisdom that is to set mankind upon the fresh path of development."

The book has a short bibliography, and is well indexed.

RECOLLECTIONS AND REFLECTIONS.
By Sir J. J. Thomson. The Macmillan Company, New York. $4.00. $9\frac{1}{4} \times 5\frac{3}{4}$; viii + 451; 1937.

One of the most interesting parts of this book is the series of character sketches of the scholars and research men with whom the great physicist came into contact during his student days in England, and later in the course of his long and distinguished career. Entertaining accounts are given of the undergraduate life at Cambridge; the activities of research students, and of the organization of the university research laboratories; "J.J.'s" work with the British government during the World War; and of his first visits to Canada and to the United States. The author has relatively little to say about American universities, but does have an amusing passage about Baltimore hospitality and the evident pleasure of life and living there generally.

The last quarter of the volume is a description of the fundamental researches in the Cavendish laboratory, over a long span of years, on electronic waves, röntgen rays, electrolytic dissociation, the theory of light, relativity, etc. An impressive list of Cavendish students who have become Fellows of the Royal Society has been added. There are name and subject indices.

THE EARTH GODDESS. *A Study of Native Farming on the West African Coast. Royal Empire Society Imperial Studies, No. 12. By G. Howard Jones. Longmans, Green and Co., New York and London.* $5.00. $8\frac{5}{8} \times 5\frac{1}{2}$; vii + 205 + 8 plates; 1936.

It is high time in the opinion of the author that some thought should be given to the future of agriculture on the West African Coast—a strip of land about a hundred and fifty miles broad stretching from the western seaboard of Sierra Leone to the eastern boundary of the Cameroons.

Deep religious convictions and tribal customs of the natives of the region together with a climate not well suited to the white man have hampered the European in inaugurating his more highly approved systems of agriculture. Changes must be wrought however as the demand for exports increases, and the author who has had years of experience and of study in the field of agriculture in Africa and in Europe believes that the most favorable line of development lies in some system of coöperative farming. He has arrived at his conclusions painstakingly, taking into consideration the temperament of the natives and the dangers of a social upheaval likely to follow in the wake of any drastic move.

THE KEY TO SWEDEN.

By Serge de Chessin. Translated from the French by Alice S. von Holstein. Fritzes, Stockholm. Kr. 6. $8\frac{3}{8} \times 5\frac{1}{2}$; 233 + 10 plates; 1936 (paper).

"Mounted on a wild goose" one flies quickly over Sweden in the first chapter, catching delightful glimpses of the varied life and country-side of this picturesque land. After the goose ride we are turned loose to get a closer and more intimate view of the character, lives, customs and institutions of this stalwart blond race, but always we have our guide—the author—along with us who interprets for us many of the things we might prefer to interpret for ourselves. We might doubtless in the end come to many of the same conclusions concerning this wonderful country and her superior people, but still it would have been more fun to poke around a bit and get views of the same things occasionally from different angles; for it is always from the spiritual heights that our interpreter speaks and his head is always in the clouds as far as Sweden is concerned. But in spite of this *The Key to Sweden* is well worth reading. The Swedish people are seen in their homes, in their schools, at work and at play, and one gains some idea of their national politics and foreign policy.

INTO THIS UNIVERSE. *The Story of Human Birth.*

By Alan F. Guttmacher. The Viking Press, New York. \$2.75. $8\frac{1}{2} \times 5\frac{3}{4}$; x + 366 + 14 plates; 1937.

This book, written by an obstetrician, is an authoritative and interesting account of human birth. It includes the lore of childbirth through the ages, the prenatal months, and the modern scientific practices.

The contents were suggested to the author by the ignorance of patients, students and laymen. Since these groups comprise the audience for whom the book was written, one apparent criticism presents itself: undue emphasis upon the abnormalities of pregnancy and labor. The author realizes this and attempts to forestall such criticism by stating in the epilogue: "It (the book) does not omit the detailed story of the normal uncomplicated case, but in the telling it may seem only one case among many." Nevertheless, the original impression lingers and the book, while suitable for the enlightenment of students and laymen might well prove upsetting to an obstetrician's patients.

Drawings and a few photographs illustrate the text and, in addition, there is a list of recommended reading. There is also an index.

THE DOCTRINE OF SURVIVALS. *A Chapter in the History of Scientific Method in the Study of Man.*

By Margaret T. Hodgen. Allenson and Co., London. 5s. net. $7\frac{3}{4} \times 5$; 192; 1936.

This volume, based on the British sources of the doctrine of survivals, concerns itself with cultural persistence, or the process of tradition. Many of the problems dealt with are older than history itself, but the author's method is new, and her style is fresh and stimulating. From the thorough discussion of "Developmentalism", we are brought face to face with the fact that typical histories of scientific development tend to be anecdotal or biographical, and that the *method* by which science advances is discussed only quite infrequently. Furthermore, the few times that it is discussed, it is left not to the scientist or the historian, but to the logician.

The author has made no attempt to give a complete account of the changes in the scientific method in the study of man, but rather has developed the history of the concept as such, together with its use by nineteenth century developmentalists. She has incorporated into the present volume many bibliographic lists which should stimulate workers in the field to further discussion and investigation.

MISCEGENATION. *A Study of the Biological Sources of Inheritance of the South African European Population.*

By George Findlay. J. L. van Schaik, Pretoria, South Africa. 1s. 6d. $8\frac{1}{2} \times 5\frac{1}{2}$; 48, 1936 (paper).

Postulating that purity of white blood is essential to the future of European civilization in South Africa, this pamphlet dis-

cusses the "bridge" by which negroid blood trickles into and progressively dilutes the white. This bridge consists of the half-breeds, from among whom individuals of lighter color and more European-like features are able to "pass" for whites. Since the bridge receives contributions from both races, miscegenation proceeds despite the virtual cessation of primary crossing.

Although the author admits that passers-for-white (called "escapers") include persons who "shine in the professions, in science, in the political world and in society generally" (p. 47), he considers miscegenation a biological evil, which he would like to have remedied. No criticism is made of society's custom of stigmatising persons because of racial origin.

NAVEN. *A Survey of the Problems suggested by a Composite Picture of the Culture of a New Guinea Tribe drawn from Three Points of View. By Gregory Bateson. University Press, Cambridge; The Macmillan Co., New York.* \$5.00. $8\frac{1}{2} \times 5\frac{1}{2}$; xx + 286 + 29 plates; 1937.

This book discusses the customs of the Iatmul tribes of New Guinea. The Naven ceremony in which men caricature women is described in great detail and its relationship to the culture and ethos of the Iatmul as a whole is analyzed. The author is attempting a new technique of studying a culture. He points out that if every aspect is thoroughly understood no single detail appears bizarre or strange. "The emotional background is causally active within a culture, and no functional study can ever be reasonably complete unless it links up the structure and pragmatic working of the culture with its emotional tone or ethos."

There are many excellent photographs, and a glossary of technical and native words which is most valuable because of the author's predilection for polysyllabic and sonorous terms.

SUBSTITUTE PARENTS. *A Study of Foster Families. By Mary B. Sayles. The Commonwealth Fund, New York; Oxford University Press, London.* \$1.75. 9 x 6; vii + 309; 1936.

The first part of this book is devoted to a discussion of some of the factors involved in the motives leading to the adoption of children and of the knowledge that foster parents should have of themselves and of the child to be adopted. The author has devoted the major portion of her work however to a study of several cases in which the placing of the children in foster homes has turned out successfully. The study is concerned primarily with those less fortunate youngsters "who have been passed from hand to hand and thus have never had a chance to be unaware that they are separated from their own parents." Most of the cases presented real problems and the author has endeavored to bring into relief those qualities of the foster parents which seem largely responsible for the success of these undertakings.

CREATIVE EDUCATION AND THE FUTURE. *By Olive A. Wheeler. University of London Press, London.* 8s. 6d. net. $7\frac{5}{8} \times 5\frac{1}{4}$; xi + 365; 1936.

In this book the author has evolved a philosophy of education by applying recent biological and psychological discoveries to educational needs. The problem has been treated from three points of view: (1) General view of the *living present*. (2) The nature of individual development and the chief varieties of individuals for whom educational provision should be made. (3) The living present in relationship to the facts collected.

Separate chapters have been devoted to science, social life, religion and ethics, etc. and show how modern development in each field needs greater emphasis on first-hand knowledge. The book is well written and the author shows a competent knowledge of the subject. In general, the emphasis has been placed upon the psychological aspect. There is an index.

L'ESPÈCE HUMAINE. *Encyclopédie Française, Tome VII.*

Edited by Paul Rivet. Comité de l'Encyclopédie Française Éditeur, 13 rue de Four, Paris. 110 francs. $11\frac{5}{8} \times 9\frac{3}{4}$; 618; 1936.

This being a volume of an encyclopedia, an attempt is made to answer any and all questions which might be looked up under this heading; that is, on the subjects of Anthropology, Ethnography, and Ethnology. Part one discusses the social organization, tools, and means of subsistence in primitive societies and gives more or less detailed accounts of most of the peoples on the earth today. Part two deals with the problem of classification and distinguishing between races, and also presents some account of human inheritance. The third and last part covers distribution and density of population and vital statistics. An extensive classified bibliography and list of journals, and a good index are included. The illustrations which are numerous and excellent are devoted mainly to photographs of typical individuals of all races and groups.

An excellent reference source.

La Destinée Humaine.

By Charles Nicolle. Librairie Félix Alcan, Paris. 1.20 francs. $7\frac{3}{8} \times 4\frac{3}{4}$; vii + 98; 1936 (paper).

This book by the Director of the Institut Pasteur of Tunis is of interest, not only for the questions that it examines, but as a chapter in the natural history of a biologist. In it Professor Nicolle returns to the questions with which he dealt in *la Nature*. Our reason he concludes, is highly efficient in its performance of the task for which it was developed, the adjustment of the organism to its environment, but ill adapted to deal with more remote questions, such as survival after death. However, since this hypothesis is as incapable of disproof as of proof, he counsels his readers to rejoin the church of their upbringing if they will be rendered easier in spirit thereby. A final note informs us that the author followed his own advice soon after writing the book.

A Woman Surgeon, *The Life and Work of Rosalie Slaughter Morton.*

By Rosalie S. Morton, Frederick A. Stokes Co., New York. \$3.00. $8\frac{5}{8} \times 5\frac{3}{4}$; xiv + 399; 1937.

Against the wishes of her family Rosalie Slaughter left a comfortable Virginia home to enter medical school and begin an adventurous life that led her to many lands—among them Germany, France, India, Laborador and Serbia. During that time she was privileged to meet many celebrities, both professional men and writers. Perhaps it is due to her Quaker modesty, but nevertheless we find it disappointing that Mrs. Morton did not tell us more of her medical work, and less about the doings of the great.

One is rather amazed at the versatility and energy of this woman. Besides her work as a surgeon she founded the American Women's Hospitals during the world war, and after the war undertook to educate sixty Serbians in American colleges and universities.

The Social Thought of the Ancient Civilizations.

By Joyce O. Hertzler. McGraw-Hill Book Co., New York. \$4.00. 9×6; xvi + 409; 1936.

Although the systematic discussion of social phenomena begins with the Greeks, the laws, proverbs and ethico-religious writings of the pre-Greek civilizations contain material with which the historian of social thought must reckon. Many of the ethico-religious writings are tantalizingly vague, such as the Zoroastrian formula, "Good thoughts, good words, good deeds," without much specification as to what thoughts, words and deeds are good. And when the writings are more specific, the approved acts and qualities of one civilization are not always the same as those of another. However, beneath these differences there are important agreements. The most widespread of all social behavior formulas is the golden rule, which is found in Egyptian, Babylonian, Hindu, Persian and Chinese literature.

Major Noah. *American-Jewish Pioneer.*

By Isaac Goldberg. Alfred A. Knopf, New

York. \$3.00. $8\frac{1}{2} \times 5\frac{3}{4}$; xvii + 316; 1937.

The subject of this biography, Mordecai Manuel Noah (1785–1851), had a finger in many pies. He was American consul at Tunis, sachem of the Tammany Society, sheriff, surveyor of the port and associate judge of the Court of Sessions of New York City, editor of various newspapers and a successful playwright. Besides these multifarious activities he found time to project a city of refuge for the Jews on Grand Island in the Niagara River, a city the development of which was incidentally to pay a handsome profit to Major Noah himself. The project got as far as the dedication, with much ceremony, of a corner stone for the intended city, but strangely enough Jewry did not show that eagerness to embark on a new exodus into the wilderness which Major Noah had anticipated.

Contributions to the Ethnography of the Kutchin. *Yale University Publications in Anthropology Number 14.*

By Cornelius Osgood. Yale University Press, New Haven; Oxford University Press, London. \$2.50. $9\frac{3}{4} \times 7$; 190 + 10 plates; 1936 (paper).

The Kutchin Indians, (consisting of eight tribes inhabiting the area around the great bend of the Yukon and eastward into the valley of the Mackenzie) are sufficiently distinguished from their neighbors in physique, culture, and language that the author considers that they merit the name, a nation. In this survey there are presented

> various aspects of Kutchin culture with the plan of drawing in a purely descriptive way a picture of aboriginal Kutchin customs at the period just previous to permanent European contact. My purpose is to provide a background which I consider necessary for a logical continuation of cultural studies based on the Northern Athapaskan tribes—.

The study includes numerous illustrations, a tabulation of aboriginal Kutchin cultural traits and a bibliography.

Twenty-Fourth Annual Report of the Secretary of Labor *for the Fiscal Year Ended June 30, 1936.*

Government Printing Office, Washington. 15 cents. $9\frac{1}{4} \times 5\frac{3}{4}$; vii + 150; 1936 (paper).

There are included reports on the employment situation and on the activities of the U. S. Employment Service, the strikes in different sections of the country, labor legislation, social security, immigration, etc. Tables containing a detailed classification of the funds spent by the various departments are given. The services rendered to the public are described, as well as the research work now in progress.

Portugal: *A Book of Folk-Ways.*

By Rodney Gallop. University Press, Cambridge; The Macmillan Co., New York. \$5.50. $8\frac{1}{2} \times 5\frac{1}{2}$; xv + 291 + 17 plates; 1936.

This is a charming account of Portuguese folk lore. The author describes the different parts of the country, the traditional beliefs and customs, the music and the literature. The mixture of pagan superstitions with Christian beliefs and their relation to the calendar is well brought out. The book is beautifully illustrated with photographs and drawings; there is an extensive bibliography and an index.

Truth of a Hopi and Other Clan Stories of Shung-Opovi. *Museum of Northern Arizona Bulletin No. 8.*

By Edmund Nequatewa. Edited by Mary-Russell F. Colton. Northern Arizona Society of Science and Art, Flagstaff, Ariz. \$1.75. $8\frac{1}{4} \times 5\frac{1}{4}$; 114; 1936.

This is a collection of legends of the Hopi clans. The original style seems to have been retained and they contain both history and mythology. There are many valuable and informatory footnotes and a bibliography.

ZOÖLOGY

The Science of Animal Life.

By Arthur W. Lindsey. Harcourt, Brace and Co., New York. \$3.75. $8\frac{1}{2} \times 5\frac{5}{8}$; xi + 656; 1937.

"Of the making of many books there is no

end." This quotation appears to have been in the mind of the author when he penned his preface, for he begins with the observation that a new book on an old subject can be justified only if the author believes that he has something new to offer the reader.

To the present reviewer it appears very definitely that the author has something new to offer. In the first place he has a literary style of such refreshing spontaneity that it is likely to hold the attention of even the casual reader to the end, and if he happens not to be a biologist, he is not unlikely to become one.

In the second place, the field of biology is now so wide that anyone attempting to deal with it in a single volume is likely to find the problem of what to omit greater than that of what to include. In the present instance excellent judgment has been used. The book is divided into sections as follows: The Foundations of Life; Organization and Classification of Animals; The Maintenance of the Individual; The Maintenance of the Species; Problems of Origin; and Biology and Human Life. These subjects are not equivalent in importance, hence the sections dealing with them are not of equal length, but they are well proportioned to each other, and together they give us not only a composite picture of the entire field of animal biology as it exists today, but also a summary of the important contributions to biological thought of the past from Hippocrates to T. H. Morgan.

In the section entitled Problems of Origin the author abandons the better known paths of *sensu stricto* science to explore the less familiar trails of cosmic philosophy, but here he seems somewhat less at home. For instance, in discussing the theory that life came to the earth from inter-stellar space (a theory for which we are indebted chiefly to Arrhenius, although his name does not appear in the text) the author states that the theory is practically valueless, "for if life came to the earth already formed its origin elsewhere remains to be explained." This, of course, is a *lapsus calami*, for if life exists elsewhere than on the earth there is no more reason to feel obliged to account for its origin from a lifeless source than there is to account for the origin of energy or of matter—all three may have coexisted since the morning stars sang together.

There is an ample index of thirty-six finely printed pages.

HANDBOOK FOR SHELL COLLECTORS. *Illustrations and Descriptions of 2,200 Species of Mollusca. Fourth Edition.*

By Walter F. Webb. Walter F. Webb, 202 Westminster Road, Rochester, N. Y. \$2.50. $9\frac{1}{4}$ x 6; 291; 1936.

The compiler of this work is a well known dealer in natural history material who has devoted no small part of his life to arousing enthusiasm for collecting. With the exception of minerals, shells are the most satisfactory material for the beginner, since they deteriorate less rapidly than bird's eggs, flowers, insects, etc., and therefore require less care. For this reason the bulk of the book is devoted to shells, though lower forms of life are not neglected.

In order to assist the beginner to identify his material Mr. Webb has collected numerous illustrations from various sources, many of them works now out of print, and has supplied a few lines of comment to accompany each one. Space at the bottom of each column that would otherwise be vacant is filled with miscellaneous observations that add to the general interest of the work.

In certain instances where the rules of the International Commission necessitate the use of unfamiliar names in place of others that have enjoyed almost universal usage, the compiler has used both names, putting the more familiar one in parentheses between the correct generic and specific term. This is unfortunate, as the commission has ruled that only subgeneric names shall be so treated. The correct way to cite a familiar name in addition to the correct one is to use the complete binomial form and place it in parentheses after the correct specific name. Departure from this practice is likely to augment the confusion, rather than diminish it. The book also shows evidence of inadequate proof reading, many pages affording examples of "printer's spelling."

Notwithstanding these defects the book will be found very helpful both to beginners and to those who are not beginners even. Most of the shells illustrated come from the coasts of North America, but a generous proportion of highly colored and bizarre shaped "mantel-piece" shells from the tropics are included. The index covers twenty-four pages, and includes popular as well as scientific terms.

BIRDS AROUND THE YEAR.
By Lorine L. Butler. D. Appleton-Century Co., New York. $2.00. $7\frac{1}{2} \times 5\frac{1}{4}$; xi + 242 + 8 plates; 1937.

BIRD BEHAVIOUR. *A Contribution Based Chiefly on a Study of the Black-Headed Gull.*
By F. B. Kirkman. T. Nelson and Sons, London. 7s. 6d. $8\frac{3}{4} \times 6$; xv + 232; 1937.

Birds Around the Year is a pleasant little volume designed to interest and entertain the bird-lover and amateur ornithologist by stressing the seasonal aspects of bird distribution and behavior. The discussions are presented in a popularized style and no attempt has been made to develop theoretical or technical matters. The material is, nevertheless, authoritative and gives evidence of having been judiciously selected. The book is essentially a natural history account in that the mating, nesting, feeding and migrating behavior of common birds is discussed relative to their seasonal abundance. To the distress of the reviewer the author frequently deserts prose for verse. This is so often true in books of this type that we, as hard-boiled cynics, are tempted to suggest that some bright young person prepare a doctorate on the stimulus given poetry by ornithology (or *vice-versa*). Indubitably, such a thesis would make interesting reading.

In contrast to the first book *Bird Behavior* by F. B. Kirkman is a mature and thought-provoking study that should find many supporters among serious students of ornithology and comparative psychology. The author has spent years concentrating on the behavior and reactions of the black-headed gull (a British coastal form) and delivers in the present book an excellent summary of his findings. The material is organized as follows: Chapters I-VIII describe the breeding cycle of the gull; Chapters IX-XII compare the difference between the perceptual capacity of this bird with other birds and with men and are based largely on experimental field studies; and Chapters XIII-XVI describe and classify the gull's major emotional reactions. The volume contains an appendix that details many of the experiments performed, has a useful bibliography and index and is embellished by a number of excellent photographs.

HAND BOOK FOR THE CURIOUS.
By Paul G. Howes. G. P. Putnam's Sons, New York. $3.75. $7\frac{1}{2} \times 5\frac{1}{8}$; xix + 364; 1936.

This is not simply another natural history, but a collection that represents the choice of the people themselves. The author, a curator of the Bruce Museum in Connecticut, has covered, as far as possible, those odd and interesting things that the people themselves have found, and wonderingly brought in to him for information. These range from the very lowest sponges and jelly fish through worms, starfish, crustaceans, arachnids, clams, snails and their like to large fish, amphibians, and reptiles. There is even a chapter on first aid in snake bites.

Simply and clearly the author describes hundreds of curious specimens found by the wandering layman in the eastern United States. He gives both their common and technical names; tells us to what group or phylum they belong; what they do; and whether they are harmful or beneficial and are to be protected. The awe inspiring praying mantises; baby fishes "with their bellies bursting out;" the "stunted lobster lost in fresh water pond" and many other strange creatures selected year after year by the curious, are adequately taken account of in this compact little book.

There are many excellent illustrations, a short, selected bibliography and a long and detailed index.

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society. Volume XXI, Part 4, Numbers 19–23.*

New York Zoological Society, New York. 90 cents. $10\frac{1}{2} \times 7$; 72 + 3 plates + 1 folding chart; 1936 (paper).

The articles in this number of *Zoologica* include: The Southwestern Desert Tortoise, *Gopherus agassizii*. By Chapman Grant. Brief observations made on the habits, young and range of this tortoise which tourists are buying as a pet, and a table of external measurements made on 30 adult males and 30 adult females.

Plankton of the Bermuda Oceanographic Expeditions. VII. Siphonophora Taken During the Year 1931. By Captain A. K. Totton. A classification of the siphonophores drawn at definite levels and in a definite area off the south coast of Bermuda.

The Female Bitterling as a Biologic Test Animal for Male Hormone. By Israel S. Kleiner, Abner I. Weisman, Daniel Mishkind and Christopher W. Coates. A summary of various experiments leading the authors to conclude that it is the male hormone in human urine which when added to the water of an aquarium containing female bitterlings, causes the lengthening of the ovipositor of these fishes.

Some Tropical Fishes as Hosts for the Metacercaria of *Clinostomum complanatum* (Rud. 1814) (=*C. marginatum* Rud. 1819). By Ross F. Nigrelli.

Caudal Skeleton of Bermuda Shallow Water Fishes. I. Order Isospondyli: Elopidae, Megalopidae, Albulidae, Clupeidae Dussumieriidae, Engraulidae. By Gloria Hollister. A study of the similarities and differences found in the caudal skeletons of the Bermuda Isospondyli.

CULTURE METHODS FOR INVERTEBRATE ANIMALS. *A Compendium prepared coöperatively by American zoologists under the direction of a committee from Section F of the American Association for the Advancement of Science.*

By Paul S. Galtsoff, Frank E. Lutz, Paul S. Welch, James G. Needham et al. Comstock Publishing Co., Ithaca. $4.00. 9×6; xxxii + 590; 1937.

A great deal of experimental biology is dependent upon the successful husbandry of diverse animal forms. This is well attested by the *Drosophila* situation where results, both genetic and otherwise, have accrued largely because this particular species has lent itself to easy culture. There are, however, many other organisms equally suitable for research and equally capable of being kept in the laboratory. Unfortunately, this information has been largely in the hands of a few specialists and not easily available to the investigator. The present compendium, prepared coöperatively by American zoölogists, will do much to remedy this situation for it gives a wealth of useful information on the culture of many invertebrates. The book will prove valuable in a number of ways and is deserving of warm welcome by biologists. The text material is organized on taxonomic lines and the various sections are designed to give practical data on the methods of rearing, handling and observing the form in question. Most of these discussions are of first-rate calibre. The book is well indexed and contains a number of pertinent references.

BAKTERIENFREIE MEERSCHWEINCHEN. *Aufzucht, Lebensfähigkeit und Wachstum, nebst Untersuchungen über das Lymphatische Gewebe.*

By Gösta Glimstedt. Levin and Munksgaard, Copenhagen. Kr. 10. $9\frac{3}{4} \times 6\frac{1}{2}$; 295; 1936.

Numerous attempts to rear bacteria-free animals have been made since 1885. The present study is a report of the largest-scale and most significant successful attempt to date, both with reference to number of animals reared and the duration of time lived under bacteria-free conditions. Previous to this study the record was held, for vertebrates, by Küster who in 1911 succeeded in keeping a goat bacteria-free for 12 days from birth, and by Cohendy and Wollman who in 1914 reported the survival of a bacteria-free guinea pig to an age of 18 days.

Glimstedt's experiments were conducted from 1928 to 1936 in the Institute for Chemical Medicine of the University of Lund. Of 57 animals from 22 litters, bacteria-free at birth, eight survived the twenty-eighth day. Of these, three were killed for examination on the twenty-eighth day, two on the thirty-second, and

three on the sixtieth. Comparisons with control animals reared on several different kinds of rations showed that the bacteria-free guinea pigs were inferior to the control groups in rate of growth and in development, in particular that of the lymphatic tissue. The author concludes that the non-pathogenic bacteria normally found in these animals exert a favorable influence upon growth and development.

This is an important book. It is provided with a thirteen-page bibliography but lacks an index.

SNAKES AND THEIR WAYS.

By C. H. Curran and Carl Kauffeld. Harper and Brothers, New York. $3.50. $9\frac{1}{4}$ x $6\frac{1}{4}$; xvii + 285 + 32 plates; 1937.

The authors have turned out a fascinating and authoritative book on the natural history of snakes. The first ten chapters deal with the more general biology of snakes, and with snake poisons and the problem of treating snake bites. There are chapters on rattlesnakes, the true vipers, snake-eating snakes, tree snakes, blind snakes, Asiatic cobras, African cobras, boas, anacondas, and fresh water snakes. The deadliest snake in this country is the diamond-back rattlesnake of Texas. While there has always been a considerable element of exaggeration in the accounts of the virulence of snake poisons, the authors readily admit that the black mamba is probably even worse than its reputation and exhibit a healthy respect for the courage of the snake charmers of Asia. The photograph on the dust jacket illustrates the best of the snake charmer tales. Fantastic stories by the legion have been invented about snakes, and some of them are given here. The importance of snakes in history and religion has a short chapter.

There are 22 pages of excellent photographs; a check list of snakes of the United States and Canada; and a general and scientific index.

AN INTRODUCTION TO NEMATOLOGY. *Section 1, Part 1.*

By B. G. Chitwood and M. B. Chitwood. Monumental Printing Co., 3110 Elm Avenue, Baltimore. $2.50. 20 per cent discount on 5 or more copies; 20 per cent discount on orders for all parts. 12 x 9; 53; 1937 (paper).

Many books have appeared that deal with the economic, medical, veterinary, and taxonomic aspects of nematology. To supply the need for a comprehensive treatise on the purely zoological aspect of the subject, the authors have attempted the present work. This brochure is the first of the series of ten numbers which will be published from time to time until the book is completed.

The writers have handled the subject from a comparative standpoint, and for this reason have divided the present book into three sections: anatomic, ontogenetic, and systematic. The morphology of the different nematodes is given in minute detail, and in many cases the description is aided by the use of drawings and diagrams. Each chapter of the book closes with an extensive bibliography on the particular phase of the subject presented in that chapter. The present volume should be useful to the beginner, as well as to the taxonomist, and, to a lesser extent, to those dealing with the experimental phases of nematology.

MARINE FISHES OF SOUTHERN CALIFORNIA.

By Percy S. Barnhart. University of California Press, Berkeley. $4.00. 10 x $7\frac{3}{4}$; iv + 209; 1937.

This work, by a well known British ichthyologist, now on the staff of the Scripps Institution of Oceanography, consists of a collection of descriptions and illustrations of all species of fish that have ever been reported taken between Point Concepcion and the TiJuana River. There may be a few which the author has overlooked, as he states in his preface, but certainly these are not very many.

It will come as a surprise to many of us to know that one species of lancelet and three of cyclostomes have been taken within these limits, and to learn that there is a sea horse on the west American coast that grows to be twelve inches in length. The large number of eel-like fishes, none of which is a true eel, is also surprising,

as is also the group of needle fishes, which resemble miniature sword fishes except that the needle is in the lower jaw. An innovation is the raising of the elasmobranchs to the rank of a class. The illustrations are well drawn, and there is a glossary, a bibliography, and two indexes.

Das Leben Deutscher Greifvögel. *Die Umwelt der Raubvögel unter besonderer Berücksichtigung des Habichts Bussards und Wanderfalken.*

By Heinz Brüll. Gustav Fischer, Jena. RM 6. $9\frac{1}{2} \times 6\frac{1}{2}$; vi + 144 + 1 folding chart; 1937.

The predatory birds native to Germany are grouped variously according to type of area inhabited, senses involved in locating prey, type of beak, mode of attack, prey, enemies, attitude to human beings and other characteristics. The short-flight birds such as the night-hawk, that depend more upon the auditory than visual sense, inhabit the wooded regions. The soaring species, exemplified by the buzzard, whose auditory and visual equipments are both highly developed, are found most often in mixed landscapes, interspersed with wooded lots and fields. The tree falcon and migrating falcon, both long-distance fliers with sight more acute than hearing, are typical of the predatory birds frequenting the open areas. Data are presented on the relative surface area of the wings for the birds discussed, and a bibliography and a glossary of hunting terms are added.

A Book of Famous Dogs.

By Albert P. Terhune. Drawings by Robert L. Dickey. Doubleday, Doran and Co., Garden City, N. Y. \$2.75. $8\frac{1}{4} \times 5\frac{1}{2}$; 300; 1937.

Here are well retold dozens of your favorite dog stories where you may renew the acquaintance of such old friends as Barry, the greatest of Saint Bernards; Satan, the world hero; and Flush, Elizabeth Barrett Browning's spaniel. You will probably be resentful, though, at the rough handling that Greyfriars Bobby receives in a "debunking" chapter on "professional mourners," for where is sentimentality excusable if not in a tale like this?

The most delightful new friend we made in this book was a black poodle by the name of Schwartz, who every morning crossed the street to a store opposite his master's house in Heidelberg to bring home a pouch of tobacco. After three years of this, Schwartz and his family moved to another city some hundred and fifty miles away and were delighted to find a tobacco store just opposite their new home as before. But Schwartz did not know this. You can guess what happened. His master demanded tobacco. Ten days later an exhausted poodle returned with the bedraggled remains of a pouch from the old store in Heidelberg.

Birds of an Indian Garden. *Second Edition Revised and Enlarged.*

By T. Bainbrigge Fletcher and C. M. Inglis. Thacker, Spink and Co., Calcutta. Rs. 12. $9\frac{7}{8} \times 7\frac{1}{4}$; xii + 201 + 35 plates; 1936.

As there has been no popular book on Indian birds this volume, which first appeared as a series of papers in the *Agricultural Journal of India* during the years 1919 to 1924, is an attempt to familiarize the Indian and European laymen with them. Fletcher gives lengthy descriptions of 33 common Indian birds—the jungle, the town and the garden varieties—and mentions many details concerning their roosting, breeding, nesting, and feeding habits. The popular, as well as scientific, names are used in describing the different species and substantial data collected on them include their parasitic tendencies, their time of migration and expositions of their calls and songs. Many of the descriptions are dry and lack vividness, but here and there the author comes forth with bits of bird life of human interest.

Thirty-three large and brightly colored plates by the co-author together with a collection of 19 black and white drawings sprinkled through the text illustrate the volume. Two plates are added at the end showing the sizes and markings of the eggs of 30 of the birds described. There is neither bibliography nor index.

THE INSECT MAN. *A tale of how the Yew Tree children went to France to hear the story of Jean Henri Fabre in the places where he lived and to see the homes of some of the insects whose life-story he has written.*

By Eleanor Doorly. Introduction by Walter de la Mare. D. Appleton-Century Co., New York. \$1.50. $7\frac{7}{8}$ x $5\frac{1}{8}$; xvii + 180; 1937.

The author has written an ingenious little book introducing to English children the life of the French entomologist, Henri Fabre, and his writings about the habits of insects. The theme of the book is a quaint story of how three English children and an older sister "went to France to hear the story of Henri Fabre in the places where he lived, and to see the homes of some of the insects whose life-story he has written."

As Mr. de la Mare says in his introduction, any child of imagination who reads this book will have learned much about Fabre and his work, and "will have won to something at least of his inmost self and spirit." Mr. Gibbings' wood-cuts, whether of insects, buildings, places or things, are an interesting addition to a very pleasant volume.

JUST CATS.

Pictured by Lowes D. Luard and described by T. O. Beachcroft. Country Life, Ltd., London; Charles Scribner's Sons, New York. \$3.75. $11\frac{1}{2}$ x $8\frac{1}{2}$; 139; 1936.

A volume of delightful illustrations, reproductions from etchings, pencil studies and pen drawings, depicting all kinds of cats from the torpid, contented cushion and fire-side variety to the taut and lanky alley cat outcast. Although Mr. Beachcroft's text, written around and about the drawings seems at times a bit strung out, perhaps in an effort to pad in between the numerous pictures, in places his characterizations and descriptions are really intriguing.

Alley cats, Siamese cats and tabbies, their origins and the legends that have sprung up around them, as well as their habits, gestures and movements, are simply and realistically described. The author's account of the old episode of our feline friend in pursuit of a sparrow is here given real life and feeling. [It is, though, hell for the sparrow. *Ed.*]

DIE TSETSEFLIEGEN: *Ihre Erkennungsmerkmale, Lebensweise und Bekämpfung. Ein Leitfaden für die Praxis.*

By F. Zumpt. Gustav Fischer, Jena. RM. 9 (paper); RM. 10.50 (cloth). $10\frac{1}{2}$ x $6\frac{3}{4}$; iv + 149; 1936.

This handbook for sanitary officers and research workers in the tsetse infested areas of Africa is the first treatise of its kind in German since the publication in 1905 of Sander's *Die Tseten.* Much more has been learned about these flies since then. For each species the distinctive characteristics and available information on habits, breeding, distribution, density (flies per boy hour), and pathogenicity are discussed. A section of the book is devoted to methods of eradication. The work is abundantly illustrated with schematic drawings, photographs, maps and an excellent colored plate. The bibliography is adequate and there is an index.

REVISTA DI PARASSITOLOGIA. *Publicazione Trimestrale. Volume 1, Number 1, January 1937.*

Edited by A. Missiroli. Redazione e Amministrazione, Via G. B. Martini, 6, Roma. L. 60, Annual Subscription; L. 15, Separate Numbers (in Italy and Colonies); L. 90, Annual Subscription; L. 25, Separate Numbers (outside of Italy). 10 x 7; 87; 1937 (paper).

REVISTA DI PARASSITOLOGIA. *Volume 1, Number 1. Supplemento Monografico N. 1.* Fauna anofelinica delle Colonie italiane, by Lidia La Face.

Edited by A. Missiroli. Redazione e Amministrazione, Via. G. B. Martini, 6, Roma.

This new quarterly, edited by an outstanding malarialogist, is dedicated to the publication of articles on protozoology, helminthology and entomology. The first number contains articles on experimental studies of the biological cycles of *Lepocreadium album* and of *Mesometra orbic-*

ularis (Rud.); on a new species and a new variety of Cercariae discovered in Sardinia, and others. There is a supplement of over 100 pages which presents a comprehensive review of the reports on the kinds of *Anopheles* which inhabit the Italian African colonies. Each of the articles carries English and German summaries.

THE INTERNAL PARASITES AND PARASITIC DISEASES OF SHEEP. *Their Treatment and Control.*

By I. C. Ross and H. McL. Gordon. Angus and Robertson, Sydney. 25s. $8\frac{1}{2}$ x $5\frac{1}{2}$; xx + 238 + 46 plates; 1936.

This monograph deals with problems of the life-history, pathogenicity, treatment and control of the helminth parasites of sheep. The book is admittedly practical in character and is designed primarily to be useful to veterinarians, stock inspectors and sheep owners of Australia. There is little doubt but that it will fulfil its goal in this regard for the material is well-chosen, helpfully organized and clearly presented. The discussions are arranged taxonomically with all the important species considered. For each section data are given about the distribution, life-history, pathogenic importance, symptoms and diagnosis, treatment and prophylaxis for the form in question. The book is illustrated with photographs and drawings and contains a first-rate bibliography, index and glossary.

STRANGE INSECTS AND THEIR STORIES. *How they live. Various kinds and species. Curious traits. Their intelligence. Ways of communicating with each other. Battle for survival. Benefit to all mankind, etc.*

By A. Hyatt Verrill. L. C. Page and Co., Boston. \$2.50. $8\frac{3}{4}$ x 6; xv + 205; + 5 plates; 1937.

Again Mr. Verrill has succeeded in capturing the enchantment of the nature world. In this eminently readable book the author discusses only those insects that the average person might observe and leaves alone the exotic ones from remote corners of the earth. He tells of insects that steal rides, roll a ball, bury the dead, and even of some that fire gas bombs. In a closing chapter he gives a few practical hints to young entomologists on mounting, preserving and breeding insects.

The book is amply illustrated with pen and ink drawings by the author. There is an index.

MORE SONGS OF WILD BIRDS.

By Albert R. Brand. Foreword by Arthur A. Allen. Thomas Nelson and Sons, New York. \$2.50. $8\frac{3}{4}$ x $7\frac{1}{2}$; 116; 1936.

This is the second of a series of books by the same author describing bird songs and calls recorded on three double faced phonograph records included in a folder in the back of the book. The notes of the birds were accurately recorded in the field on sound films. These show that much of the song is pitched far higher than the human ear can perceive. Phonograph records and needles are incapable of faithfully reproducing even the audible notes pitched above the range of the piano and violin so the records are rather disappointing. However, the drawings and descriptions of the birds, and a chapter on the significance of the songs make the book worth having.

AN ACCOUNT OF THE FISHING GEAR OF ENGLAND AND WALES. *Third Edition (Revised, 1936). Fishery Investigations, Series II. Vol. XV, No. 2.*

By F. M. Davis. His Majesty's Stationery Office, London. \$1.75. $10\frac{5}{8}$ x $7\frac{1}{4}$; 139; 1937 (paper).

Detailed information is presented in this publication on the making of various types of nets and on the use of nets and other fishing gear as employed in England and Wales. The text is well supplemented with numerous diagrammatic sketches. Since the appearance of the first edition of this work in 1923 the Vigneron-Dahl gear has become the most important trawling apparatus and the sections on this gear have been entirely rewritten in the present volume. Another addition is a chapter dealing with the modificaions of fishing gear, designed to mitigate the ill-effects

of the destruction of small unmarketable fish.

There is an index and a selected bibliography of 129 references.

GUARDIANS OF THE WILD.

By M. B. Williams. Thomas Nelson and Sons, London and New York. 2s. 6d. 7 x 5; x + 148; 1936.

Miss Williams tells the story of the Canadian National Parks, how they came to be started, and what is being done at the present time in the interests of conservation. Large areas of the National Parks, heretofore unexplored, have been made accessible by highways, and tourists have been attracted in such numbers that the Parks have become an important source of revenue to the Canadian government. Wild animals which in the past faced extinction are now increasing in numbers. The book has been designed for popular consumption, but it makes very interesting reading. There are a number of photographic plates.

PHYSIOLOGIE DER SÜSSWASSERFISCHE MITTELEUROPAS. *Handbuch der Binnenfischerei Mitteleuropas, Band II B.*

By Wilhelm Wunder. E. Schweizerbart'sche, Stuttgart. In Germany: 49. marks (paper); 52. marks (cloth). Outside of Germany: 36.75 marks (paper); 39. marks (cloth). $10\frac{1}{2}$ x $7\frac{1}{2}$; xi + 340; 1936.

This comprehensive treatise on the physiology of the fresh-water fishes of Central Europe gives information on the skin, sense organs, nervous system, breathing apparatus, blood, nutrition, excretion, internal secretions, temperature, locomotion, reproduction and embryonic development. In fact, it contains "everything theoretical or practical" which might be of interest to the professional (fisher, research worker, or fishery official), the amateur or the sportsman. It is illustrated, indexed, and each section has a bibliography.

BRITISH GRASSHOPPERS AND THEIR ALLIES. *A Stimulus to their Study.*

By Malcolm Burr. Philip Allan and Co., London. 6s. net. $6\frac{1}{2}$ x $4\frac{1}{4}$; xvi + 162 + 6 plates; 1936.

In the British Isles there are less than forty species of Orthoptera, using that term in its broader sense. It is therefore possible to cover them all in a book small enough to be carried in the pocket, and have enough space left over for the earwigs. This is a well written work, but it is not in any sense a beginner's book. Its chief merit is its completeness and its compactness; its chief defect is the sparsity of its illustrations, but those which it has are excellent. Its most distinctive feature is a series of maps of the British Isles, showing the geographic distribution by counties of the more important species. There is no index, but such a brief work does not need one.

HOUSEHOLD PESTS: *Their Habits, Prevention and Control.*

By Peter B. Collins. Sir Isaac Pitman and Sons, London; Pitman Publishing Corp., New York. \$1.00. $7\frac{1}{4}$ x $4\frac{3}{4}$; xiii + 98; 1936.

This work is intended chiefly for the housekeeper, and contains only just enough scientific information to help her to cope intelligently with vermin. In many instances the most effective way of dealing with such unwelcome guests involves the use of poisons which the inexperienced person should not handle, and the merit of the book lies in the fact that it gives so much information on the art of using poison and the necessary precautions to be taken—information that it is sometimes difficult to obtain. The index covers four pages.

HOST-PARASITE RELATIONS IN THE DISTRIBUTION OF PROTOZOA IN TERMITES. *University of California Publications in Zoology, Volume 41, Number 15.*

By Harold Kirby, Jr. University of California Press, Berkeley. 35 cents (with No. 16) $10\frac{1}{4}$ x $6\frac{3}{4}$; 25; 1937 (paper).

THE DEVESCOVINID FLAGELLATE PARAJOENIA GRASSII FROM A HAWAIIAN TERMITE. *University of California Publications in Zoology, Volume 41, Number 16.*

By Harold Kirby, Jr., University of California Press, Berkeley. 35 cents (with No. 15) $10\frac{1}{4} \times 6\frac{3}{4}$; 10; 1937 (paper).

A REVIEW OF THE GENERA CLAUSIDIUM KOSSMANN AND HEMICYCLOPS BOECK (COPEPODA, CYCLOPOIDA), WITH THE DESCRIPTION OF A NEW SPECIES FROM THE NORTHEAST PACIFIC. *University of California Publications in Zoology, Volume 41, No. 14.*

By S. F. Light and Olga Hartman. University of California Press, Berkeley. 25 cents. $10\frac{1}{4} \times 6\frac{3}{4}$; 18; 1937 (paper).

BOTANY

TREES. *A Pictorial Volume for Lovers of Nature.*

By Thomas O. Sheckell. Frederick A. Stokes Co., New York. $4.00. $9\frac{3}{4} \times 7\frac{3}{4}$, 176; 1936.

Eighty-two photographs of United States trees from the Atlantic's rocky Maine coast to the Pacific shores of southern California. This is not only a naturalist's collection, it is an artist's as well. While the outlines of the individual trees are clear and beautiful in themselves, the enchanting part of these exceptional photographs lies in their background of typical and illustrative scenery. Here we see the rugged spruce perched high on the Wasatch mountains of Utah, the date palm casting its shade over a trim, tropical Pasadena garden, a gnarled cypress clinging desperately to the rock-bound shore of Monterey, the Pacific roaring at its foot, the old elms looking over the peaceful hayfields and rolling hills of New York state, Lombardy poplars, sentinels to the valley of the Great Salt Lake, with the Oquirrh mountains misty in the distance, and the sturdy pine looking out to sea from the rock-ribbed coast of Maine. The Joshua tree of Nevada is shown standing alone in the desert heat; the dogwood and fine New Jersey apple tree in all their spring blossoms; and winter trees casting their naked shadows on snowy landscapes.

The photographs have not only the beauty of paintings but almost the realness of the outdoors itself. The expressive simplicity of Mr. Scheckell's short captions add a poetic touch to an already beautiful piece of work.

THE STORY BOOK OF FOODS FROM THE FIELD. *Wheat, Corn, Rice and Sugar.*

By Maud and Miska Petersham. John C. Winston Co., Philadelphia. $2.50. $7\frac{3}{4} \times 8$; 128; 1936.

Once upon a time children made friends with fairies and goblins and gnomes and loved Alice dearly. But the self-respecting modern child should, if properly brought up, hold up his hands in horror at such scientific heresies. Rather he should be deeply interested in a learned, though simply expressed, discussion of the virtues of cleaning one's teeth and where the breakfast food comes from. So conditioning a youth is well calculated to produce a properly regimented citizen. We, however, will be so bold as to express the opinion that there is something to be said for the old horse and buggy type of fairy story since we prefer individuals with some little imagination and originality even if they are not quite so thoroughly self-washed behind the ears.

The present book is instructive and carefully accurate. The illustrations are colorful and enjoyable. A great many psychology-conscious parents and maybe a few children will be delighted with it.

THE PRODUCTION OF FIELD CROPS. *A Textbook of Agronomy. Second Edition.*

By T. B. Hutcheson, T. K. Wolfe and M. S. Kipps. McGraw-Hill Book Co., New York. $3.50. 9×6; xvii + 445; 1936.

The first edition of this text has been in use since 1923 and the present volume reaps the benefits derived from constructive criticisms and suggestions made by those who have used the first for teaching purposes. The original plan of the book was patterned after an outline for a standard introductory course in field crops, adopted by the American Society of Agronomy. The plan of the revised edition is essentially the same although the size of the volume has been somewhat reduced to fit more nearly the requirements of a one

semester course. The first section of the book is devoted to a discussion of the fundamentals underlying all crop production and the following sections to the production of special crops: cereal or grain crops, legumes for seed, forage crops, root crops, tubers, sugar plants and stimulants. Each chapter of each section is followed by a list of references and topics for discussion. There is an index.

FUNDAMENTALS OF BACTERIOLOGY.

By Martin Frobisher, Jr. W. B. Saunders Co., Philadelphia. \$3.25. $7\frac{3}{4} \times 5\frac{1}{4}$; 474 + 1 folding chart + 4 plates; 1937.

This volume has been prepared especially for students seeking a clear and practical presentation of the facts concerning both pathogenic and non-pathogenic forms of bacteria and their effect on everyday human life and activity.

The text is divided into three parts: I—The Fundamental Principles of Bacteriology; II—The Class Schizomycetes; and III—The Relation of Bacteria to Disease. The use of the microscope, methods of cultivation and staining, and the general morphology of bacteria are presented with an unusual regard for detail. The various classifications of bacteria, the essential facts about them and their practical effect upon food, health, and many classes of industry and agriculture are clearly and precisely described. The many photographs, diagrams, and drawings add considerably to the presentation of a very inclusive assemblage of material. The book boasts an index and a detailed table of contents, which enhance its value as a reference and as a text.

METHODS IN PLANT PHYSIOLOGY. *A Laboratory Manual and Research Handbook. With a Chapter on Statistical Methods, by George W. Snedecor. First Edition.*

By Walter E. Loomis and Charles A. Shull. McGraw-Hill Book Co., New York. \$4.50. 9 x 6; xviii + 472; 1937.

The material in this volume has been compiled for use by the instructor, the student, and the research worker in plant physiology. It is divided into two parts, the first consisting of laboratory exercises, and the second of detailed outlines of methods and procedures. The laboratory exercises, though grouped by subject, have been divided into three grades: elementary, intermediate and advanced. In every case the grade is indicated and, in addition, at the end of each exercise there are review questions.

The book has been written for those having only moderate training in physics and chemistry and represents a comprehensive survey of the field of plant physiology. There is a chapter on statistical methods by George W. Snedecor and an appendix containing numerous useful tables of elements, solutions, measurements, etc. There is, besides, an index.

THE ROMANCE OF TEA. *An Outline History of Tea and Tea-Drinking Through Sixteen Hundred Years.*

By William H. Ukers. Alfred A. Knopf, New York and London. \$2.75. $8\frac{5}{8} \times 5\frac{3}{4}$; xiv + 276 + xii; 1936.

In this delightful book the editor of the *Tea and Coffee Trade Journal* narrates the history of tea in China and Japan from the first certain reference to it in the fourth century A.D., and the spread of tea growing to other countries of the Orient and of tea drinking to the Occident. The story of the East India Company and of the clipper ships which later brought the cargoes of tea from China to the West are full of interest. The book also describes the growing and manufacture of tea, gives a statistical résumé of the commerce of tea, and deals with tea manners and customs of Orient and Occident, with the ceramics of tea pots and with allusions to tea in literature. A final chapter gives useful information on the choice and brewing of tea.

A MONOGRAPH ON THE GENUS HEUCHERA. *Minnesota Studies in Plant Science Volume II.*

By C. O. Rosendahl, F. K. Butters and O. Lakela. University of Minnesota Press, Minneapolis. \$3.00. $9\frac{3}{8} \times 6$; 180; 1936 (paper).

This exhaustive study of the genus *Heuchera* (of the Saxifrage family) is based on material from the principal herbaria in this country and from one abroad, from botanical centers, and from field and garden. The authors show that the changes in the several characters "can be traced step by step from the more primitive types up to the most highly specialized, and in several instances parallel lines of development occur. It is to be noted, however, that if the majority of species of *Heuchera* were to become extinct and only a few of the more extreme forms were to remain, these would probably be regarded as distinct genera." The volume contains a number of illustrations, is well documented and has an index of the botanical names in the genus.

MORPHOLOGY

Recent Advances in Cytology. *Second Edition.*

By C. D. Darlington. Foreword by J. B. S. Haldane. P. Blakiston's Son and Co., Philadelphia. $6.00. 8 x $5\frac{1}{8}$; xvi + 671 + 16 plates; 1937.

The first edition of this book, published in 1932, (*cf.* Q.R.B. Vol. 3, No. 1, p. 107) aroused much controversy and brought forth many objections. J. B. S. Haldane, in the forward to the 2nd edition, says, "However, most of these objections have been quietly withdrawn in the four succeeding years. The most important correction to the views expressed in the first edition has been made by Dr. Darlington himself." The first edition attempted to describe only one aspect of cytology, the study of the nucleus and the chromosomes in plants and animals, and devoted the last chapter to "the evolutionary point of view." Increasing knowledge has made it possible for him, in this edition, to "recast the whole account in terms of evolution" (i.e. showing meiosis as developing from mitosis, etc.) and he has devoted the last chapter to cell mechanics.

The book includes four appendices: interpretation, technique, glossary, and bibliography. The text is copiously illustrated with photographs, diagrams, and tables, and is well indexed.

Mammalian Anatomy *with Special Reference to the Cat. Sixth Edition.*

By Alvin Davison. Revised by Frank A. Stromsten. P. Blakiston's Son and Co., Philadelphia. $3.00. $8\frac{3}{8}$ x $5\frac{5}{8}$; xiv + 328; 1937.

The sixth and latest edition of this well-known book has been revised by making a number of minor changes in the text; by enlarging several sections; by adding new and redrawn figures, and by emphasizing comparative material more than was done in previous editions. In the preparation of this volume the authors have kept in mind the fact that an increasing number of students in psychology, education and physical education, as well as those in pre-professional schools, are taking comparative anatomy and have organized their material accordingly. The book possesses a useful glossary and an adequate index. The illustrations are well drafted but the lettering is frequently amateurish. It seems safe to predict that this volume in its latest edition will continue to find friends among students of elementary comparative anatomy.

A Manual of Normal Histology and Organography. *Seventh Edition, Thoroughly Revised.*

By Charles Hill. W. B. Saunders Co., Philadelphia. $3.50. $7\frac{1}{2}$ x 5; 530 + 6 plates; 1937.

This book is designed for a very elementary course indeed and as such is excellent for organography, being largely a simplified anatomy, but is decidedly lacking in histology. The author feels that "neglect of proper care of the teeth is a common failing" and so devotes a more detailed discussion to them than to any other part of the body. It is intended that the teacher should make up his own laboratory procedure and let it precede the text assignments which he is then to supplement.

It is difficult to see just where such a manual as this finds the considerable array of customers that it must have to reach a seventh edition. It is far too elementary for a medical school course on the one hand, and few undergraduate departments would teach this much human anatomy

on the other. High school teachers of hygiene who wish to obtain a good working idea of the human body should find it admirable.

Anatomy of the Fetal Pig.
By John G. Sinclair. Collegiate Press, Ames, Iowa. \$2.00. 9 x $6\frac{1}{2}$; xiv + 80; 1936.

The foetal pig has become in recent years a useful laboratory type in the study of anatomy. This manual aims at a fairly complete study of those structures which should be familiar to a student entering the medical school and elides those which differ so greatly from the human structure that they would not be applicable. Fifty-one illustrations by the author are included in the text which is clearly and concisely written. Teachers of college anatomy will find this an excellent laboratory guide.

Vom Bau und Leben des Gehirns.
By Ernst Scharrer. Julius Springer, Berlin. RM 4.80. $7\frac{1}{8}$ x $4\frac{5}{8}$; vii + 169; 1936.

Volume 31 of this popular science series concerns the structure and functioning of the brain. The author begins with a brief general description of the nervous system, and then proceeds to discuss in detail the nervous systems of invertebrates, of vertebrates, and finally of man. Subsequent chapters are devoted to the central nervous system, brain and speech, reflexes, and the nervous system as a whole. An excellent little book for the layman.

PHYSIOLOGY AND PATHOLOGY

Electrical Signs of Nervous Activity.
By Joseph Erlanger and Herbert S. Gasser. University of Pennsylvania Press, Philadelphia; Oxford University Press, London. \$3.50. 9 x $5\frac{7}{8}$; x + 221; 1937.

This is the third in a series of lectures sponsored by The Johnson Foundation for Research in Medical Physics. It covers the same general ground as the second lecture series given by E. D. Adrian in 1931 on *The Mechanism of Nervous Action*, but enormous advances have been made in this field during the intervening five years. The great difficulty has always been that there was no adequate means of amplifying and graphically recording the extremely weak potentials of the nerve. The old type amplifiers badly distorted the wave formation particularly in low frequencies and would not pick up the weaker potentials at all, while the oscillographs used distorted the form of briefer currents, were kept in adjustment only with much difficulty, and were disturbed by the slightest vibrations in the laboratory. In the last few years, however, the cathode ray oscillograph has been fully developed for physiological use and does away with almost all the former difficulties of recording. The only moving part in this instrument is an electronic beam which has so little inertia that it can faithfully follow electrical oscillations at a frequency as high as 200 million per second. At the same time, due to the recent wonderful improvements made in vacuum tubes, amplification now gives comparatively little difficulty.

As the two authors differ in the interpretation of many points, they decided to divide the lectures each writing those facts of most interest to him. Dr. Erlanger discusses: (1) the analysis of compound action potential of nerve; (2) the comparative physiological characteristics of nerve fibres; and (3) some reactions of nerve fibres to electrical stimulation; while Dr. Gasser discusses (4) sequence of potential changes; and (5) the excitability cycle. Both authors handle their complex material extremely well and we highly recommend the book to workers in other fields who wish to become familiar with this subject without too much work.

Cancer and Diet. *With Facts and Observations on Related Subjects.*
By Frederick L. Hoffman. Williams & Wilkins Co., Baltimore. \$5.00. 9 x 6; xx + 767; 1937.

This book is a comprehensive study of the relation of diet to the cause and control of cancer. It is based largely on questionnaires collected from 2,234 cancerous and

1,149 non-cancerous people that tabulate many diverse data about the food, eating habits and general physique of these 3,383 individuals. The text material is organized around five divisions: (1) dietary theories of cancer; (2) the modern diet; (3) cancer metabolism; (4) dietary facts concerning cancer patients, and (5) general summary and conclusions.

There can be no doubt but that the author has painstakingly and systematically summarized a wealth of material in this volume. To what extent his findings may be supported, either at the moment or at some future date, falls outside the scope of this review. The following selected quotations best summarize the author's opinion relative to the general cancer problem:

> . . . . I have come to the conclusion that the prevailing view that cancer, in its origin, is always a local disease and that treatment should be limited to the local lesion is seriously at fault. It is held, therefore, that without a seriously disturbed derangement of the metabolism the local irritation would be wholly ineffective in producing malignant proliferation. . . . I am absolutely convinced that the underlying cause of cancer is to be found in an excessive intake of foods of a high organic or mineral content, or generally of an alkaline base instead of acid.

The volume has extended bibliographies following the various sections and boasts a good author and subject index. An appendix discusses in detail the questionnaire and is replete with protocols of various sorts.

THE DEVELOPMENT OF MODERN MEDICINE. *An Interpretation of the Social and Scientific Factors Involved.*

By Richard H. Shryock. University of Pennsylvania Press, Philadelphia; Oxford University Press, London. $4.00. 9 x 6; xv + 442 + 8 plates; 1936.

Progress in the art and science of medicine is here viewed in relation to the development of modern civilization. Against the background of scientific and social history, the author presents the major aspects of the evolution of modern medicine. He begins his survey with the 16th and 17th centuries which were to see the first applications of empirical or experimental methods. These were also applied to medicine but in a very limited fashion because theoretical systems continued to prevail. It was not until the 18th century that medicine took another step forward with the teachings and observations of such men as Haller and Morgagni. But the art of healing was still unable to utilize their methods and observations, or those of the empirical sciences. Consequently the public received very little benefit and confronted by such negative results was not inclined to place much reliance in the physician. It was not until the discoveries of the past century which resulted from the application of scientific methods, and the efficient public health measures, all of which have contributed to a striking decline in the morbidity and mortality from certain diseases, that medicine has assumed its high position as a social entity and that medical research has received public support and approval.

This attempt to emphasize the social aspect of medicine is without doubt one of the finest contributions to the study of the history of medicine.

LA PERMÉABILITÉ EN PHYSIOLOGIE ET EN PATHOLOGIE GÉNÉRALE.

By Ernst Gellhorn and Jean Régnier. Masson et Cie, Paris. 160 fr. 10 x 6½; 928; 1936 (paper).

There are few problems of general physiology that do not revolve in one way or another around the question of permeability. It is patent that in studying the dynamics of living cells the biologist must be intimately concerned with the entrance and exit of a variety of substances through the cellular membrane. The present text in French is a comprehensive survey of the permeability problem in both normal and pathological tissues and is a revised and expanded edition of the German volume *Das Permeabilitatsproblem* written by E. Gellhorn in 1929. The text is divided into four major sections: (1) backgrounds for the study of permeability; (2) permeability of cells; (3) permeability of organs, and (4) results and conclusions. The last section is the most interesting one in the book for it discusses critically such problems as the existence of the plasma mem-

brane and the various theories of permeability (the lipoid, colloidal, adsorption and ultra-filtration hypotheses). The book is provided with an excellent table of contents and has an adequate index and bibliography. Without question it is an important contribution to the field of general physiology and will be cordially received by both the student and the investigator.

Annales de Physiologie et de Physicochimie Biologique. *Tome 12, Nombres 2 et 3.*

Edited by L. Lapicque, A. Mayer and P. Portier. Gaston Doin et Cie, Paris. 25 francs for single number; annual subscription for 5 numbers: 120 francs (in France and Colonies); 135 francs (for countries that have a 50 per cent exemption of tariff on periodicals); 150 francs (for other countries). $9\frac{5}{8}$ x 6; No. 1, 169–300; No. 2, 301–524; 1936 (paper).

A series of three articles by E. Le Breton on the physiology of ethyl alcohol oxidation in the body constitutes the most important work published in numbers 2 and 3 of the 12th volume of this journal. These articles report numerous animal experiments made to determine, in particular, the relationship between alcohol concentration in the tissues and speed of oxidation, and the means by which the living organism utilizes alcohol. The results seem to show that alcohol is not a source of body heat nor is it utilized in muscular contractions. Both in rats and rabbits alcohol can be substituted for a considerable portion of other food without appreciable reduction of metabolism. These articles contain also a thorough and critical review of the literature on the subject and detailed descriptions of the experimental techniques.

Other papers in these numbers of the journal discuss individual variation in the nutrition of rabbits; the functions of the carotid sinus and aortic nerves; sex differences; the physico-chemical composition of the blood. The last, a very interesting study, is based on observations made on marine animals.

Connais—Toi ou la Physiologie sans Pleurs.

By Henri Coutière. Librairie Polytechnique Ch. Béranger, Paris. 16.50 francs. $6\frac{3}{4}$ x 4; 174; 1936.

The ignorant or even moderately educated layman knows so little about the functioning of his body, and consequently of the proper way to take care of it, that he is often preyed upon by quacks to the point of being reduced to poverty. M. Coutière is of the opinion that this fact is well known the world over, but few if any people try to do anything about it.

In his attempt to remove, in some small measure, the 'evil of ignorance', the author has prepared this small volume on human physiology. It is written in a popular style, and in language readily understandable to every class of people. The chapters are based on the broad organ-systems of the body, but all point toward the integrated functioning of the organism as a whole. The descriptions of the different organs are unique, and are made quite clear by the use of comparisons and analogies. The functions of the different systems of the body and the reactions (chemical, physical, or psychical) are adequately explained without the intricate and tiring detail so frequently associated with physiology texts.

Snow on Cholera. *Being a Reprint of Two Papers.*

By John Snow, together with A Biographical Memoir by B. W. Richardson, and an Introduction by Wade H. Frost. The Commonwealth Fund, New York; Oxford University Press, London. \$2.50. $8\frac{1}{8}$ x $5\frac{1}{2}$; xlviii + viii + 191 + 2 folding maps; 1936.

Dr. Frost considers John Snow's papers on cholera a "nearly perfect model of epidemiology." In a clear, easily understandable style, this eminent Victorian physician and scientist states his reasons for believing that the pollution of drinking water was responsible for widespread epidemics of cholera. Dr. Snow builds up the evidence with minute care and draws his conclusions from hundreds of individual case histories which he has laboriously amassed. Perhaps the most

dramatic proof of his theories came during the terrible Golden Square cholera epidemic, when Dr. Snow persuaded the Board of Guardians of St. James's Parish that if the pump handle were removed from the Broad Street pump, the epidemic would abate. The handle was removed and Dr. Snow was proved correct.

The second paper is entitled *On Continuous Molecular Changes, More Particularly in Their Relation to Epidemic Diseases.* Not the least interesting section of the book is the biographical sketch by Sir Benjamin Ward Richardson, himself a distinguished physician, who pays tribute to John Snow as a representative man of Victorian medicine.

DAS HORMON DES CORPUS LUTEUM *(Biologie, Chemie und Klinik).*

By Erich Fels. Franz Deuticke, Leipzig and Vienna. 12. marks (paper); 14.40 marks (cloth). $10\frac{1}{4} \times 7$; viii + 169; 1937.

In consideration of the large amount of work that has been published on various aspects of the corpus luteum hormone, this review of the literature and synthesis of the results of biological, chemical and clinical investigations is needed, if for no other reason than as a source of orientation. The author has himself contributed much to the present knowledge of the subject. Beginning with the historical development of the problem in general and a discussion of the test for the corpus luteum hormone, some of the specific aspects treated include: the biological action of the hormone on the uterus, vagina, and symphysis; its influence on the hypophysis and mammary glands; the relationship between ova and corpus luteum, and between the follicle hormone and the corpus luteum hormone; and the therapeutic value of the hormone in the treatment of menstrual disorders, sterility and difficult menopause. Dr. K. H. Slotta of the Instituto Butantan contributes a chapter on the chemistry and synthesis of the corpus luteum hormone. The book is annotated, contains author and subject indices, and is illustrated.

HUMAN PHYSIOLOGY. *A Practical Course. Second Edition.*

By C. G. Douglas and J. G. Priestley. Oxford University Press, New York; The Clarendon Press, Oxford. $4.25. $8\frac{3}{4} \times 5\frac{5}{8}$; xi + 229; 1937.

This fine text, in its present form, is an account of the practical course in Human Physiology for the Final Honour School in Oxford. It embodies much of the experimental work accomplished by the authors, as well as the newer and better techniques in the field of human physiology. The authors have made no attempt to cover the entire field of human physiology, but have deliberately omitted certain specialized branches, referring the student to texts by specialists in these fields.

The book has a very logical organization, and is written in a clear, straightforward style. The presentation of material gives evidence of much careful selection, alteration, and addition over the older edition. The many diagrams and charts should prove helpful to students using the book as a laboratory manual. The detailed table of contents, the short index, and the many footnote references to more detailed discussions, add substantially to the thoroughness of the volume.

HEALTHY GROWTH. *A Study of the Influence of Health Education on Growth and Development of School Children.*

By Martha C. Hardy and Carolyn H. Hoefer. University of Chicago Press, Chicago. $3.50. 9×6; xii + 360; 1936.

This is a report of a series of periodic observations on the health and physical and mental development of about 400 school children of Joliet, Ill. This study, which lasted from 1923 to 1935, had as its main objective to evaluate the effects of an intensive health education program. The children selected were classified into three groups: children who received intensive training, children who participated only in a limited way to the health program, and finally those children who were trained intensively only for a relatively short period. According to the authors, the results demonstrate the effectiveness of the training. Moreover they observed

that good health and good physical development is correlated with higher intelligence and better psychic adjustment. The figures as presented by the authors, however, do not warrant any very definite conclusions. The statistical technique, characterized by excessive faith in the correlation coefficient, is especially inadequate. Nevertheless, a great amount of data is given which will be found useful for other investigators.

RASSE UND KRANKHEIT.

Edited by Johannes Schottky, with articles by various authors. J. F. Lehmanns, Munich. In Germany: 20. marks (paper); 21–60 marks (cloth). Outside Germany: 15. marks (paper); 16.20 marks (cloth). $9\frac{3}{4} \times 6\frac{1}{2}$; xv + 468; 1937.

This book attempts to separate the wheat from the chaff in the numerous but sometimes uncritical observations on racial pathology. As Dr. Schottky laments, the anthropologist often knows too little of medicine to diagnose accurately, while on the other hand the physician knows too little of anthropology to give a satisfactory indication of the race of his patients.

After an introductory chapter of definitions and discussion of methodology and a chapter on racial physiology the material is arranged by medical specialties as follows: Internal diseases, infections, tropical diseases, dermatology, neurology, tabes and paresis, psychiatry, surgery, gynecology and obstetrics, ophthalmology, otolaryngology, dentistry, and malignant tumors. A final chapter deals with the effect of race mixture on disease. Bibliographies are appended to the various chapters and there are detailed name and subject indexes.

EXPERIMENTAL AND CLINICAL STUDIES OF THE SPINE OF THE DOG.

By Geoffrey B. Brook. William Wood and Co., Baltimore. $2.00. $8\frac{3}{8} \times 5\frac{5}{8}$; ix + 122 + 24 plates; 1936.

A book useful to clinicians in the diagnosis and treatment of diseases of the dog; also to the comparative worker utilizing observations on lower animals in the study and elucidation of disease processes in man. The two parts of the volume deal with: puncture of the *cisterna magna* in the dog (8 sections); and experimental and clinical investigations into the use of iodized oil as a radiopaque medium in the examination of the spinal subarachnoid space of the dog (4 sections). In an appendix is discussed certain features in X-ray photographs of the spine of the dog. Tabular matter and lengthy reference lists are given. The 50 figures (photographic reproductions) are placed at the end of the volume. There is no index.

THE PHYSIOLOGICAL BASIS OF MEDICAL PRACTICE. *A University of Toronto Text in Applied Physiology.*

By Charles H. Best and Norman B. Taylor. William Wood and Co., Baltimore. $10.00. 9 x 6; xxi + 1684; 1937.

The two eminent Canadian physiologists have prepared this textbook to serve the purpose of giving the medical student a clearer view of the physiological changes which take place in disease. The exposition of the subject matter is as follows: (1) blood and lymph; (2) circulation of the blood; (3) respiration; (4) excretion of urine; (5) digestion; (6) metabolism and nutrition; (7) endocrines; (8) nervous system. After a brief description of the anatomy of the organs or systems involved, the experimental data relative to the particular physiological process are presented and discussed with particular reference to information regarding man in health and disease. It is a well written text, clear and comprehensive, and will be found very useful. There is an extensive bibliography of 99 pages.

A STUDY OF THE DIETS OF SIXTY-NINE WORKING CLASS FAMILIES IN NEWCASTLE UPON TYNE.

By J. A. Charles. City and County of Newcastle upon Tyne, Health Department, Newcastle upon Tyne, England. 1s. $9\frac{5}{8}$ x 6; 45 + 1 folding table; 1936.

A practical inquiry into the food consumption and expenditure of certain working class families (38 unemployed, 28 employed, 3 widows) during the autumn of 1934. All food stuffs (including all waste as well as food supplied to dogs and cats) were weighed, measured and recorded during the investigation, which lasted a week for each family. Nearly all the members of each family were weighed during the survey and blood haemoglobin was determined in most cases at the end of the study. The data are presented in tabular form. Considerable space is given to a discussion of the nutrient value of the food stuffs consumed by the unemployed as compared with that of the employed, the physical condition of the different members of the two groups of families, etc.

THE PHYSIOLOGY AND PHARMACOLOGY OF THE PITUITARY BODY.

By H. B. Van Dyke. University of Chicago Press, Chicago. $4.50. 9 x 6; xvii + 577; 1936.

The author attempts to describe and evaluate the research done on the pituitary body from 1920–1935, in all its different phases, including physiology, pharmacology and related sciences. Because of the tremendous number of new discoveries that have been made in recent years, particularly in the last decade, there is need for such a book as this which gives a complete, condensed and critical restatement of the work. Chapter I is a description of the anatomy of the pituitary and is followed by chapters on the interrelationship of the hormones of the different parts of the pituitary with other glands and organs of the body, effects of extracts of the various parts of the gland, etc. There is a bibliography listing over 3,000 publications. The author himself consulted some 5,000 articles.

AN INTRODUCTION TO MEDICAL SCIENCE.

By William Boyd. Lea and Febiger, Philadelphia. $3.50. $9\frac{1}{2}$ x $5\frac{3}{4}$; 307; 1937.

As the author states, the object of this book is "to give an aeroplane view of disease." Intended primarily for nurses, premedical students and laymen, it is limited to a brief discussion of disease entities and associated pathology. In the first part of the book, the reader is given a general view of human anatomy and physiology, in the second there is a description of the various body organs in health and disease. The third part discusses disease prevention and public health measures. It is a well written book, comprehensive and contains excellent illustrations. The author has achieved his objective and his volume should prove popular.

NUTRITIVE AND THERAPEUTIC VALUES OF THE BANANA. *A Digest of Scientific Literature.*

Research Department, United Fruit Co., Boston. Free. $9\frac{3}{4}$ x $6\frac{7}{8}$; 143; 1936 (paper).

Until the last few years the banana was largely condemned as an indigestible article of food. The tide has turned in recent years, however, and it is now fast becoming highly valued as a source of vitamins and easily digested food and as a direct aid in the correction of various physical ailments. Reasons for this change of thought are brought out by a perusal of this annotated bibliography which presents digests of some 292 articles which have appeared in scientific literature during the last two or three decades showing the results of experimentation and research upon the value of the banana in the daily diet.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Lieferung 460. Abt. V. Methoden zum Studium der Funktionen der einzelnen Organe des tierischen Organismus, Teil 10, Heft 6. Allgemeine und vergleichende Physiologie.* Containing following articles: *Eine Verstärkeranordnung zur Registrierung gehirnelektrischer Spannungen,* by Rudolf Ottenthal; *Neue kataphoretische Verfahren,* by Hugo Theorell; *Lichtelektrische Zellen*

und ihre Anwendung für wissenschaftliche Messungen, by Marie Wreschner.

Urban und Schwarzenberg, Berlin. RM. 9. 10 x 7; 160; 1936 (paper).

These three papers present descriptions of apparatus, directions for its use and examples of application. The first apparatus described is one perfected in the *Institut für elektrische Fernmeldetechnik* in Vienna and is designed for amplifying the electrical potential of brain discharge. The subject of the second paper is cataphoresis, and that of the third, photoelectric cells.

INFANT MORTALITY IN MEMPHIS. *U. S. Department of Labor, Children's Bureau, Publication No. 233.*

By Ella Oppenheimer. Government Printing Office, Washington. 15 cents. $9\frac{1}{4}$ x $5\frac{3}{4}$; vii + 103; 1937 (paper).

In 1932 the infant mortality rate in Memphis, Tenn. was higher than that of any other city of 100,000 or more population. In 1933 and 1934 it continued to increase and a study of the situation was requested. U. S. Census statistics and local studies were analyzed. In this report the possible and probable causes are discussed and recommendations suggested for remedying the existing conditions. It is interesting to note that already a beginning has been made with the appointment of a Director of Maternal and Infant Hygiene in the Memphis Department of Health.

EXPERIMENTAL PHYSIOLOGY.

By George H. Bell. John Smith and Son, Ltd., Glasgow. 4s. 6d. net. 9 x 7; 70; 1937.

This is a laboratory manual designed for an elementary course in medical schools. The directions are all very explicit requiring little added instruction by the teacher, who may therefore handle a rather large class without assistance. No explanations are given of phenomena to be observed, blank pages being left for the student to fill this in for himself. As almost no preliminary knowledge is required on the part of the student and little expensive apparatus is called for, we would recommend the use of this book in a college course for premedical students though a few of the demonstrations might have to be omitted for lack of equipment.

NOTES ON CLINICAL LABORATORY METHODS. *Third Edition.*

Standing Committee on Laboratory Methods, University of Glasgow. John Smith and Son, Ltd., Glasgow. 2s. net. $7\frac{1}{4}$ x $4\frac{3}{4}$; 86; 1936.

The purpose of this little book, issued under the direction of a committee of twelve representing the General Hospitals and University of Glasgow, is to secure uniformity of the laboratory methods in the teaching of clinical medicine in hospital wards. The field covered "is equivalent to side-room testing as usually carried out by students of medicine, together with further tests introduced with a view to completeness." A number of figures and tables are included in the text and there is an excellent index.

HORMONE.

By H. Giersberg. Julius Springer, Berlin. RM 4.80. $7\frac{1}{8}$ x $4\frac{5}{8}$; vi + 169; 1936.

This little book on hormones, No. 32 of a series of scientific publications for the layman, contains much that is interesting about endocrine glands for the general reader. The author starts with a discussion of the thyroid, about which most is known and where the lack or excess of the specific hormone is most spectacular. Subsequently he discusses parathyroids, adrenals, pancreas, pineal and thymus glands, gonads, pituitary gland, hormones and vitamines, hormones among the invertebrates, plant hormones, and a final general chapter on the characteristics of hormones.

WEGE DER SEUCHEN. *Lebensgemeinschaft, Kultur, Boden und Klima als Grundlagen von Epidemien. Unter Berücksichtigung der Tropenkrankheiten dargestellt.*

By E. Martini. Ferdinand Enke, Stuttgart. RM. 6. (foreign countries 25 percent less). 10 x $6\frac{1}{4}$; vi + 109; 1936 (paper).

The influences of culture, soil and climate on the origin and course of epidemics are treated first separately and then in interrelation with each other. Examples are drawn from all parts of the world but special emphasis is placed on tropical diseases, both animal and human. The first chapter discusses briefly typical life cycles of parasites and bacteria, grouped by modes of transference of infection. Statistical methods of analysis and prediction are presented in an appendix. There is an index.

La Santé Publique et la Collectivité. *Hygiène et Service Social, Coordination. Rapport général de la Commission d'Hygiène de la III^e Conférence du Service Social, Londres, juillet 1936.*

By Henri Sellier and R. H. Hazemann. Ministère de la Santé Publique, Bruxelles. $8\frac{3}{4}$ x 6; 72; 1936.

The Minister of Public Health and Physical Education of France and the chief of his technical cabinet outline the aims of public health, its present organisation and its effects. The benefits of centralised administration are stressed, particularly for long-range national planning. Social hygiene is shown not to encroach upon individual rights, but rather to insure them, through euthenic means of wide application.

Alters-Forschung. *Untersuchungen und Berichte über Lebensdauer, Altern und Tod. 3. Jahrgang.*

Edited by Josef Kluger. Josef Kluger, Gartenstrasse 135, Wünschelburg-Heuscheuer. RM. 3. $9\frac{1}{4}$ x $6\frac{1}{2}$; 40; 1937 (paper).

The third volume of this journal devoted to research in longevity contains two articles about old age in plants and trees, with special reference to the *Affenbrotbaum*. More than half of the journal is taken up with a bibliography of various phases of longevity of the articles appearing in 1935—36 and some from 1934. The more important works are reviewed briefly.

BIOCHEMISTRY

Prelude to Chemistry. *An Outline of Alchemy, its Literature and Relationships.*

By John Read. Macmillan Co., New York. \$5.00. $9\frac{1}{4}$ x 6; xxiv + 328; 1936.

Dr. Read here presents a very careful and scholarly volume on the history, literature, and underlying idea of alchemy from the earliest Egyptian and Chinese origins until the final sounding of its death knell by Lavoisier. Beginning apparently with the practical metallurgical arts, mysticism soon overshadowed experimental evidence, and the basic metaphysical principals which dominated alchemical philosophy until the very end were well established before the Christian era. The extravagant writings of the late renaissance period show little originality of thought, all harking back to the ancient sulphur-mercury theory in their directions for proceeding with the "Great Work" of preparing the philosophers stone, while giving free rein to most lively imaginations in their allegorical descriptions and in their claims of magical power. The present volume is profusely illustrated with reproductions of original engravings and is largely devoted to a discussion of their significance. Unfortunately, little is said of the actual procedures used by the alchemists aside from what they themselves tell in guarded language. The bibliography is extensive and there is a glossary.

An Introduction to Comparative Biochemistry.

By Ernest Baldwin. Foreword by Sir Frederick G. Hopkins. University Press, Cambridge; The Macmillan Co., New York. \$1.50. $7\frac{1}{4}$ x 5; xviii + 112; 1937.

The comparative point of view in any scientific study is always an illuminating approach. In such fields as anatomy, embryology and physiology it has become accepted as commonplace but this has not lessened its value. In biochemistry, how-

ever, emphasis in the past has been placed largely on particular kinds of animals and no great attempt has been made to compare, for example, the fundamental chemical reactions of invertebrate with vertebrate organisms. The present book, although short, makes an interesting start in this direction and is deserving of commendation for this reason. The author prepared the book (1) to serve as a text for students taking elementary biochemistry and (2) to provide a starting point for those who "may find themselves attracted to the subject." The material is both well chosen and clearly presented and covers such topics as the ionic composition of the blood, the regulation of osmotic pressure, nitrogen metabolism, respiration processes, pigments, etc. The book has an index and bibliography and is modestly illustrated. There is a complimentary "Foreword" by Sir Frederick Gowland Hopkins, F. R. S.

Practical Physiological Chemistry for Medical Students.

By G. M. Wishart, D. P. Cuthbertson and J. W. Chambers. John Smith and Son, Ltd., Glasgow. 3s. 6d. net. 7 x 4¾; 127; 1936 (paper).

Even when a laboratory manual is written for use with a particular text book, the experimental work of a course is apt to get quite out of line with the current lectures and text assignments. This book meets that difficulty by combining a laboratory manual with a text book. The union seems to be a happy one. It is designed for a 40 hour course with a large class and a small teaching staff. Both the theoretical discussions and the experimental instructions are written clearly and simply in a rather non-technical language so little preliminary knowledge is required on the part of the student. The subject matter is confined to points of the most direct interest to practitioners, so does not go deeply into theoretical chemistry. We consider it rather unfortunate, however, that absolutely no references are given. For convenience in taking lecture and laboratory notes one side of each page is blank. A rather serious handicap for a book to be used in the laboratory is the fact that the paper cover is not sewn on and has already half fallen off of our slightly handled review copy.

Manual of Biological Assaying.

By James C. Munch. J. B. Lippincott Co., Philadelphia. $2.00. 11 x 8½; 179; 1937.

This book, organized on the laboratory outline plan, attempts to summarize a number of techniques of interest to the experimental biologist, pharmacologist, and clinician. Biological assays are defined as "tests of potency of drug products conducted upon intact living animals, or upon surviving isolated tissues." The author discusses such specific assays as anthelmintics, antisyphilitics, insecticides, vitamins, thyroid extracts, etc. Each discussion gives, where possible, information on the physiological action, assay procedure, potency and reference standard of the drug in question. A number of blank forms are included to facilitate the recording of actual results. The book has a good bibliography and an index and should prove useful to workers requiring the detailed knowledge it provides.

Chemistry of Food and Nutrition. *Fifth Edition Completely Rewritten.*

By Henry C. Sherman. The Macmillan Co., New York. $3.00. 7¾ x 5¼; x + 640; 1937.

The fifth edition of this book has been completely rewritten in order to include the recent important advances made by research work in the chemistry of food and nutrition. An excellent text for college classes and a sound reference book. The volume is well documented and indexed.

Quarterly Bulletin of the Health Organisation. *Special Number, November 1936. Biological Standardisation, II.*

League of Nations, Geneva. Columbia University Press, New York. 65 cents. 9½ x 6¼; 175; 1936 (paper).

This report brings up to date the extremely valuable work of the Commission on Biological Standardisation. By far the largest part of the report is devoted to insulin, eleven papers being presented on this subject.

SEX

THE CHARACTER, CAUSE, AND CURE OF HIGH SCHOOL IMMORALITY (*Syllabus*).

By Teunis Oldenburg. Calvin Press, Grand Rapids, Mich. 35 cents. 7 x 5 + 48; 1936 (paper).

The author regards "every form of unnatural sex behavior as a sin" and believes that "sex perversion antedates Society and was . . . inherited from our first parents through the Fall in Paradise."

With this point of view, a program of educational legislation, censorship and discipline is proposed. The school would return to orthodoxy. Teaching personnel would be carefully selected and supervised on a moral basis, completely masculised except for kindergarten and domestic science. Mixed company in High School clubs would be tolerated only during the daytime and under strict supervision; evening parties and all automobiling forbidden. Sexology would be relegated to parent-teacher societies, and advice therein given by "a Christian physician or a well-informed clergyman."

[Reginald, the Office Boy, was pretty scornful about this *opus*. "Anybody ought to know that it is a lot easier to prohibit alcohol than hormones, and look where we got prohibiting alcohol! What they ought to do is make Casanova and the Decameron required reading in high school," was his wicked and subversive comment.]

ON YOUR GUARD! *The Prevention and Treatment of Sex Diseases.*

By Carl Warren. Foreword by M. J. Exner. Emerson Books, New York. $1.00. $5\frac{1}{4}$ x $7\frac{5}{8}$; xvii + 160; 1937.

This little book is, in the words of the author, "a discussion of personal health aimed to be at once frank, practical and scientifically accurate." The greater part of the material had originally been published as a series of articles, written by Mr. Warren for the New York *Daily News*. There are separate chapters on the history of venereal diseases, symptoms and treatment of syphilis, and of gonorrhea, prophylaxis, and on the cost to society of venereal infection. Since the book was written for the layman, every effort has been expended to make the account as clear and non-technical as possible. Sensational statements of any sort have been scrupulously avoided, and nowhere in the text can we recall having come across a single moral judgment. There is a two page index.

A TEST FOR MEASURING SEXUAL EXCITABILITY IN THE FEMALE RAT. *Comparative Psychology Monographs, Volume 14, Number 1, Serial Number 67.*

By Josephine Ball. The Johns Hopkins Press, Baltimore. $.75. 10 x $6\frac{3}{4}$; 37; 1937 (paper).

This paper describes in detail a method of testing and rating the sexual excitability of female rats (reliability coefficient .94). It also gives "results of investigations of normal reactions, including a more detailed and extensive study than has been reported heretofore of the exact relations of sexual excitability to vaginal changes as determined by smears, individual differences in excitability and the consistency of behavior from one cycle to the next in the same individual." Graphs and tables are included in the test and there is a list of 17 references.

THE INTIMATE SIDE OF A WOMAN'S LIFE.

By Leona W. Chalmers. Foreword by Winfield S. Pugh. Illustrated by Frank H. Netter. Pioneer Publications, Inc., New York. $1.50. 8 x $5\frac{1}{4}$; 128; 1937.

The author, a lay woman, has gathered her subject matter apparently from extensive reading of reliable authorities. The thesis of the book is "keep clean," and the main body of the book is taken up with extremely minute information about the

technique of keeping clean. There is no index.

BIRTH CONTROL FOR SAINTS AND SINNERS. *A Critical Consideration of the Birth Control Movement as Influenced by Science and Revelation.*

By Teunis Oldenburger. Calvin Press, Grand Rapids, Mich. $2.50. $7\frac{3}{4} \times 5\frac{1}{4}$; xx + 330; 1934.

In the Gospel according to Teunis Oldenburger it is written that birth control is against God's will. Which of course settles the question. In reading such books as this we always wonder how the authors managed to install a private wire to heaven.

BIOMETRY

DER GEBURTSTOD (*Mutter und Kind*).

By Sigismund Peller. Franz Deuticke, Leipzig and Vienna. $9\frac{3}{4} \times 6\frac{1}{2}$; vii + 110; 1936 (paper).

This remarkable little book presents a thorough review and discussion of post-war research on the mortalities of infancy, childhood and maternity, mostly in Europe and particularly in Germany and Austria where so many such studies were produced up to the advent of fascism. Much of the original work is the author's own. The general aim is to show that the death rates of well-developed and normal infants and mothers is connected less with the "skill" of gynecologists, medical techniques and health officers than with various economic and social conditions, many of which are far from clear.

The most striking evidences for this thesis are drawn from such facts as (1) The marked increase in infant mortality, especially of the first two days of life, in most districts of Central Europe during the first four years after the war- the "post-war hunger period."(2) The universally higher mortality rates of illegitimates, which was especially marked during this period. (3) The rapid diminution of death rates which followed returns to moderate prosperity. This was most striking in England. (4) Marked correlations between mortalities and social-economic levels as determined by a number of criteria, principally occupational. (5) The much lower mortalities of mother and infant when the former retires from working during the last two or three months before delivery. The universally low rates of Jews may be partially explained by this and similar folk customs.

Many other factors are analysed in connection with variation in mortality rates: race and nationality, constitution, urban versus rural habitat, size of family, parity, particular month of birth, general mortality rate and special morbidity rates of the locality, prevailing medical facilities, religious affiliation, and others. These are shown on the whole to be subordinate to those factors which come under the general heading of "Social Hygiene". New data are quoted on partial correlations between social factors and mortality, which confirm this. Local variations in the regulations for recording vital statistics which may lead to error of interpretation are noted. Fewer data are presented on mothers' mortalities than on infants'. But the analysis is as thorough, and the conclusions similar. Unfortunately the book has no index or tabulation of references.

WORLD STATISTICS OF ALIENS. *A Comparative Study of Census Returns 1910–1920–1930. Studies and Reports Series O (Migration) No. 6.*

International Labour Office, Geneva. International Labour Office (Washington Branch), Washington, D.C. $2.75. $9\frac{1}{2} \times 6\frac{1}{4}$; vii + 251; 1936 (paper).

In making a critical examination of the data of these eight international tables of alien statistics, one should keep in mind some of the difficulties involved in their compilation, as for instance, the fact that the dates of censuses do not correspond and that the term "alien" has a different interpretation among the different countries and sometimes even in the same country at different times—being classified variously according to political nationality, country of birth, or by race.

Tables I through V list aliens by na-

tionality both by absolute figures and by percentages for countries or territories of Africa, America, Asia, Europe and Oceania. Table VI:—Nationals resident abroad in 1910, 1920 and 1930, by continent. Absolute figures and percentages of world total of all nationals resident abroad. Table VII:—Nationals resident abroad in 1930, by country—as percentages of world total of each nationality resident abroad. Table VIII:—Nationals of European countries resident in other European countries in 1930—as percentages of the total number of each nationality resident in other European countries. There are four introductory chapters, 67 pages of explanatory notes and an appendix with miscellaneous tables concerning alien distribution in years prior to 1910.

GAME FISH RECORDS.

By Jock Scott. H. F. and G. Witherby, Ltd., London. 12s. 6d. $8\frac{3}{4} \times 5\frac{1}{2}$; xii + 316; 1936.

Interesting statistics can be reported and interpreted on the subject of game fishing with no mention of probable error. The present book presents the largest known sizes of various game fish, including time and place and name of the catcher; casting records; and assorted records of performance, both human and piscine. Principally concerned with Great Britain, it also gives one chapter apiece to Canada, South Africa, New Zealand and Tasmania, United States, and Northwestern Europe. The anecdotes are illustrated with photographs and cartoons from "Punch".

ANTHROPOMETRY OF THE NATIVES OF ARNHEM LAND AND THE AUSTRALIAN RACE PROBLEM. *Peabody Museum of American Archaeology and Ethnology, Harvard University, Vol. XVI, No. 1.*

By W. W. Howells. Data collected by W. L. Warner. Peabody Museum of Archaeology and Ethnology, Cambridge. \$1.00. $9\frac{5}{8} \times 6\frac{1}{4}$; viii + 97; 1937 (paper).

The measurements on which this analysis is based include 239 adult male northern Australian aborigines and 69 females. These differ little in physical type from aborigines of other parts of Australia. The author concludes with Sir Arthur Keith that the Australian is not a blend but a major race which represents an earlier stage in the development of *Homo sapiens* than does any other existing race.

PSYCHOLOGY AND BEHAVIOR

PSYCHOLOGY AND THE SOCIAL ORDER. *An Introduction to the Dynamic Study of Social Fields.*

By J. F. Brown. McGraw-Hill Book Co., New York. \$3.50. 9 x 6; xiv + 529; 1936.

This book deals with the methods and principles which underly the application of *Gestalt* psychology to the study of sociology. The subject matter is presented in four sections: methodology, sociology, psychology, and political science. In the first, the author discusses the advantages of the organismic as opposed to the vitalistic and mechanistic theories. From it he proceeds to build the logical construction of the field theory which is founded on "organismic philosophy, hypothetico-deductive methods and language of constructs." In the following sections he discusses problems of race, religion, personality, psychoanalysis, fascism, communism, etc.

The author's forceful and well written presentation of this subject will arouse the admiration of the reader. This treatise stands far above the majority of similar works so that one is tempted to overlook some of the defects inherent in the theory and in the author's viewpoint. These are examples of two fallacies. First, that the treatment of social science is "scientific" as long as geometric symbols are used in the exposition. Second, that since "there is no such thing as human nature independent of the structure of the social field" one need only study the environment and forget completely that also there is no structure of the social field independent of human nature. Most remarkable of all is the author's belief that fascist dictatorship is different from communist dictatorship simply because in one the leaders belong to a class labelled bour-

geois, while in the others they label themselves proletarian.

Psychometric Methods.

By J. P. Guilford. McGraw-Hill Book Co., New York. $4.50. 9 x 6; xvi + 566; 1936.

"The primary aim of this volume is to teach the student of psychology how to deal effectively and intelligently with quantitative data." The first of its three parts concerns statistical methods as applied to experimental psychology, in particular to the psychophysical methods. Two chapters of introduction to general statistics are included here. The second part concerns psychological scaling methods. The third part on correlation and test methods has five chapters as follows: The elements of curve fitting; Simple correlation methods; Multiple and partial correlation; Mental-test methods; Factor analysis.

An enormous range of topics is covered, thoroughly and systematically with a wealth of illustrative material and examples which include some of the latest of researches. Little statistical theory is presented, but the application of methods is presented with great detail and systematisation. Important formulas are given serial numbers: they total 235, not including alternatives. A helpful list of problems and exercises follows each chapter. Author and subject index are given, also an appendix of 14 useful tables of figures (36 pages).

The book represents at once a most ambitious achievement in itself, and a milestone of the recent achievements in its own wide field. It should prove invaluable in college teaching for many years.

Controlling Human Behavior. *A First Book in Psychology for College Students.*

By Daniel Starch, Hazel M. Stanton, Wilhelmine Koerth, assisted by Roger A. Barton. The Macmillan Co., New York. $2.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; xiv + 638; 1936.

This work is the organized, practical advice of three social psychologists in several fields of personal adjustment. "It is designed to deal with problems connected with making a living, with establishing and maintaining family life, with behavior in business operations, with influencing people in the mass, and finally with the art of effective, satisfying living."

Part I, on controlling the behavior of self, considers the topics: habit formation, learning and study, efficient thinking, memory, concentration, efficiency in the mobilisation of energy, control of emotions and feelings, adaptations to various social groups, selection of vocation, and "behavior hygiene." While the treatment is laborious and the scientific basis not always clear, a great many valuable suggestions are made, from which few persons would fail to derive profit.

Part II, on controlling the behavior of others, is more frequently open to challenge and of less obvious merit, excepting the first chapters which concern children. Other chapters discuss vocational direction, abnormal persons, commercial advertising, public speaking, music and art and entertainment, propaganda, and religion.

A vast amount of material is presented, in great detail and unabstrusely, even if not always convincingly. With a base which extends over broad fields of biology, anthropology, sociology and economics, however, it is impossible to avoid some points which are highly controversial.

Biology and Human Behavior.

By Mark Graubard. Tomorrow, Publishers, New York. $2.50. $7\frac{3}{4}$ x $5\frac{1}{2}$; 413; 1936.

The first seven chapters of this book constitute a well written outline of elementary biology with particular emphasis on the study of tropisms and genetics. In the remaining chapters, two in number, the author presents his ideas on the relationship between biological facts and sociological theories. He contends that the sociology of Marx and Engels is the "only complete and scientific" realization of the biological phenomena which underly social behavior. His arguments are too complex to be summarized. It is sufficient

to note that he concludes that social differences between individuals are essentially a product of the environment and not of any innate biological variation; capitalistic society has conditioned man towards evil and crime; if all had equal opportunities this would be an earthly paradise; and so on. All good old Marxist beliefs.

Psychological Studies of Human Variability. *Psychological Monographs Number 212. Dodge Commemorative Number. Edited by Walter R. Miles. Psychological Review Company, Princeton and Albany.* \$4.50. $9\frac{3}{4} \times 6\frac{1}{2}$; xxx + 415; 1936 (paper).

This Dodge Commemorative volume of Psychological Monographs is a composite publication of the works of many devoted friends and students of Professor Raymond Dodge. The preface contains an account of his life and contributions to psychology and to the Institute of Human Relations. There are four chapters dealing respectively with experimental studies on children, experimental studies on adults, experimental studies on visual functions, and historical and theoretical studies. A wide range of topics is covered, from such things for instance as age and job satisfaction, to highly complicated researches on vision.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

The Belly Book *or Diner's Guide. By C. Louis Leipoldt. Williams and Norgate, London.* 7s. 6d. net. $7\frac{1}{4} \times 4\frac{3}{4}$; xx + 298; 1936.

This thorough-going treatise is a review, with a new title, of a book that appeared over a quarter of a century ago. The author is a medical man, of good taste gastronomically speaking, as well as good judgment scientifically. The book is replete with dietetic common sense. He blows up food fads and fallacies—including some very orthodox ones—with a succession of resounding bangs.

The chapter on the art of dining is a major contribution to aesthetics. It contains among other things, a series of menus, with comments by the author. One of these we cannot forbear quoting, just to make the mouths of lustier zoologists water:

Family Dinner in Trinidad

Mangrove oysters served on ice with pepper-vinegar and limes
Purée of lentils with toasted bread
Boiled red snapper with mayonnaise sauce
Blue crab farci
Stewed lappe served with akee
Avacado salad
Morocoy with pigeon peas
Coconut ice
Guava preserve with cream
Dessert
Cheese
Coffee Liqueurs

The mangrove oyster is one of the finest of the oyster class, and is brought in direct from the swamps with the bivalves still clinging to the root, and opened fresh for the table. The pepper-vinegar must also be freshly made. The blue crab is a species of land crab which needs special preparation. The crabs are kept in a "crabbery" which is attached to the kitchen, and are fed on a special diet in which capsicums enter largely. At the end of a month they are fit for the table. The crab is boiled and the flesh from its big claws and legs carefully removed, the shells being kept. This flesh is then fried, after being mixed with breadcrumbs and spices, and the farci thus prepared is replaced in the empty shell, in which it is served up. Lappe is the Creole name for the pacca (*Cœlogenys pacca*), a species of *agouti*, which is one of the most succulent morsels of game that any gourmand can wish for. Akee is the boiled fruit of *Blighai sapida*, and is an excellent vegetable to accompany the game dish when plainly boiled in milk. Morocoy is a land-tortoise (the Brazilian tortoise, *Testudo tabulata*), the preparation of which is very elaborate, since it must be stewed in Bordeaux wine with spices. The liver of this animal is highly esteemed and surpasses in flavour the best qualities of *foie gras*. Pigeon peas are derived from an Indian shrub, and replace in Trinidad the ordinary new peas. Coconut ice as prepared in the West Indies is a delicacy which no one who has only tasted the imitations made on this side of the Atlantic can presume to know the full value of. It is prepared from the milk and jelly found in the tender young nut, which are boiled and mixed with cream and then slowly frozen. Guava preserve is obtainable in England, and, served with cream, is an admirable dessert dish. The cheese figuring on this menu is the flaky *queso de mano* which comes from Venezuela, and which has a mild and agreeable flavour.

The book has a long and interesting bibliography, and is thoroughly indexed. We strongly recommend this treatise to all and sundry.

THE WORLD OF SCIENCE.

By F. Sherwood Taylor. Reynal and Hitchcock, New York. $3.75. 9 x $5\frac{1}{8}$; xvi + 1064 + 48 plates; 1937.

Shortly after the appearance of H. G. Wells' *Outline of History* the literary world was flooded with imitative outlines of various things—art, science, literature, religion, among them. Most of these showed signs of having been very hurriedly assembled, and practically all were distinctly inferior to their prototype.

It is unfortunate that Dr. Taylor did not contribute to this series a volume or two dealing with science, for he would have done it admirably, if the present volume is any indication. Like a true outline, it covers the diverse fields of physics, astronomy, geology, chemistry, and biology, in such a way as to make clear that which they have in common and the way in which they contribute to each other. The material is so logically arranged that the narrative appears continuous, and there is a mine of information about the theories of Rutherford, Planck, and Jeans, that will be found most helpful by those who have been brought up on the classical physics of Galileo, Newton, and Laplace.

For obvious reasons the author has omitted the sciences of mathematics and psychology, which seems a wise decision. There is an index of thirteen pages and a list of acknowledgments of five, which latter seems to indicate that the author has left no stone unturned in his search for coöperation in producing a readable book.

THE LEICA MANUAL. *A Manual for the Amateur and Professional Covering the Entire Field of Leica Photography.*

By Willard D. Morgan, Henry M. Lester and Contributors. Morgan and Lester, New York. $4.00. 8 x $5\frac{1}{2}$; 484; 1937.

Probably most minature camera users have little or no idea of the vast possibilities of their cameras. To them and to other amateur photographers this extremely informative book will prove most revealing. Although the material pertains specifically to the Leica camera, it can, with the exception of a few special instances, be applied equally as well to other similar miniature cameras.

The volume contains detailed information about equipment as well as about technique of taking, developing, printing and enlarging the photographs. In addition, there are chapters devoted to special types of photography (such as dental, ophthalmic, microscopic, and aerial) which indicate the possibilities of such a camera for use by biologists, geographers, etc. The editors have done an excellent piece of work and the answer to practically any question on the subject, which is at present answerable, can be found in this book. The text is illustrated with a large and varied assortment of photographs, and there is a detailed index.

NATURWISSENSCHAFTLICHE ERKENNTNIS UND IHRE METHODEN.

By M. Hartmann and W. Gerlach. Julius Springer, Berlin. RM. 2.40. $8\frac{3}{4}$ x $5\frac{1}{2}$; iv + 70; 1937 (paper).

These two provocative essays, *Wesen und Wege der biologischen Erkenntnis*, by Max Hartmann and *Theorie und Experiment in der exakten Wissenschaft*, by Walther Gerlach, were originally presented as lectures at the 1936 meetings in Dresden of the *Gesellschaft Deutscher Naturforscher und Ärzte*. The first treats the biological sciences, with illustrative examples taken mainly from genetics; the second has physics for a subject. They are printed together here because of the similarity in viewpoint as to the relative positions of deductive and inductive reasoning in scientific research and philosophy. Both emphasize intensive and extensive collection of data and synthesis with results on the same problem obtained from other sciences.

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THE QUARTERLY REVIEW *of* BIOLOGY

ESTABLISHMENT OF THE NERVOUS SYSTEM

By J. FRANK DANIEL

University of California

INTRODUCTION

IN THE following paper I should like to apply the most recent method used in modern zoology, that of experimentation, to the ancient problem of substance and pattern. Our question is: How is the substance of a single cell, the ovum, transformed into a pattern of many diverse systems, including the complex nervous system? To the early biologist who regarded the structures characteristic of the adult as preformed in the egg, development of a complicated pattern meant only the growth of the preformed rudiments into the adult systems.

Roux (1883), who early studied the causal relations underlying development, postulated cleavage of the egg as a factor in its unfolding. He assumed that the two daughter nuclei, resulting from the division of the original fertilization or fusion nucleus, determine the first two cells or blastomeres for right and left sides of the body, and that succeeding early daughter nuclei set the succeeding blastomeres for particular parts, thus building up a mosaic like a tile pattern. Hertwig (1893) showed, however, that through pressure on the dividing egg the early daughter nuclei may be shifted into blastomeres which are foreign to them, and that these blastomeres regardless of their strange nuclei give rise to their proper parts of the embryo. Evidence of this type indicated that in early cleavage the nuclei do not set the cytoplasm for particular ends.

Spemann (1928) appears to have proved this point. By placing a noose around the jelly of an egg of the European newt, *Triton taeniatus*, and constricting the egg in such a way that both the male and the female pronuclei (*n.*, fig. 1a) remain on same side of the noose, he showed that the side containing the fusion nucleus (fig. 1b) develops, but that the cytoplasm on the opposite side of the noose does not develop *unless* a daughter nucleus enters it. If, however, a one-eighth or even a one-sixteenth of the original fusion nucleus passes over the uniting bridge into this cytoplasm, then this cytoplasm also is activated to development. But what it develops into depends not upon the daughter nucleus entering it, as Roux had postulated, but upon the cytoplasm itself. Thus, if we assume that in figure 1b the

noose is so tied as to bisect the so-called gray crescent (dorsal organs), then the cytoplasm in which the original nucleus is located will form a *complete* embryo and not a half, and the cytoplasm on the opposite side of the noose into which the daughter nucleus enters late will also form a whole embryo; so that by this sagittal constriction twins result (fig. 2a). If, on the contrary, the constriction is frontal, separating the dorsal organs from the yolk, then the cytoplasm of the dorsal organs will produce an embryo (*em.*, fig. 2b); while the other half into which one-eighth nucleus enters may form parts of all of the other germ layers, yet it will not produce an embryo. This experiment demonstrates that while one-eighth of the original nucleus is sufficient to instigate development, it is the cytoplasmic substance itself which is responsible for the pattern.

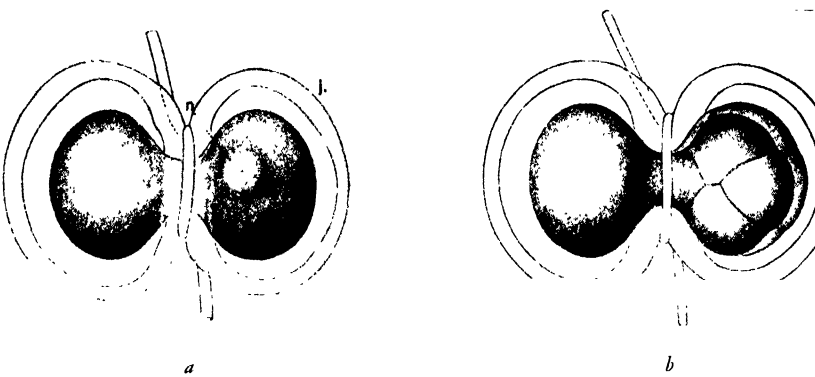


FIG. 1. CONSTRICTION OF THE EGG OF TRITON TAENIATUS

a. A noose is placed around the jelly (*j.*) in such a way that both male and female pronuclei (*n.*) remain on the same side of the noose. *b.* The cytoplasm on the side containing the fusion nucleus has undergone cleavage, while that on the opposite side (without a nucleus) has not developed. (After Spemann, 1928.)

FIG. 2. DEPENDENCE OF THE PATTERN OF DEVELOPMENT UPON THE CYTOPLASM

a. Twins of *Triton taeniatus* resulting from sagittal constriction of a single egg, bisecting the gray crescent (dorsal organs), a daughter nucleus having entered the non-nucleated cytoplasm late from the opposite, developing side. *b.* Normal embryo (*em.*) and mass of cells which result if the noose is so placed as to form a frontal constriction separating the dorsal organs from the yolk. (After Spemann, 1928.)

A. DETERMINATION OF THE MEDULLARY PLATE

(*a*) *In normal development*

From the above experiment of Spemann it appears that the region early occupied by the gray crescent (dorsal organs) is a center of organization from and around which important systems of the embryo are built. Vogt (1929), by means of vital dyes, has mapped out this area as an oblique cresent (chorda; somites; fig. 3) around the egg and has shown its surface

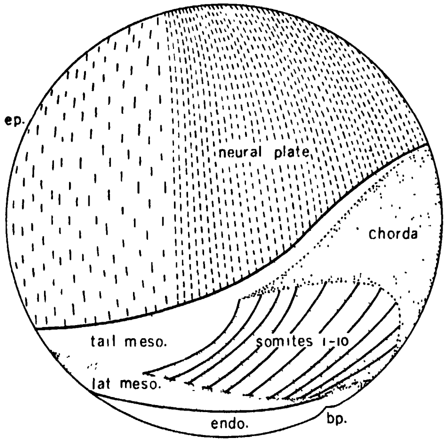


FIG. 3. SURFACE RELATIONS OF THE REGIONS OF THE YOUNG GASTRULA PREVIOUS TO THE FORMATION OF THE MEDULLARY PLATE
(After Vogt, 1926.)

relations to other regions previous to the formation of the neural plate; and Marx (1925) has demonstrated that when this center of organization, as the roof of the early digestive tract, comes to underlie and to press up against the surface layer of ectoderm (neural plate, fig. 3), it transforms the ectoderm into the rudiment of the central nervous system. Further, I have recently shown (Daniel, 1936) that this region may be followed in the living egg as it rolls in to form the roof of the digestive tract, and that certain details of its relation to, and contact with, the ectoderm as the ectoderm is tansformed into the medullary plate may be made out.

If we look at a transverse section through the region after the ectoderm has been changed into the medullary plate (*mp.*, fig. 4), we see that the plate lies in close contact with the original chordamesoderm (*ch.* and *so.*) which at this stage has already lifted up and so no longer forms the roof of the digestive tract (*dg.*). In such a section it is observed that where

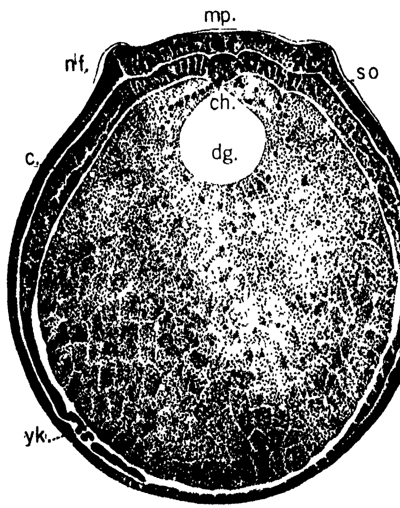


FIG. 4. TRANSVERSE SECTION OF NEURULA OF TRITURUS TOROSUS

Medullary plate region (*mp.*) which thins out over the notochord (*ch.*) as a median groove. Where the outer part of the somite (*so.*) touches the medullary plate the plate cells elongate and form the neural folds (*n.f.*).

the notochord (*ch.*) comes into contact with the medullary plate, the plate early thins out above as a median longitudinal groove, and protrudes ventrally, just above the notochord into a longitudinal neural keel (Baker, 1927). But equally as important is the observation that where the outer part of the somite (*so.*) touches the medullary plate, the plate cells elongate and rise up as neural folds (*n.f.*), roughing out the medullary plate into its definitive spatulate pattern (fig. 6a).

(*b*) *In an accessory embryo*

Just a little over a decade ago Spemann and Hilde Mangold (1924) performed the significant experiment which first demonstrated the influence of the center on the organization of an embryo. They showed that if a piece is taken from the organizing center in *Triton* and placed under indifferent ectoderm of another embryo, the ectoderm will be induced to form a nervous system; further, the piece will organize the subjacent host tissue, completing an accessory or secondary embryo (figs. 5a-b). Moreover, a piece put into the blastocoele and brought by gastrulation into contact with the overlying ectoderm will similarly induce a secondary embryo (fig. 5a at nt^2). Bautzmann (1926) showed that the organizing center includes the dorsal and lateral lips of the blastopore (chorda; somites; fig. 3), that is, the oblique crescent of Vogt. He further (1928) showed that a piece of the young notochord may act as an organizer. O. Mangold (1929) added that a piece of the medullary plate after being induced, or a piece of the brain of a tadpole, may also act as an organizer.

(*c*) *In a foreign position*

One of my students, Richard M. Eakin (1933), has shown that if the blastocoele, or segmentation cavity of the egg, be filled with a substance like gelatin which will solidify when cool, then the organizing center is unable to roll into the egg to form the roof of the digestive tract. Under this experimental condition this center (chorda; somites; fig. 3) is forced to occupy an unusual position. Fixed at its place of attachment to the entoderm, the organizing center folds down over the yolk area with one layer of the fold deeper and the other superficial. This migration may continue until most (or all) of the organizing area has rolled under as the deeper layer, so that what should be the roof of the digestive tract now extends downward across the yolk. The unique thing in this experiment is that the organizing center, even in this foreign environment, is still capable of inducing the over-

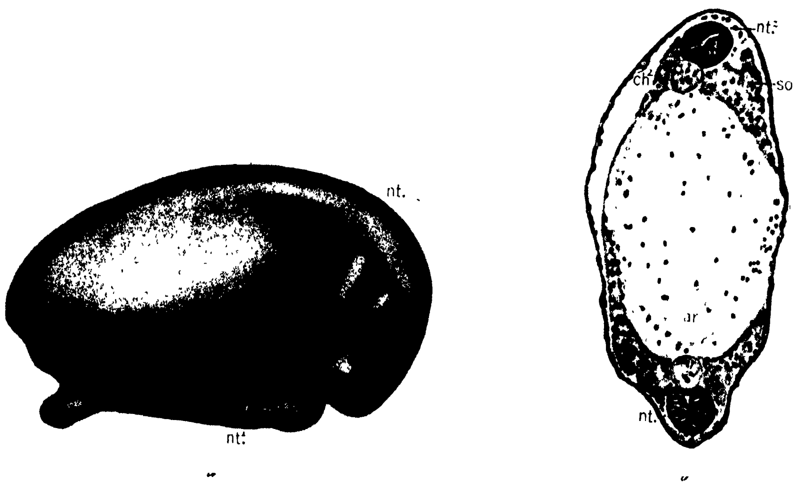


FIG. 5. A SECONDARY EMBRYO HAS BEEN INDUCED IN AN EMBRYO OF TRITURUS TOROSUS
Induced by a piece of organizing center under indifferent ectoderm; *nt.* and *nt.*², neural tubes of normal and induced embryos, respectively. *a.* External view from the right side. *b.* Cross section of the double embryo; *ar.*, archenteron; *ch.*², implanted chorda; *so.*², implanted somite. (After Schechtman, 1934.)

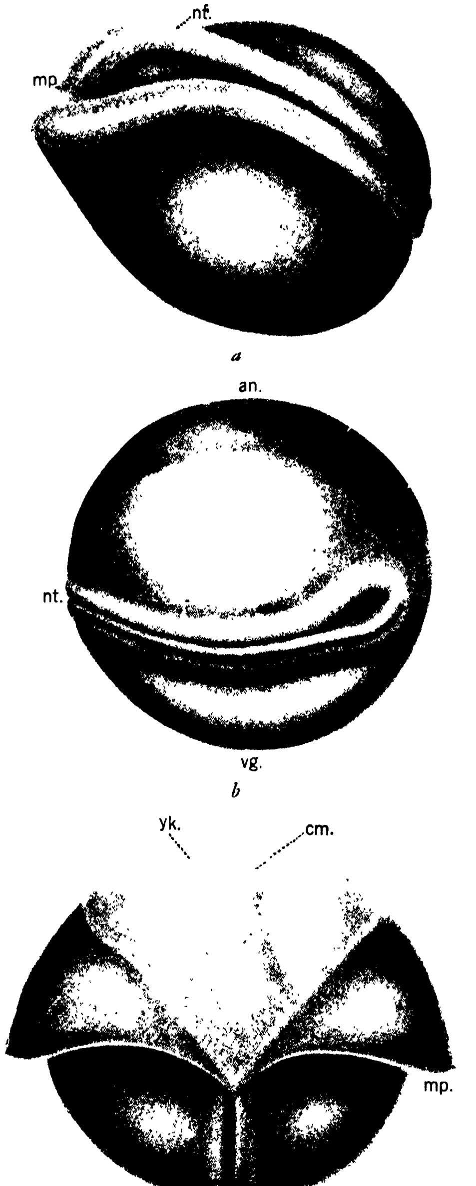


FIG. 6. INDUCTION OF MEDULLARY PLATE ON VENTRAL SURFACE OF EMBRYO

a. Normal neurula of *Triturus torosus*; *mp*., medullary plate; *nf*., neural fold, *b*. The blastocoele has been filled with gelatin, preventing the invagination of the organizing center, which then folds down over the yolk area and induces a medullary plate on the ventral surface. *c*. The medullary plate (*mp*.) has been cut and thrown back to show the underlying organizing center (*cm*.) lying against the yolk (*yk*.). (After Eakin, 1933.)

lying (underlying) layer to form medullary plate (fig. 6b), although this localizes the medullary plate on the belly instead of in its normal position on the back.

In figure 6c an incision has been made longitudinally through the neural tube, and the sides of the surface layer (*mp*.) have been thrown back so as to show the deeper layer as the organizing center of Spemann (*cm*.) lying against the yolk (*yk*.).

From the above experimental studies it is seen that the rôle of the organizing center is the same in these three experiments. Wherever this center comes into contact with ectoderm, there the simple ectoderm takes the outline of the medullary plate which later develops as the central nervous system.

But does this association of the center of organization and ectoderm demonstrate that the medullary plate owes its determination wholly to the center of organization?

Separation of the center of organization and the tissue to be induced

In recent experiments Holtfreter (1933a, b) has shown that ectoderm which should normally go to form medullary plate, if isolated from the organizing center, is not so transformed. By allowing the denuded eggs of Axolotl to develop in a weak Ringer's solution instead of in water, the yolk and chordamesoderm, instead of invaginating, may evaginate so as to produce of the egg a dumbbell-shaped figure (fig. 7a), one part of the dumbbell being produced by the evaginated yolk and chordamesoderm (*cm*.) and the other part by the ectoderm (*ec*.). In this experiment an almost complete separation of the ectoderm (*ec*.) from the organizing center (chordamesoderm [*cm*.] and entoderm, figs. 7b-c) is effected. Without the determining influence of the chordamesoderm, however, the ectoderm (*ec*., fig. 7c)

shows no signs of differentiating into medullary cells. This experiment indicates that the presumptive medullary material is dependent for its determination as a medullary plate upon contact with the organizing center.

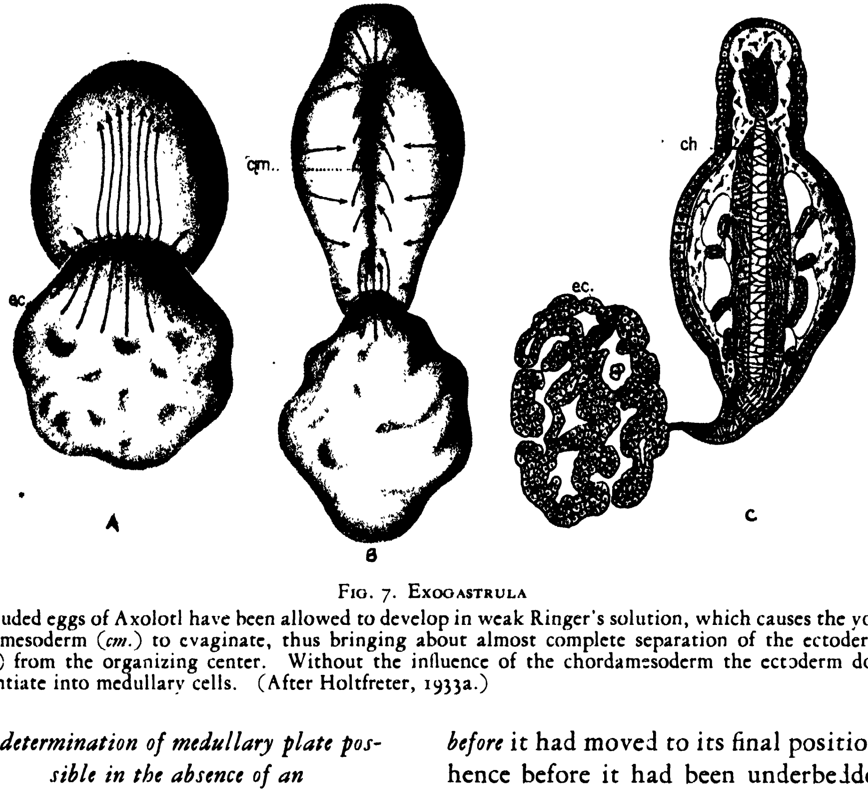


FIG. 7. EXOGASTRULA
Denuded eggs of Axolotl have been allowed to develop in weak Ringer's solution, which causes the yolk and chordamesoderm (*cm.*) to evaginate, thus bringing about almost complete separation of the ectoderm (*ec.*, fig. 7c) from the organizing center. Without the influence of the chordamesoderm the ectoderm does not differentiate into medullary cells. (After Holtfreter, 1933a.)

Is determination of medullary plate possible in the absence of an organizing center?

(a) Experimental separation of presumptive medullary material from the organizing center

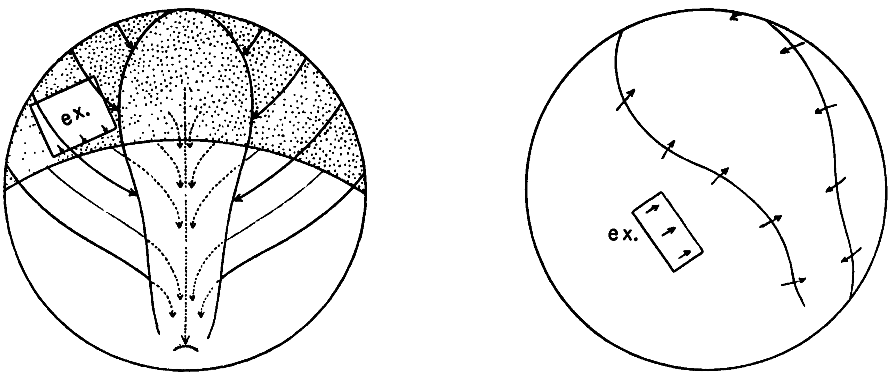
There have been data amassed indicating that the medullary plate may be partly determined before it comes into contact with the organizing center. If this be true, the organizer would act only in reinforcing induction. Goerttler (1927), by the vital staining method, has shown that the substance which goes to form the medullary plate is localized at the beginning of gastrulation as a crescent (stippled in fig. 8a) near the animal pole, and that it then moves into its definitive position to form the spatulate medullary plate (fig. 8b). Goerttler further made transplants of this presumptive medullary crescent *before* it had moved to its final position and hence before it had been underbedded by the roof of the digestive tract, in order to test whether at that time it was partly determined. A piece (*ex.*, fig. 8a) transplanted to an older neurula (fig. 8b), and so oriented as not to disturb its own directional movements (shown by arrows), resulted in a small accessory medullary plate (*mp.*[2], fig. 9). This experiment Goerttler held to demonstrate early (labile) determination of the medullary material independent of the center of organization. Holtfreter (1933c) has criticized this interpretation, however, maintaining that the pieces so transplanted were in

contact with mesoderm or with nerve tube, both of which are known to have organizing capacities.

(b) Isolation and cultivation of presumptive medullary material (*in vitro*)

If material presumed to be neural or medullary (see fig. 3) be determined or partly determined at this early period, its removal and cultivation outside the egg might be used to test this capacity.

Erdmann (1931), employing this method on *Amblystoma*, concluded that the presumptive medullary material grown outside the egg in a weak Ringer's solution developed as medullary material. Schechtman (1934), in our laboratory, has studied this problem at length on *Triturus torosus*. He has found that pieces removed from the presumptive medullary plate and cultured in Holtfreter's solution before they have been underbedded by the roof of the digestive tract may indeed possess elongate cells (fig. 10a) resembling medullary cells. A careful examination of these cells, however, in a more recent study (Schechtman, 1935) has brought forth several points of special interest. He has observed that in an explanted piece curling usually occurs, and that the curling tends to make some of the cells wedge-shaped. He has observed, further, that the explant also contracts, rendering the cells still more slender and elongate, and hence still more like medullary cells (fig. 10b). It is readily shown that both of these tendencies of the explant to curl up and to contract, mechanically influence the form of the cells; and that both are due to the operational procedure.



a b

FIG. 8. PURPORTED EARLY DETERMINATION OF MEDULLARY MATERIAL INDEPENDENT OF ORGANIZING CENTER

The stippled area in *a* shows the material which will form the medullary plate, localized as a crescent near the animal pole at the beginning of gastrulation. A piece of this crescent (*ex.*) was transplanted to an older neurula (*b*), where it formed a small accessory medullary plate (see fig. 9). (After Goerttler, 1927.)

If, however, these explants now be cultured for a short period in Holtfreter's solution, their cells become cuboidal or low columnar in shape, only occasional elongate cells remaining. If these cultured explants be compared with cultured explants which have been underbedded by chordamesoderm and hence certainly determined (fig. 10c), the difference between the two is striking. The true medullary cells occur as a compact mass, while there is paucity of elongate cells in the area presumed to be medullary.

Another indication that they are non-

medullary may be found in the fact that similar elongate cells also occur in explants of the presumptive epidermis which we know are not determined or even partly determined as medullary material.

We may answer our question in part by repeating that wherever an organizing center like the chordamesoderm comes into contact with ectoderm, there medullary structures are produced. In the absence of such contact, on the contrary, we must conclude that determination or even partial determination has not as yet been demonstrated.

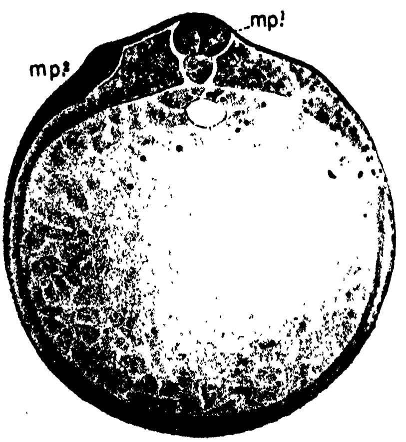


FIG. 9. ACCESSORY MEDULLARY PLATE ($mp.^2$) FORMED BY A TRANSPLANT OF PRESUMPTIVE MEDULLARY PLATE MATERIAL FROM AN EARLY GASTRULA TO AN OLDER NEURULA
(After Goerttler, 1927.)

Induction by killed material

Above we have considered the organizing center of Spemann as a source of the inductor which transforms ectodermal material into the medullary pattern, but we have also seen that pieces of young notochord, induced medullary plate, and young brain may also be used as inductors. A paper by Bautzmann, Holtfreter, Spemann, and Mangold (1932) has added the important findings that medullary structures may be induced by materials that have been killed by drying, boiling, or freezing, as well as by the living tissues. Indeed, pieces from gastrulae or neurulae, preserved in formalin for six months, are still effective medullary inductors; and pieces kept in 20 per cent hydrochloric acid for some time and then washed will still induce medullary structures from presumptive ectoderm. Pieces, however, which have been put

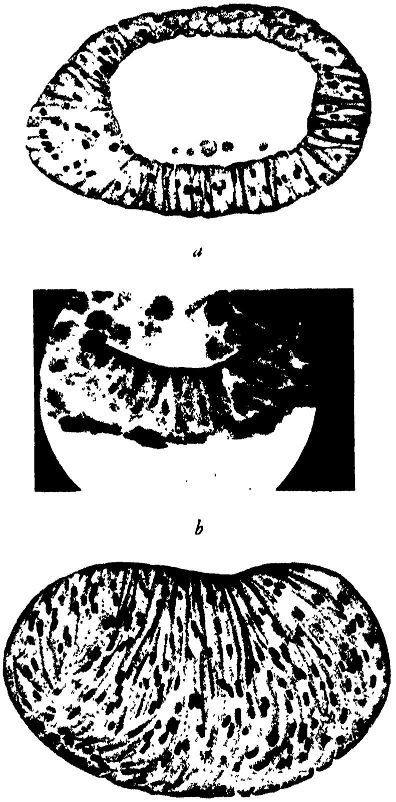


FIG. 10. PRESUMPTIVE MEDULLARY MATERIAL CULTIVATED IN VITRO

a. Section through an explant taken from presumptive medullary material of the early gastrula of *Triturus torosus*. *b.* Presumptive medullary material from a late blastula, fixed soon after extirpation. The explant has curled outward. Note the long, slender cells. *c.* Section through an explant taken from medullary material of the late gastrula of *Triturus*. (After Schechtman; a and c, 1934; b, 1935.)

into boiling paraffin or which have been kept an hour and a half in ether, lose wholly or in part their ability to induce (Holtfreter, 1933 d, e).

In these experiments it has also been shown that certain tissues which are impotent as inductors in the living state are potent if killed. Thus, if a piece of presumptive epidermis which in the living condition is unable to induce, be killed and placed between two layers of presumptive epidermis, then this dead presumptive epidermis will induce medullary structures from the layers of presumptive epidermis. Or if a small bit of presumptive epidermis be placed on a larger sheet of killed presumptive epidermis, it will develop into medullary structures. Moreover, entoderm or yolk material, if killed, may be similarly used as an inducing agent for presumptive epidermis (fig. 11). Or even pieces of the fertilized egg or zygote, if killed, may be used effectively as inductors.

Holtfreter (1933e) has further shown that the ability to induce is present in tissues of some invertebrates and all vertebrate classes. The tissues of the liver, heart, spleen, muscles, or ovarian eggs of fishes, when put under the presumptive skin of *Triton*, induce large neural structures; and pieces of the liver, kidney, testes of the common lizard, *lacerta agilis*, give brain-like structures. The liver, kidney, testes, and thyroid of birds are capable of inducing large medullary plates. Furthermore, an extract of chick embryos cultivated outside the egg and heated to form a coagulum produces neural structures above this coagulum as it would above a piece of an organizer. Also, other structures of a secondary embryo may be produced in *Triton*, as, for example, notochord, muscles, and kidney tubules. In a mammal like the mouse, the liver, kidney (fig. 12), heart, brain, orlens of the adult will produce from the material of the side of the embryo of the newt *Triton* an accessory embryo with large neural structures, a notochord, muscles, kidney, and connective tissue. Even pieces of the human liver, brain, thyroid, or certain muscles will induce neural structures, notochord, and muscles of an accessory embryo of *Triton* as would the organizing center of Spemann.

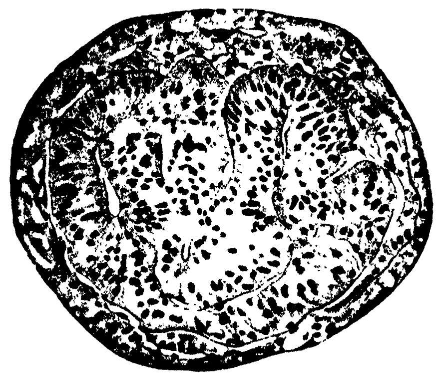


FIG. 11. MEDULLARY STRUCTURES INDUCED IN PRESUMPTIVE EPIDERMIS BY KILLED ENTODERM OR YOLK MATERIAL
(After Holtfreter, 1933d.)

Presence and nature of the medullary inductor

It would appear from a summary of all these studies that the stuff, whatever it be, which determines the medullary plate is either particularly abundant in the organizing center of Spemann, or is available to a great degree in this center. In certain living embryonic layers, as, for example, ectoderm or entoderm, it is not available. If these be killed, however, by any of the ways indicated above, then they too are capable of inducing medullary structures in presumptive epidermis. In these, either the inducing substance is not present in the living tissues and is a product of the killing process, or it is present but not available until the tissue is killed. It has further been demon-

strated that it (or something comparable to it) is present in different tissues of a great group of animals, and is capable of inducing not only medullary structures but also of organizing the host tissue of *Triton* into an accessory embryo; and that the accessory embryo resulting is practically as complete as is an accessory embryo produced by the organizer of Spemann.

Recent work has sought to ascertain the nature of this inductor. The fact that induction can be brought about by killed material indicates strongly that the inductor is chemical in nature. Fisher and Wehmeier (1933) have argued that it appears to be animal starch or glycogen. But Waddington and Needhams (1933) have shown that an extract from young chicks is inducing, although chicks at seven days have practically no glycogen present. Recently Barth (1934) has succeeded in producing neural plates in neurulae of Axolotl by means ofcephalin. Whether induction results from the cephalin itself or from impurities in the cephalin is yet to be ascertained.

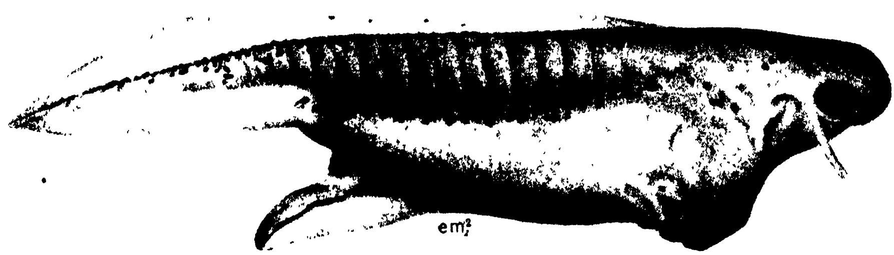


Fig. 12. Induction of an Accessory Embryo (*em.*²) on the Side of an Embryo of Triton by Tissue from the Kidney of the Mouse
(After Holtfreter, 1933e.)

B. Development of the neural tube

Its general regions and closure

The normal medullary plate early roughed out from the ectoderm becomes the brain and cord. That the general regions for the brain and cord owe their determination to the underlying chordamesoderm is shown by the fact that if three similar pieces of gastrular ectoderm be placed over the forward, middle, and last thirds of Holtfreter's exogastrula (fig. 7b) *before* the entoderm flows over the chordamesoderm, the anterior piece will produce brain together with other structures of nose, eye, and ear pertaining to this segment, and the middle and posterior pieces will form the longitudinal spinal cord.

As the medullary plate begins to sink in the midline along the median longitudinal groove and to fold up along its periphery by the elevation of neural folds, the neural tube as such begins to take form. The sinking in the midline extends forward above the median longitudinal groove and keel to just behind what will be the infundibular recess. The folds, as we have seen, are produced by the elongation of cells directly above the outer margins of the somites (*so.*, fig. 4). The more lateral of these cells which finally become the neural crests (Baker, *loc. cit.*; Landacre, 1921), rise rapidly and fuse first just behind the ear capsules (fig. 13 b). From this point they close slowly backward over the spinal cord,

and a bit more promptly forward over the brain area (*Triturus*).

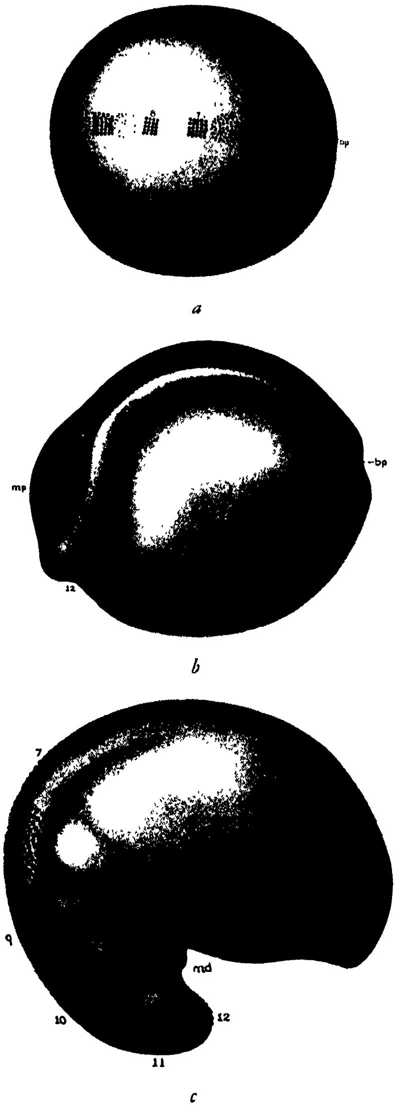


FIG. 13*a–c*. GASTRULA (A) OF TRITURUS TOROSUS
Gastrula stained with vital dyes (1–12) to show extension of areas; in neurula (*b*) stains 1–6 show neural folds elongating; in larva (*c*), *md.*, mandible; neural fold closed, hiding stains 1–6. (After Schechtman, 1932.)

During and following the closure of the tube Schechtman (1932) has shown that there is great stretching in a longitudinal direction in *Triturus torosus*. Vital dyes placed on or near the horizon of the gastrula (*1–12*, fig. 13a) shift their positions in the neurula, stains 7–12 bending with the flexure of the neural folds and stains 1–6 stretching longitudinally in the cord of the larva (fig. 13b). In figure 13c the stains (*1–6*) have been enclosed within the elongated spinal cord.

In addition to marking out the main regions of the neural tube, the roof of the digestive tract controls the details of the pattern, as has been shown in *H. regilla* by another of my students, A. L. Alderman (1935). As the anterior part of the brain, the prosencephalon, depends on its substratum for its pattern, so does the eye, a derivative of that part of the brain, depend on a specific substratum. Thus, if that part of the forebrain which should give rise to the eyes be underbedded by a part of the roof just back of the eye and transplanted to the ventral region, the piece of prosencephalon will now form brain and not eye.

Differentiation of motor and sensory neurones and the establishment of descending and ascending tracts

(a) Motor neurones

The differentiation of the cells within the neural tube of *Amblystoma* has been studied by Baker (1927) and by Coghill (1929). After the medullary plate has been determined, the cells just under and at the sides of the median longitudinal groove (figs. 4 and 14) form the neural keel. The keel extends farther ventrally and comes into so close physical relation with the notochord and mesoderm that these structures cannot be separated from it without injury (Coghill). Later the neuroepithelial cells in this region of the cord construct from their outer ends the external limiting membrane, separating the keel from the mass of mesoderm. The

cells in the keel itself now orient themselves radially with their outer, broader ends toward the mesoderm and notochord, and their narrower pigmented ends directed inwards. The pigmented ends of the cells on one side of the keel now pull away from their fellows on the opposite side, leaving a cleft which further deepens the median longitudinal groove.

Neuroepithelial cells in the keel now differentiate into the first localized center of primary motor cells (*m.c.*, fig. 14a). The manner in which this differentiation takes place, according to Coghill, is as follows: The protoplasm and nuclei of ventral neuroepithelial cells move toward the external limiting membrane. The central part of the body of the cell draws away from the membrane, leaving only the anterior and posterior extremities attached. The extremities of the cell next to the membrane now become the growing tips, thus giving a longitudinal neurone (*m.c.*, fig. 14a).

Both growing tips creep along the external limiting membrane but in opposite directions. One becomes the axone (*ax.* fig. 14c); the other, the dendrite (*d.*). The motor axones (*ax.*, fig. 15) pass ***down*** along the limiting membrane of the tube, forming the descending motor tract. The direction of their growth, according to Coghill, is in response to the gradient within the unsegmented mass of mesoderm (*m.*, fig. 16). Motor axones grow against this current and consequently down the external limiting membrane of the tube. Motor dendrites (*d.*, fig. 14c) go with the current and hence up the tube.

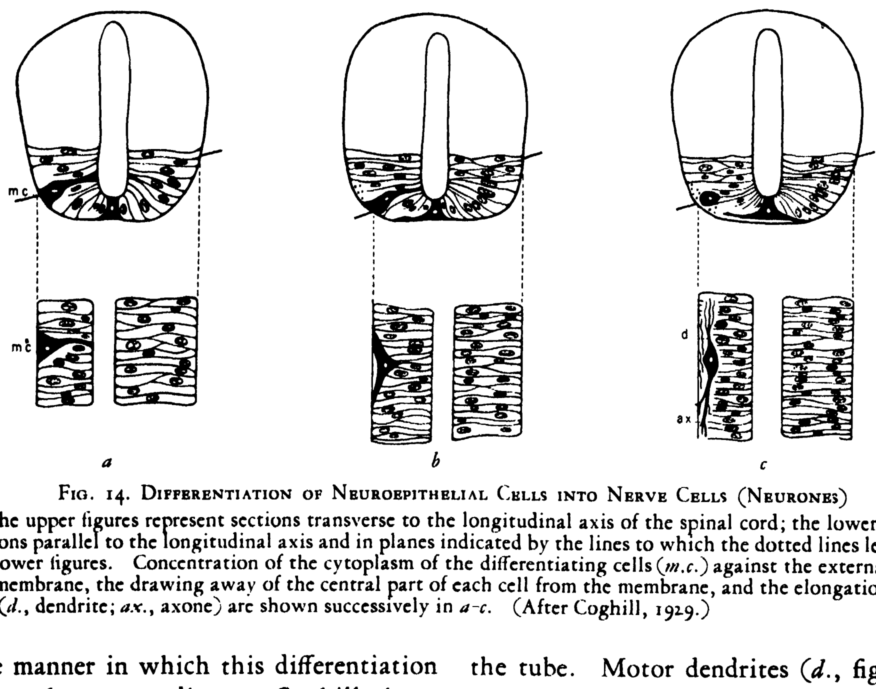


FIG. 14. DIFFERENTIATION OF NEUROEPITHELIAL CELLS INTO NERVE CELLS (NEURONES)

The upper figures represent sections transverse to the longitudinal axis of the spinal cord; the lower figures, sections parallel to the longitudinal axis and in planes indicated by the lines to which the dotted lines lead from the lower figures. Concentration of the cytoplasm of the differentiating cells (*m.c.*) against the external limiting membrane, the drawing away of the central part of each cell from the membrane, and the elongation of the tips (*d.*, dendrite; *ax.*, axone) are shown successively in *a–c*. (After Coghill, 1929.)

(b) Sensory neurones

Primary sensory cells are anatomically like primary motor cells in that they lie within the walls of the neural tube (spinal cord) and form longitudinal tracts, but the sensory cells arise from the neuroepithelial cells in the dorsolateral walls of the tube. A single growing tip of the cell faces the external limiting membrane

and then bifurcates, its two parts creeping in opposite directions along the membrane. The dendrite (*d.*[1], fig. 15) runs backward with the current and the axone proper (*ax.*[1]) runs forward against the ectodermal current, and with other similar axones forms the ascending tract.

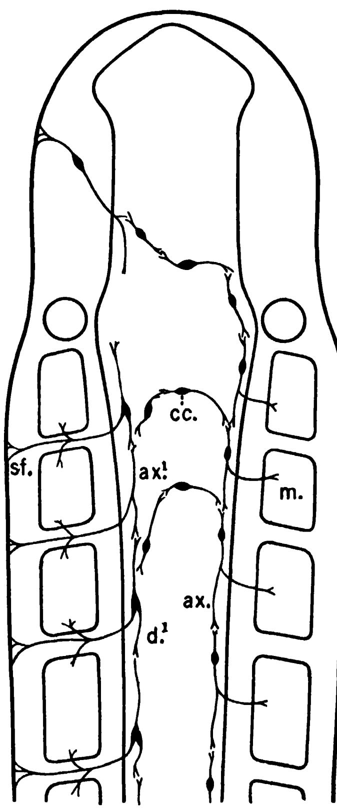


FIG. 15. DIAGRAM OF THE RELATION OF THE DEVELOPING NEURON OF AMBLYSTOMA TO THE MUSCLES AND SKIN (SEEN IN FRONTAL SECTION)

The descending motor axones (*ax.*) making up the motor tract are shown on the right; the ascending, sensory axones (ax[1]) making up the sensory path are on the left; and the commissural (floor-plate) cells (*cc.*) in the center. (After Coghill, 1929.)

Differentiation of commissural neurones and the union of ascending and descending tracts

Some of the neuroepithelial cells which lie in the floor plate directly under the neurocoele (*cc.*, figs. 16 and 15) form growing tips which creep laterally right or left toward the mesoderm and then turn posteriorly in right or left motor paths. A second growing tip then forms from such a cell and, as a dendrite, grows laterally in the direction opposite the originating axone. The dendrites of this commissural or floor-plate cell (*cc.*) are then put into communication with the sensory path of the opposite side by a second neurone (*n.*[2], fig. 16) so that sensation received from the ascending sensory tract of one side can now cross over to the descending motor path of the opposite side.

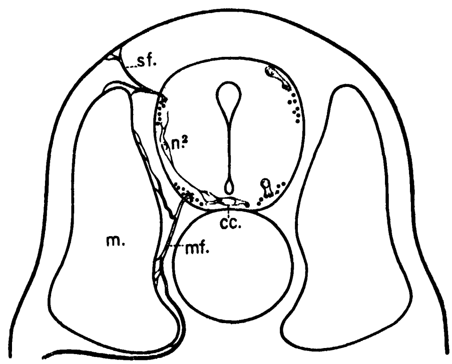


FIG. 16. DIAGRAM OF THE RELATION OF THE DEVELOPING NEURONES OF AMBLYSTOMA TO THE MUSCLES AND SKIN IN THE TRANSVERSE SECTION

Dorsally on the left side are the sensory cells, and ventrally the motor cells. The processes of both sensory and motor cells branch into ascending and descending fibres, forming a sensory or afferent tract on the one hand and a motor or efferent tract on the other. On the left side of the figure the branching of a sensory fibre (*sf.*) to innervate both skin and muscle is illustrated dorsally, and ventrally a motor fibre (*mf.*) that innervates the muscle segment is shown with a branch going on to the primodium of the forelimb. (After Coghill, 1929.)

Establishment of primitive peripheral nerves

When the longitudinal mass of mesoderm is divided into myotomes (*m.*, fig. 15), then motor fibers leave axones of the motor tract and pass laterally toward the

active mesoderm to attach at the middle of the myotome (*m.*, figs. 15 and 16), where the nuclei of the myoblasts are located. It is assumed by Coghill that these peripheral motor root branches of the descending axones grow toward the region of higher metabolic activity.

Similarly, side branches as dendrites (*sf.*, figs. 15 and 16) grow off the sides of the sensory cells to form the primary peripheral sensory system. These branches give twigs to the skin and to the ends of the myotomes.

With the establishment of the peripheral nervous system, sensation received by the skin or from the myotomes passes over the afferent (sensory) nerve into the sensory tract, where it may either go forward toward the beginning brain or across by the commissure formed by the plate cells to the motor tract of the opposite side. If it takes the latter course, then from the motor tract it passes as a motor impulse over the efferent nerve to the myotomes, which at this time are capable of contraction.

Acknowledgment: I am indebted to the Cambridge University Press and the Macmillan Company for permission to use Figures 14, 15, and 16 from Coghill's *Anatomy and the Problem of Behavior.*

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STUDIES IN ANIMAL POPULATIONS

II. SEASONAL POPULATION-TRENDS OF THE HONEY-BEE

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1. GENERAL OBSERVATIONS ON HONEY-BEE POPULATIONS

THE honey-bee (*Apis mellifica* L.) is a social bee with populous colonies, which under natural conditions dwell in caves of old trees, in clifts of rocks, etc. The nests persist for many years and a formation of new colonies takes place by partition of the original colony. At the seasonal population peak a large percentage of the colony may swarm out together with, or followed by, the old or young queen. The swarming bees aggregate in a cluster and enter a new suitable breeding locality. Fertilization occurs only once in the lifetime of the queen (longevity: 3–4 years, maximum observed 9 years), during a special nuptial flight and the sperms of this copulation last throughout the queen's life. The workers are genotypically females which by special feeding have been bred to workers, whereas the diet of queen larvae is especially rich in vitamin E. The males originate from unfertilized eggs. Honey and pollen are stored during the favourable season. During the unfavourable season the brood-rearing cycle is interrupted in temperate regions and reduced in warmer climates.

In modern bee-hives (of the Langstroth-type) there are 30,000–40,000 and up to 70,000 adult bees present at the population peak. Most of these individuals are workers. The quantity of drones is very different, assumingly based mainly on racial differences, and ranges from 50 to 5,000 per colony. During the bad season (e.g. hibernation) the colony may be reduced to 10,000 worker bees or less. In a good hive about 30,000 bees may leave the hive with the first swarm. Eventual after-swarms are much less populous. The fertility of the queen has often been greatly exaggerated. For good colonies a daily maximum oviposition rate (maintained only during some weeks) of 1000-2000 eggs can be expected. From 100,000–150,000 eggs total annual production is expected from a good queen under favourable conditions. A general discussion of all these data may be found in Bodenheimer (1936).

2. EBERT'S ANALYSIS OF THE SEASONAL POPULATION TREND IN THE HIVE OF THE HONEY-BEE

The number of the exact census of bee-populations is very small and all of them content themselves by considering only part of the total populations: either brood, sealed brood only, or adults. Exact counts for over a whole season are only those made by Nolan (1925) of the sealed brood. But the population is composed of all ages and stages. Counts of human populations taking into consideration only the ages above or below 30 years or even only those of 20–30 years may be of interest, but they do not permit an analysis of the trend of the total population.

The only previous attempt to study the dynamics of the total bee-population, known to the author, is that of Ebert (1922). He divides the bee-population

into three categories: brood, young bees (not yet leaving the hive) and field bees.

(1) The determination of the brood quantity present on every date is made with the aid of a planimeter or by calculating the surface as an ellipse, using the formula: sum of the products of the various heights and breadths multiplied by 1.6. An example is given in Table 1.

As 3.9 cells are present per cm². the product 1339 × 3.9 = 5223 represents the number of brood present.

(2) The determination of the field bees is made by weighing. The door of the hive is opened at a certain hour of normal flight activity in the morning and closed again when the increase of weight shows that the bees are returning. Ten bees are supposed to weigh 1 gram. The difference in the weight at both moments gives the number of field bees. When 570 grams difference are obtained, then 5700 field-bees are supposed to be present. This method is by no means exact, but should be respected as a first approach to the problem.

(3) The determination of the young bees is made during the absence of the field bees by taking out all combs and stripping off, with a carbolized rag, the young bees, which remain in the hive. The combs are weighed and as the weight of all other parts of the hive is known, the remaining difference yields the weight of the young bees. Their number is calculated as in (2): difference 273 grams = 2730 young bees.

These observations are made at intervals of 21 days. A second series of observations with 21 days (= duration of brood development) interval may be intercalated.

TABLE 1

	COMB I	COMB II	COMB III	SUM
Height........	15	18	21	
Breadth.......	11	14	20	
Product.......	165	252	420	837 × 1.6 = 1,339 cm².

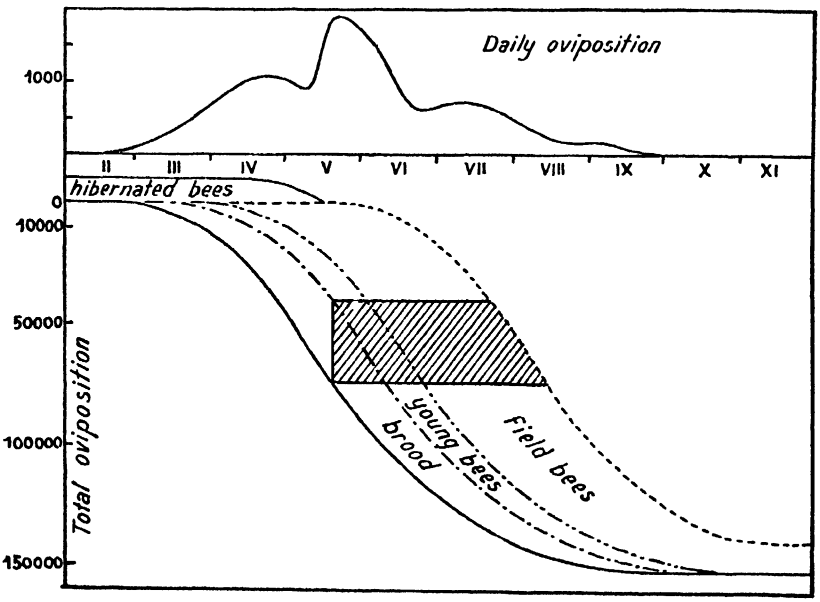

FIG. 1. THE SEASONAL TREND OF AN IDEAL BEE-POPULATION (AFTER EBERT)

This system is fairly exact for the total quantity of brood throughout the season. It yields both the total seasonal egg-production of the queen and, by additional calculation, the daily oviposition. An idealized scheme of the annual development is reproduced in Fig. 1 for a colony with 10,000 hibernated bees, 152,000 total egg-production from February to September and an average longevity of field bees of 7 weeks.

Many practical applications of this type of research are reported by Ebert, but his suggestions do not seem to have found listeners to this day.

3. A NEW METHOD OF CALCULATING THE SEASONAL DYNAMICS OF A HONEY-BEE-COLONY

Further research in the biology of bees, and especially the work of Nolan (1925), Roesch (1925) and Morland (1930) enable us now to use a more simple system in evaluating the population trend of a bee-

TABLE 2

	♀	♂
Development:	(days)	(days)
Egg-stage	3	3
Larval-stage	6	6
Sealed-brood	12	16
Adult life:		
Nurse-bee	10	100†
House-bee	10	
Field-bee	22*	

* (In winter longer.)
† (3–4 months.)

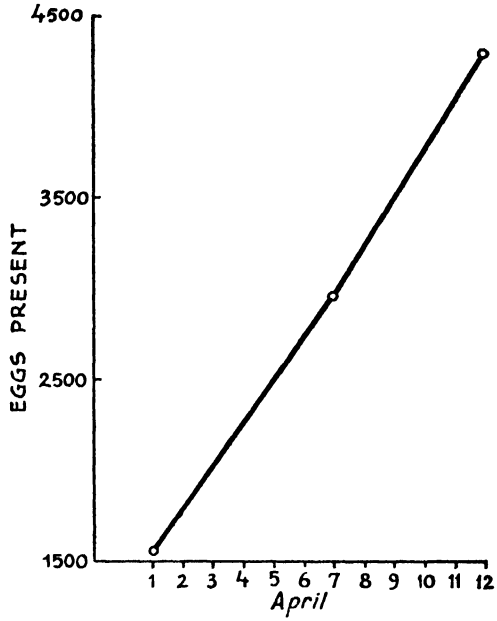

FIG. 2. SKETCH OF AUXILIARY CONSTRUCTION IN EVALUATING THE DAILY QUANTITY OF BROOD (SEALED-BROOD) PRESENT ON EVERY DAY OF THE INTERVAL BETWEEN THE ACTUAL COUNTS (POINTS)

TABLE 3

1	2	3	4	5	6	*7*	8	9	10	11	*12*	APRIL
1,553	1,810	2,060	2,300	2,550	2,790	*2,986*	3,280	3,520	3,770	4,010	*4,297*	No. of sealed brood

colony with probably a much greater accuracy.

Premises: (1) The calculation is based on the consecutive division of labour (Roesch, Morland), which on the average results in the scheme shown in Table 2.

The stages of adult life are to a certain degree modifiable and the house-bees especially may serve somewhat longer as nurses, or be speeded to serve as field-bees. They are therefore called "control-bees" by some American authors. (2) Any successfully developing egg does not show under normal conditions a further mortality (cf. Bodenheimer, 1936). (3) The fertility of the queen does not differ considerably from one day to another, but changes only gradually.

These points will be discussed later on.

Procedure: (1) The given data on the quantity of sealed brood present are plotted in a system of coördinates, where the abscissa illustrates the time, the ordinate the number of sealed brood present. The dots of the observed data are joined by a line and the number of sealed brood present at any day of the interval between two dots can be read on the ordinate (cf. Fig. 2). The values for the intervals between the two observations of 1st and 7th and 12th of April are seen in Table 3.

(2) The second step is the evaluation of the daily addition for the stage concerned (sealed brood in our case). Four columns are erected for this purpose. Column I contains the quantity of sealed

TABLE 4

DATE	I SEALED BROOD PRESENT EVERY DAY	II DAILY DIFFERENCE AGAINST YESTERDAY	III DAILY CHANGE INTO NURSE-BEES	IV ACTUAL DAILY INCREASE OF SEALED BROOD
Feb.				
23	0	0	—	0
24	5	5	—	5
25	12	7	—	7
26	18	6	—	6
27	23	5	—	5
28	29	6	—	6
29	34	5	—	5
Mar.				
1	*41*	7	—	7
2	100	59	—	59
3	140	40	—	40
4	180	40	—	40
5	220	40	—	40
6	275	55	0	55
7	320	45	5	50
8	360	40	7	47
9	400	40	6	46
10	450	50	5	55
11	490	40	6	46
12	520	30	5	35
13	580	60	7	67
14	620	40	59	99
15	*660*	40	40	80
16	590	−70	40	−30
17	520	−70	40	−30
18	440	−80	55	−25
19	*368*	−72	50	−22
20	410	42	47	89
21	450	40	46	86
22	500	50	55	105
23	530	30	46	76
24	585	55	35	90
25	*608*	23	67	90
26	850	242	99	341
27	880	30	80	110
28	1,020	140	−30	110
29	1,160	140	−30	110
30	1,300	130	−25	105
31	*1,533*	123	87	210
Apr.				
1	1,810	257	86	343
2	2,060	250	105	355
3	2,300	240	76	316
4	2,550	250	90	340
5	2,790	240	90	330
6	*2,986*	196	341	537
7	3,280	294	110	404

TABLE 4—*Concluded*

DATE	I SEALED BROOD PRESENT EVERY DAY	II DAILY DIFFERENCE AGAINST YESTERDAY	III DAILY CHANGE INTO NURSE-BEES	IV ACTUAL DAILY INCREASE OF SEALED BROOD
Apr.				
8	3,520	240	110	350
9	3,770	250	110	360
10	4,010	240	105	345
11	*4,297*	287	118	406
12	4,460	163	210	373
13	4,630	170	343	512
14	4,810	180	355	535
.	.	.	.	.
.	.	.	.	.

brood present every day, Column II, the daily difference of Column I against yesterday. However, this difference does not represent the daily addition of sealed brood, because after every 12th day the sealed brood disappears daily, becoming nurse-bees. This disappearing sealed brood is represented in Column III, and Column IV represents the real daily addition of new sealed brood after the values in Columns II and III have been added. Columns III and IV are identical, only Column III appears 12 days later than Column IV. The 12 first values are entered in Column IV and Column IV is calculated as far as possible. The 12 last values of Column IV are then entered as a continuation of Column III, etc.

The calculation of the Cyprian colony (Nolan, 1926) may serve as an example: The actually published data on the quantity of sealed brood at various dates are reproduced in Nolan's Chapter 4. Table 4 illustrates the procedure hitherto described for getting the first steps of the calculation.

(3) In this way, we received the real daily addition of sealed brood. When every instar has a definite duration, this daily increase must be identical for all stages. We have to defer the dates of

TABLE 5

DATE	I DAILY INCREASE OF EGGS	II DAILY INCREASE OF LARVAE	III DAILY INCREASE OF SEALED BROOD	IV DAILY INCREASE OF NURSE-BEES	V DAILY INCREASE OF HOUSE-BEES	VI DAILY INCREASE OF FIELD-BEES
Feb.						
14	0	—	—	—	—	—
15	5	—	—	—	—	—
16	7	—	—	—	—	—
17	6	0	—	—	—	—
18	5	5	—	—	—	—
19	6	7	—	—	—	—
20	5	6	—	—	—	—
21	7	5	—	—	—	—
22	59	6	—	—	—	—
23	40	5	0	—	—	—
24	40	7	5	—	—	—
25	40	59	7	—	—	—
26	55	40	6	—	—	—
27	50	40	5	—	—	—
28	47	40	6	—	—	—
29	46	55	5	—	—	—
Mar.						
1	55	50	7	—	—	—
2	46	47	59	—	—	—
3	35	46	40	—	—	—
4	67	55	40	—	—	—
5	99	46	40	—	—	—
6	80	35	55	0	—	—
7	−90	67	50	5	—	—
8	−30	99	47	7	—	—
9	−25	80	46	6	—	—
10	−22	−30	55	5	—	—
11	87	−30	46	6	—	—
12	86	−25	35	5	—	—
13	105	−22	67	7	—	—
14	76	87	99	59	—	—
15	90	86	80	40	—	—
16	90	105	−30	40	0	—
17	341	76	−30	40	5	—
18	110	90	−25	55	7	—
19	110	90	−22	50	6	—
20	110	341	87	47	5	—
21	105	110	86	46	6	—
22	118	110	105	55	5	—
23	210	110	76	46	7	—

TABLE 5—*Concluded*

DATE	I DAILY INCREASE OF EGGS	II DAILY INCREASE OF LARVAE	III DAILY INCREASE OF SEALED BROOD	IV DAILY INCREASE OF NURSE-BEES	V DAILY INCREASE OF HOUSE-BEES	VI DAILY INCREASE OF FIELD-BEES
Mar.						
24	343	105	90	35	59	—
25	355	118	90	67	40	—
26	316	210	341	99	40	0
27	340	343	110	80	40	5
28	330	355	110	−30	55	7
29	537	316	110	−30	50	6
30	404	340	105	−25	47	5
31	350	330	118	−22	46	6
Apr.						
1	360	537	210	87	55	5
2	345	404	343	86	46	7
3	405	350	355	105	35	59
4	373	360	316	76	67	40
5	513	345	340	90	99	40
6	535	405	330	90	80	40
7	506	373	537	341	−30	55
8	530	513	404	110	−30	50
9	530	535	350	110	−25	47
10	740	506	360	110	−22	46
	.	.	.	.	.	.
	.	.	.	.	.	.

sealed brood additions for 12, 20 or 30 days in order to receive the daily addition of nurse-, house-, and field-bees respectively. In postponing the additions for another 22 days, we evaluate the normal longevity of the field-bees. This system of book-keeping is shown in Table 5.

(4) Now everything is prepared for the final calculation. For most purposes it will be quite sufficient to know the number of all brood-instars present in a hive in weekly intervals. With respective changes the same calculation may be applied for smaller or longer intervals.

The last day before the beginning of the seasonal oviposition is marked as starting point. Every seventh day of the last table

TABLE 6.—*Weekly status of a bee-population (Cyprus-race, Nolan 1926)*

DATE	I EGGS 3	II LARVAE 6	III SEALED BROOD 12	IV NURSE-BEES 10	V HOUSE-BEES 10	VI FIELD-BEES 22	VII DRONE EGGS 3	VIII DRONE LARVAE 6	IX DRONE SEALED-BROOD 16	X DRONE ADULTS 100	XI TOTAL LIVING
Feb.											
14	0	0	0	0	0	0	0	0	0	0	0
21	18	23	0	0	0	0	0	0	0	0	41
28	152	191	29	0	0	0	0	0	0	0	372
Mar.											
6	246	279	275	0	0	0	0	0	0	0	800
13	278	82	580	41	0	0	0	0	0	0	981
20	330	788	408	349	23	0	0	0	0	0	1,898
27	1,011	996	868	580	215	5	0	0	0	0	3,675
Apr.											
3	1,111	2,977	2,058	417	454	100	0	0	0	0	7,116
10	1,800	2,677	3,768	1,205	285	418	3	6	0	0	10,162
17	2,031	3,246	5,188	2,117	808	770	20	14	7	0	14,201
24	3,448	5,021	6,249	3,742	1,697	1,046	158	46	23	0	21,360
May											
1	3,899	6,619	8,818	4,837	3,303	1,670	111	322	122	5	29,706
8	3,909	8,062	12,788	5,807	4,328	3,264	90	202	470	20	38,930
15	3,053	6,515	15,298	9,013	5,330	5,857	471	591	654	64	46,840
22	3,308	6,550	14,357	11,774	7,030	8,865	32	728	1,060	388	54,092
29	3,811	6,340	12,494	13,229	10,615	11,089	362	126	1,475	681	60,222
June											
5	3,389	7,414	13,187	10,998	12,788	15,642	—	255	1,378	1,168	60,219
12	3,527	7,179	13,937	10,749	12,266	20,382	438	—	342	2,099	70,919
19	3,076	7,155	16,517	11,168	10,121	25,061	—	483	450	2,297	76,328
26	3,804	7,424	14,197	11,837	10,507	24,170	244	82	362	2,865	75,492
July											
3	3,345	6,930	13,327	11,738	11,827	23,359	—	546	747	2,575	74,394
10	2,923	6,927	13,817	11,302	11,940	22,027	123	—	412	3,308	72,779
17	2,712	4,808	13,056	12,137	11,996	23,098	—	515	476	3,235	72,032
24	2,061	5,092	11,216	11,440	12,082	23,799	122	—	531	3,764	70,107
31	2,604	4,322	8,916	10,525	11,980	24,116	37	342	484	3,700	68,026
Aug.											
7	2,014	5,120	9,416	8,493	10,891	24,373	350	—	557	4,155	65,369
14	2,656	4,039	9,596	8,121	9,282	23,594	—	349	502	4,229	62,368
21	3,031	4,656	9,766	7,724	8,164	23,107	235	389	470	4,420	61,962
28	2,884	6,317	10,154	8,082	7,641	20,529	298	125	859	4,203	61,152
Sept.											
4	1,456	4,434	11,061	8,164	8,689	18,823	—	575	467	4,470	68,139
11	2,071	3,214	9,916	8,581	7,872	17,768	270	204	831	4,382	55,309
18	2,446	4,684	6,418	10,299	8,086	17,584	141	12	890	3,639	54,199
25	3,067	4,893	8,016	6,642	9,371	18,275	—	526	327	3,847	54,964
Oct.											
2	210	1,417	9,646	5,631	8,442	18,724	258	—	860		45,191
9	—	800	8,516	7,686	5,247	19,984	—	258	675		43,166
16		70	2,962	7,632	6,800	17,521		—	356		35,341

TABLE 6.—Concluded

DATE	I EGGS 3	II LARVAE 6	III SEALED BROOD 12	IV NURSE-BEES 10	V HOUSE-BEES 10	VI FIELD-BEES 22	VII DRONE EGGS 3	VIII DRONE LARVAE 6	IX DRONE SEALED-BROOD 16	X DRONE ADULTS 100	XI TOTAL LIVING
Oct.											
23*		—	760	5,277	7,766	16,125			—		29,928
30			—	2,069	7,724	14,038					23,831
Nov.											
6				—	1,389	16,825					
13					632	13,279					
20					—						

* From October 23, all stages are regarded as stable. The total number of 29,928 bees is greatly reduced at the beginning of winter and presumably to a lesser degree during the winter. From 10,000 tp 15,000 adult bees normally survive until the coming brood-rearing cycle. These survivors die at a rapid rate during the first weeks of brood-rearing.

is underlined and the following additions made: for evaluating the number of eggs present at any of these dates the daily additions to the egg-stage for the last 3

As in the present case, the sealed brood for workers and drones had been registered separately, we have to perform the calculation separately too. The relative

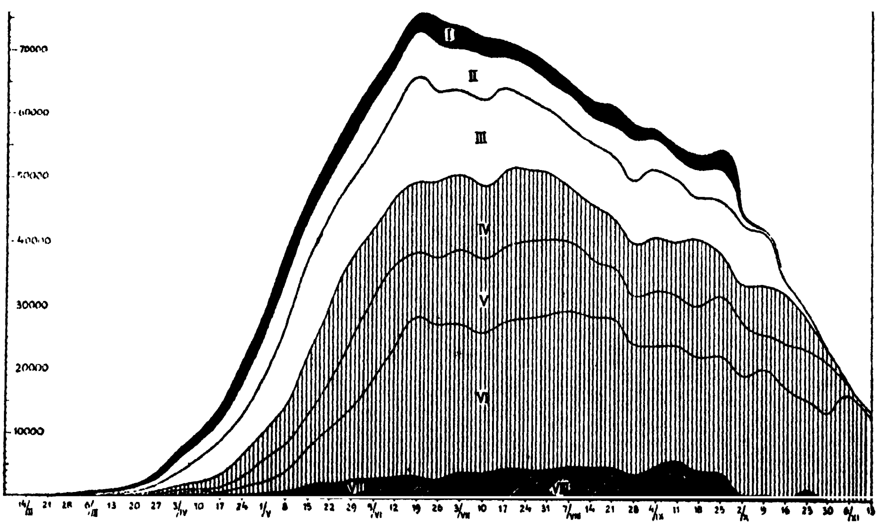

FIG. 3. THE SEASONAL POPULATION TREND OF A COLONY OF CYPRIAN BEES (1926)
(Calculated after Nolan's observations on sealed brood)

I–VI, workers; VII/VIII, drones. I, egg; II, larvae; III, nymphs; IV, nurse-bees; V, house-bees; VI, field-bees. VII, drone brood; VIII, adult drones.

days are summed up (duration of egg stage, 3 days). The respective additions for the other instars are: larvae six daily increases, sealed brood 12 days, nurse-bees 10 days, house-bees 10 days and field-bees 22 days.

data for drones are: egg 3 days, larva 6 days, sealed brood 16 days, adult drones 100 days (3–4 months, until the drone-battle). In this way the final table (Table 6) is obtained.

In our own observations we contented

ourselves with the total number of brood (all instars) present at a 21 day interval. Following the procedure of Ebert, improved by Polteff, we measured the maximal height and breadth of the brood area of both sides of every comb in units of 16 mm. It is then possible to read from a table prepared by Polteff the number of cells covered by the brood area without further calculation.

of empty cells present in the brood area. In two cases for example the readings were:

Units read	*Polteff's table*	*Actual brood*	*Empty cells*	*Total*
8 x 15	900	944	81	1,025
9 x 18	1,300	1,365	44	1,409

Also negative deviations were observed. It may therefore be assumed that the quick

TABLE 7

Polteff's table

(The units of length and height are 16 mm. each, those of the combination hundreds [3 = 300].)

LENGTH	HEIGHT																			
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
1	—	—	—	—	—	—	—	1	1	1	1	1	1	1	1	1	1	1	1	2
2	—	—	—	1	1	1	1	1	1	2	2	2	2	2	2	2	3	3	3	3
3	—	—	1	1	1	1	2	2	2	2	3	3	3	3	3	4	4	4	4	5
4	—	1	1	1	2	2	2	2	3	3	3	4	4	4	5	5	5	6	6	6
5	—	1	1	2	2	2	3	3	3	4	4	5	5	5	6	6	7	7	7	8
6	—	1	1	2	2	3	3	4	4	5	5	6	6	7	7	7	8	8	9	9
7	—	1	2	2	2	3	4	4	5	5	6	7	7	8	8	9	9	10	10	11
8	1	1	2	2	3	4	4	5	6	7	7	7	8	9	9	10	11	11	12	13
9	1	1	2	3	3	4	5	5	6	7	8	8	9	10	11	11	12	13	13	14
10	1	2	2	3	4	5	5	6	7	8	9	9	10	11	12	13	13	14	15	16
11	1	2	3	3	4	5	6	7	8	9	9	10	11	12	13	14	15	15	16	17
12	1	2	3	4	5	6	7	7	8	9	10	11	12	13	14	15	16	17	18	19
13	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
14	1	2	3	4	5	7	8	9	10	11	12	13	14	15	16	18	19	20	21	22
15	1	2	3	5	6	7	8	9	11	12	13	14	15	16	18	19	20	21	22	24
16	1	2	4	5	6	7	9	10	11	13	14	15	16	18	19	20	21	23	24	25
17	1	3	4	5	7	8	9	11	12	13	15	16	17	19	20	21	23	24	25	17
18	1	3	4	7	7	8	10	11	13	14	15	17	18	20	21	23	24	25	27	28
19	1	3	4	6	7	9	10	12	13	15	16	18	19	21	22	24	25	27	28	30
20	2	3	5	6	8	9	11	13	14	16	17	19	20	22	24	25	27	28	30	31
21	2	3	5	7	8	10	11	13	15	16	18	20	21	23	25	26	28	30	31	33
22	2	3	5	7	9	10	12	14	15	17	19	21	22	24	26	28	29	31	33	35
23	2	4	5	7	9	11	12	14	16	18	20	22	23	25	27	29	31	32	34	36
24	2	4	6	7	9	11	13	15	17	19	21	23	24	26	28	30	32	34	36	38
25	2	4	6	8	10	12	14	16	18	20	22	24	25	27	29	31	33	35	37	39

While in Polteff's hive 800 cells were present in 200 cm.^{2}, in some counts in Palestine there were 806, 812, and 836. We would thus obtain a number of cells of the brood area, which is somewhat below their actual number. However, this deviation is balanced by the number

estimate gained by this method leads to figures approaching ± 5 per cent of the actually present brood. This approximation is sufficiently significant to justify the application of this quick and simple method.

Polteff's table, having appeared in a not

easily accessible journal, is given here for the benefit of other workers (Table 7).

TABLE 8

Nolan's records of sealed brood in some bee colonies throughout the year

(The dates are the days of inspection, the figures the number of sealed brood present in the hive on the day of inspection.)

ITALIAN RACE		CYPRIAN RACE		
Colony 4 (1921)	Sealed brood	(1926)	Workers	Drones
Mar.		Mar.		
16	5,012	1	41	—
23	9,024	15	660	—
29	11,186	19	368	—
Apr.		25	608	—
5	12,327	Apr.		
12	14,787	1	1,553	—
19	15,402	7	2,986	—
26	14,072	12	4,297	—
May		19	5,593	9
3	15,028	24	6,249	23
10	16,982	30	8,283	66
17	17,859	May		
24	17,155	7	12,240	431
31	10,955	12	14,710	625
June		18	15,737	661
7	13,079	24	13,499	1,270
14	12,408	29	12,387	1,575
21	13,674	June		
28	14,588	8	13,426	1,180
July		14	13,949	215
5	13,659	21	14,127	370
12	11,741	28	13,925	343
19	11,084	July		
26	9,904	8	14,375	33
Aug.		16	13,186	24
2	8,204	26	10,548	18
9	7,840	Aug.		
16	7,459	2	9,472	17
23	7,871	9	9,133	10
30	8,896	15	9,430	22
Sept.		28	9,931	16
6	9,448	Sept.		
13	8,015	7	11,246	229
20	7,862	18	5,702	17
27	9,433	Oct.		
Oct.		6	9,869	5
4	7,644	15	3,648	—
11	3,973			
18	1,143			

4. THE SEASONAL HISTORY OF NOLAN'S HIVES AT BALTIMORE

The data calculated here (Table 8) are based on Nolan's counts of sealed brood in a large apiary near Baltimore. These counts were made at (not less than) 12 day intervals on colonies of different races and quality.

We shall describe briefly the conditions of development in Nolan's model colony No. 4, 1921 (Table 9), which proved to be identical in two successive years. This colony stored the most surplus honey. It was left in the preceeding winter without packing, in two 10-frame Langstroth hive bodies with abundant stores of honey. In the spring, this colony started with a fairly strong force of hibernated bees, a prolific queen, combs composed chiefly of worker cells and no shortage of honey stores.

1. Initial expansion. The chief source of nectar, tulip tree, was yielding freely by May 21. The maximum of sealed brood was reached during the third week in May. Nectar and pollen came in abundantly throughout March, but the secretion of nectar and the production of pollen were affected adversely by the inclement weather at the end of March and beginning of April, and probably the lower temperatures also affected unfavorably the activity of the unpacked colony. The adverse conditions finally checked the initial seasonal expansion of the brood, as is shown by a decrease in the amount of sealed brood in the latter half of April. A recovery in the rate of brood rearing was made subsequently, but at a time when factors associated normally with the major period were making themselves felt. It follows that the maximum amount of brood rearing in 1921 was nor purely the result of the initial seasonal tendency.

2. Major period. Since the maximum had been attained just before the locust bloom, the high rate was kept up for a couple of weeks. During the week of maximum sealed brood, brood rearing was undoubtedly still being carried on under the impulse of the initial tendency, but influencing factors characteristic of the major period were also becoming evident. The week of the maximum marked the point of division between the initial seasonal tendency and the major period. Nectar subsequently

TABLE 9

Italian bee-colony 4 (1921)

DATE	I EGGS (3)	II LARVAE (6)	III SEALED BROOD (12)	IV NURSE-BEES (10)	V HOUSE-BEES (10)	VI FIELD-BEES (22)	VII TOTAL
Feb.							
11	—	—	—	—	—	—	—
18	100	40	—	—	—	—	140
25	400	405	70	—	—	—	875
Mar.							
4	1,720	1,320	600	—	—	—	3,640
11	2,245	3,970	2,230	140	—	—	8,585
18	2,810	5,420	6,075	840	40	—	15,185
25	2,595	5,980	9,195	3,160	475	5	21,410
Apr.							
1	4,291	6,625	11,195	6,695	1,720	200	30,726
8	3,482	8,510	12,925	8,315	5,175	1,195	39,602
15	4,645	7,512	14,565	9,535	7,590	4,285	48,132
22	4,518	6,038	14,305	11,811	9,620	8,255	54,543
29	3,627	8,578	13,925	11,997	10,758	14,097	62,982
May							
6	4,420	8,358	16,365	12,072	12,336	18,814	71,365
13	3,721	7,370	16,885	12,100	11,722	22,150	73,948
20	2,024	4,787	17,175	13,403	11,154	24,130	72,673
27	5,515	8,313	14,235	14,220	13,060	26,475	81,818
June							
3	544	2,433	11,475	13,776	14,035	25,984	68,227
10	4,152	9,522	12,425	10,244	15,015	27,373	78,731
17	2,896	2,924	12,555	10,690	11,779	30,129	70,973
24	1,165	10,202	12,501	9,520	8,238	30,279	71,905
July							
1	4,838	2,122	11,481	11,098	11,774	26,835	68,148
8	852	6,332	11,204	8,882	9,035	26,627	62,932
15	3,787	3,743	9,504	11,480	11,356	20,936	60,806
22	1	2,465	8,695	6,692	11,768	24,406	54,026
29	599	6,697	7,310	8,615	7,800	21,362	52,383
Aug.							
5	2,530	−613	6,160	7,982	8,744	24,176	48,979
12	221	5,698	5,780	4,040	5,450	21,111	42,300
19	4,686	1,207	5,645	5,824	7,645	17,342	41,349
26	318	6,107	7,615	2,600	5,446	16,172	37,622
Sept.							
2	1,972	4,738	7,129	6,405	4,387	11,184	35,815
9	1,216	644	7,583	5,824	5,424	13,404	34,095
16	619	6,722	5,928	5,786	3,361	9,767	32,183
23	0	1,054	6,528	6,033	6,952	11,627	32,194
30	0	0	7,985	3,179	4,456	12,342	29,978
Oct.							
7	0			6,677	5,617	11,362	29,631

From October 7 on, all adult bees remain in their respective stages of development. A strong initial and a less strong later hibernal mortality reduces them to 10,000–15,000 adults at the beginning of the following breeding season.

coming in from the tulip-tree tended to restrict the queen, and after this honey flow there was a dearth of nectar until the middle of September. During June, however, there was an appreciable amount of incoming pollen, and in August there was an intense pollen yield. As a consequence the decline in brood-rearing activity which set in with the beginning of the tulip-tree honey flow and extended until the intense pollen yield in August was broken by a marked increase in June, in response to the pollen yield. The sharp decline which proceeded it happened because just prior to the honey flow the queen had ascended to the third hive body. Incoming nectar, however, so quickly cut down the number of cells available for the queen as to force her soon to return to the second hive body. Here, too, so many available cells within the brood area proper had been filled with nectar during her absence that the total number of cells made empty either by emerging bees or by consumption of stores did not suffice during that week to permit keeping up her former rate. During the next two weeks, with the emergence of many bees in both the second and third hive bodies, the queen had more room. There was also an exceptionally large quantity of honeydew available, associated with incoming pollen during June, also a certain amount of nectar from sweet clover. Throughout this month the queen had all the room needed for a normal response to these stimuli. At the end of July brood-rearing activity had been reduced to approximately one-half that represented by the maximum.

3. Final contraction. The final contraction in brood-rearing activity took place almost entirely in October, covering three weeks. The abruptness of the contraction in these few weeks is shown from the fact that in the last week of September there were practically half as many cells of sealed brood as were found in the maximum counts for the year. As a result, the colony entered the following season strong in bees.

4. Year 1922. The seasonal brood-rearing curve is strikingly similar to that in 1921, taking into consideration certain minor differences due primarily to weather conditions.

5. Conclusions. The brood-rearing record of this colony, although not ideal, is the most satisfactory of any of the 16 (studied) colonies because the maximum brood rearing bears some correlation to the initial expansion. The portion of the major period immediately following the period of main nectar secretion is not marked by a disproportionate degree of brood-rearing activity. In the late stages of the major period, moreover, there is an increase in brood rearing, providing a sufficient number of newly emerged bees at the beginning of the final contraction to insure successful wintering and an auspicious beginning of the next active season. That conditions within the colony remained nearly constant during the two consecutive years is indicated by the striking similarity in the curves of brood-rearing activity during both active seasons (Fig. 4). The performance of this colony leads to the conclusion that,

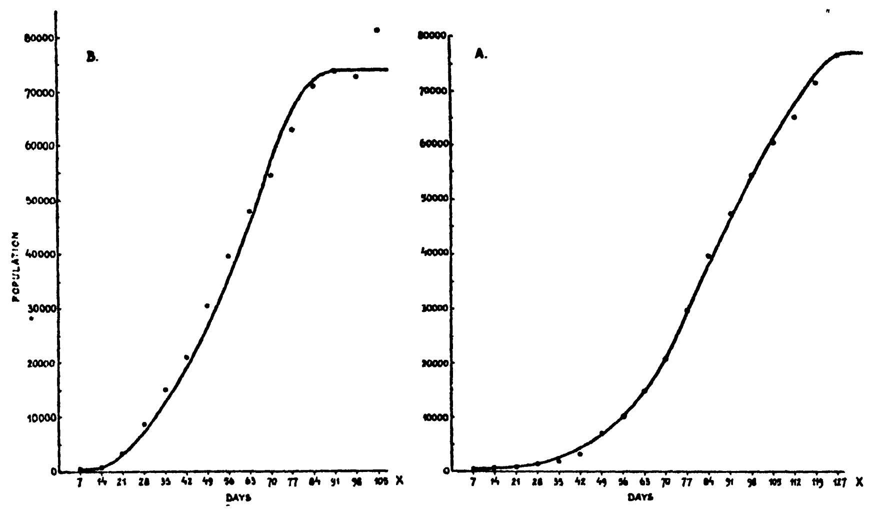

FIG. 4. THE GROWTH CURVE (LOGISTIC CURVE) OF A COLONY OF CYPRIAN (A) AND ITALIAN (B) BEES NEAR BALTIMORE
(Calculated from the data obtained by Nolan)

other conditions being equal, a strong colony tends to remain strong. (Nolan, 1925.)

It is of interest to compare the brood-rearing cycle of a colony of Cyprian bees in 1926 at the same locality in Maryland (Nolan, 1928). It proved to be strikingly similar to the typical colony of Italian bees which has just been described (Colony 4). Other singularities which are ascribed in literature to the Cyprian race did not verify. The colony exhibited no tendency to tolerate laying worker-bees while it had a laying queen, nor did it build more queen cells than usual for the Italian bees in the same apiary. The queen was not more prolific than that of the Italian race.

TABLE 10

x	y	$k-y$	$\frac{k-y}{y}$	$\log\frac{k-y}{y}$
0	0	76,328	∞	∞
7	41	76,287	1,860.66	3.26975
14	372	75,956	204.16	2.30984
21	800	75,528	94.41	1.97502
28	981	75,347	76.81	1.88942
35	1,898	74,430	39.21	1.59340
42	3,675	72,653	19.76	1.29579
49	7,116	69,212	9.72	0.98767
56	10,162	66,166	6.51	0.81358
63	14,201	62,127	4.37	0.64048
70	21,360	54,968	2.57	0.40993
77	29,706	46,622	1.55	0.19033
84	38,930	37,398	0.96	0.99227–1
91	46,846	29,482	0.63	0.79934–1
98	54,092	22,236	0.41	0.61278–1
105	60,222	16,106	0.26	0.41497–1
112	66,219	10,109	0.15	0.17609–1
119	70,919	5,409	0.07	0.84510–2
126	76,328	0	0	

$x = 21.\quad \log\frac{k-y}{y} \times 2.3026 = 1.97502 \times 2.3026 = 2.5726.$

$x = 77.\quad \log\frac{k-y}{y} \times 2.3026 = 0.19033 \times 2.3026 = 0.2479.$

$$a + 21\,b = 2.5726.$$
$$a + 77\,b = 0.2479.$$
$$b = -0.2906.$$
$$a = 3.4444.$$
$$y = \frac{76.328}{1 + e^{3.4444 - 0.2906x}}.$$

This statement is highly important, because it shows that differences in behavior, fertility, etc., which generally are described as racial qualities, are in reality responses to certain environmental conditions in the same direction or even intensity which other races of the honey bee show towards the same conditions.

TABLE 11

x	y	$k-y$	$\frac{k-y}{y}$	$\log\frac{k-y}{y}$
0	0	74,000		
7	140	73,860	527.1	2.72189
14	875	73,125	83.5	1.92169
21	3,640	70,360	19.3	1.28556
28	8,585	65,415	7.61	0.88138
35	15,185	58,815	3.87	0.58771
42	21,910	52,590	2.46	0.42160
49	30,726	43,274	1.42	0.14922
56	39,602	34,398	0.86	0.93450–1
63	48,132	25,868	0.52	0.71600–1
70	54,543	19,457	0.35	0.54407–1
77	62,982	11,018	0.17	0.23045–1
84	71,365	2,635	0.03	0.47712–2
91	73,948	52	0.001	0

$x = 28.\quad \log\frac{k-y}{y} = 0.88138 \times 2.3026 = 1.9295.$

$x = 77.\quad \log\frac{k-y}{y} = 0.23045 - 1 \times 2.3026 = -1.7618.$

$$a + 28\,b = 1.9295$$
$$a + 77\,b = -1.7618$$
$$b = -0.075$$
$$a = 4.0295$$
$$y = \frac{74{,}000}{1 + e^{4.0295 - 0.075x}}.$$

The development of Nolan's representative colonies (Cyprian 1926, Italian 1921) is computed and compiled in Tables 10 and 11 respectively. x indicates the time in days from the beginning of the seasonal population increase, y the total bee-population of all ages and stages, and k the maximum reached (upper asymptote).

The seasonal development of the Cyprian bee follows a perfect logistic curve. This is calculated from Table 10: k = 76,328.

The agreement between the simple formula of the logistic curve and the population trend is not as good in the development of the Italian bee colony No. 4 in 1921. This discrepancy is fully explained by Nolan's explanation under "Initial expansion." The conditions prevailing in March and April were not very favourable and would have allowed a maximal development of only about 74,000 individuals. The very favourable conditions starting in the latter half of May produced

The most outstanding fact is the reduction of the interruption of the brood-rearing cycle to 19 days (October 19 to November 7), whereas in the warmer climate of the coastal plain no interruption at all is observed in a normal winter. The lowest population level is reached just at the end of December. The peak is gained rapidly at the end of April. The progressive part of the seasonal trend lasts thus four months. Oviposition decreases at first slowly from the beginning of April, later quickly, reaching its minimum in late June. This summer depression of the oviposition is manifested in a depression in the total population in late July, which shows a

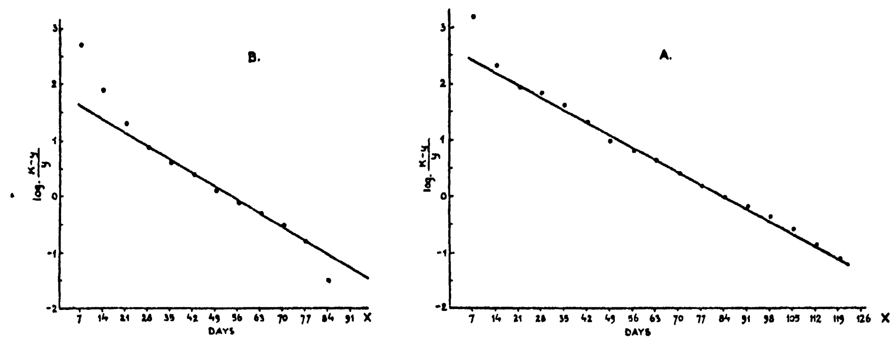

Fig. 5. The Logarithmic Fitting with the Straight Line for the Seasonal Population Growth of the Colonies Described in Fig. 4

an additional rise, which superseded the initial seasonal tendency. This superposition increased the maximum to about 82,000 individuals. The influence of environmental conditions on the population is thus clearly demonstrated.

The data for this colony are in Table 11: k = 74,000.

5. THE SEASONAL HISTORY OF A BEE-HIVE NEAR JERUSALEM

The seasonal history of a modern bee hive (type Langstroth, single-walled) at Kiryath Anavim in 1935/36 has been described in detail (Bodenheimer and Ben Nerya, 1936) elsewhere. Briefly summed up, it is as follows:

minor peak in September. From the interruption of oviposition in late October on, the total population dwindles quickly until the minimum is reached at the end of the year. The maximum and minimum population in April and December respectively show a relation of 9:1.

The flight activity reaches its peak in March/May, the egg-production from February to May, the quantity of stored honey and the maximal weight in April, the sources of nectar from March to June.

The population trend is summed up in Table 12.

In the Palestinian hills oviposition is interrupted only during a very short period, which lasted in 1935 from October

TABLE 12—*Population of Single Walled Hive 1935/36*

DATE	EGGS	LARVAE	PUPAE	NURSE-BEES	HOUSE-BEES	FIELD-BEES	TOTAL
Jan.							
1	1,315	2,650	2,500	1,240	1,655	2,440	11,800
11	1,915	2,680	5,170	1,900	1,240	3,245	16,150
21	1,735	3,590	5,940	4,340	1,900	4,115	21,620
Feb.							
1	2,815	3,520	7,610	5,010	4,755	3,710	27,430
11	2,835	5,690	8,440	5,790	5,010	6,780	34,545
21	3,225	5,580	11,400	6,430	5,270	9,110	41,015
Mar.							
1	3,695	7,360	11,300	9,470	6,320	10,460	48,605
11	3,045	7,290	13,410	8,280	9,470	12,440	53,935
21	3,125	6,170	13,410	11,850	8,280	18,490	61,355
Apr.							
1	2,780	6,240	12,070	11,470	12,090	20,330	65,980
11	2,975	5,970	11,770	10,010	11,470	23,230	65,325
21	2,925	6,150	11,350	10,390	10,010	25,460	66,285
May							
1	2,995	5,320	10,725	10,390	10,390	23,940	63,760
11	2,950	5,345	11,000	9,810	10,390	21,520	61,015
21	2,925	5,240	11,420	9,130	9,810	21,845	60,370
June							
1	1,590	5,155	11,410	9,180	8,890	21,690	57,915
11	1,595	2,340	9,500	9,440	9,180	21,960	54,015
21	650	2,540	5,695	8,345	9,440	20,095	46,765
July							
1	1,870	3,255	4,770	4,605	8,345	20,050	42,895
11	1,185	3,450	6,340	4,130	4,605	19,520	39,230
21	2,270	3,575	6,210	5,075	4,130	10,555	31,815
Aug.							
1	1,545	4,150	7,660	5,210	5,615	8,485	32,660
11	1,970	4,305	7,530	6,145	5,210	9,975	35,135
21	1,505	3,540	7,765	6,780	6,145	11,255	36,990
Sept.							
1	1,700	3,735	6,500	6,665	6,390	12,865	37,855
11	1,215	3,100	6,740	5,805	6,665	14,165	37,690
21	1,425	2,870	5,750	5,490	5,805	13,665	35,005
Oct.							
1	1,055	2,675	5,035	5,160	5,490	13,460	32,875
11	1,650	2,440	4,825	4,575	5,160	12,525	31,175
21	0	2,395	4,790	4,365	4,575	11,510	27,665

Correction: Beginning with October 22, the adults of all groups live 14 days longer than normal and all die between December 1–21 (nurse-bees 1, house-bees 11, field-bees 21 days), i.e. all groups agree again with the above scheme on December 21.

DATE	EGGS	LARVAE	PUPAE	NURSE-BEES	HOUSE-BEES	FIELD-BEES	TOTAL
Nov.							
1	0	0	4,005	4,310	4,320	10,425	23,070
11	470	750	0	2,645	4,320	9,085	17,270
21	380	790	1,370	0	2,645	9,375	14,560
Dec.							
1	725	810	1,560	1,100	0	7,700	11,895
11	365	760	1,900	1,320	1,100	4,560	10,005
21	1,015	780	1,500	1,630	1,320	1,150	7,345

TABLE 12—(*Corrected as above*)

DATE	EGGS	LARVAE	PUPAE	NURSE-BEES	HOUSE-BEES	FIELD-BEES	TOTAL
Nov.							
1	0	0	4,005	4,545	4,175	10,200	22,925
11	470	750	0	7,190	4,175	10,200	22,785
21	380	790	1,370	4,615	4,175	9,475	20,805
Dec.							
1	725	810	1,560	1,120	7,605	4,685	16,505
11	365	760	1,900	1,320	1,120	8,880	14,345
21	1,015	780	1,500	1,630	1,370	1,100	7,345

19 to November 7, i.e. 19 days. On our coast-plains such an interruption does not, as a rule, occur. But even so, the difference between the upper and lower population asymptote is rather remarkable: 66,285 − 7,345 = 58,940. Its sizes at mini-

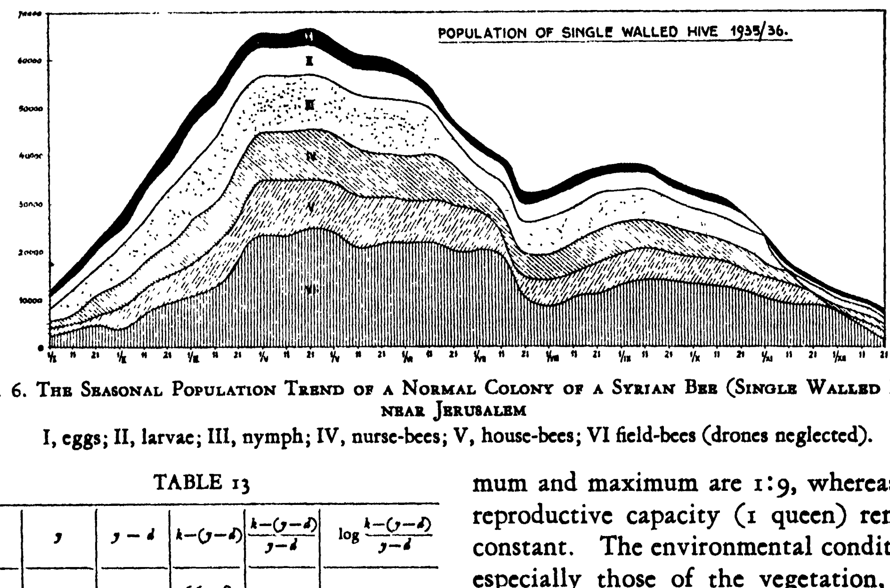

FIG. 6. THE SEASONAL POPULATION TREND OF A NORMAL COLONY OF A SYRIAN BEE (SINGLE WALLED HIVE) NEAR JERUSALEM

I, eggs; II, larvae; III, nymph; IV, nurse-bees; V, house-bees; VI field-bees (drones neglected).

TABLE 13

x	y	$y-d$	$k-(y-d)$	$\frac{k-(y-d)}{y-d}$	$\log\frac{k-(y-d)}{y-d}$
0	7,345	0	66,285	∞	∞
11	11,800	4,455	61,830	13.9	1.14301
21	16,150	8,805	57,480	6.5	0.81291
31	21,620	14,275	52,010	3.6	0.55630
42	27,430	20,085	46,200	2.3	0.36173
52	34,545	27,200	39,085	1.4	0.14613
62	41,015	33,670	32,615	0.96	0.98227−1
70	48,605	41,260	29,025	0.61	0.78533−1
80	53,935	46,590	19,695	0.42	0.62325−1
90	61,355	54,010	12,275	0.22	0.34242−1
101	65,980	58,635	7,650	0.13	0.11394−1
111	65,325	57,980	8,305	0.14	0.14613−1
121	66,285	58,940	7,345	0.12	0.07918−1

$$y = 7{,}345 - \frac{66{,}285}{1 + e^{2.8462 - 0.0464x}}.$$

mum and maximum are 1:9, whereas the reproductive capacity (1 queen) remains constant. The environmental conditions, especially those of the vegetation, constitute the limiting factor.

The seasonal increase follows a logistic curve rather well: k = 66,285; d = 7,345 (Table 13).

In a parallel observation in a double-walled hive the population was as follows: k = 65,945; d = 6,400 (Table 14).

The good agreement between both formulae as well as of the general population trend shows that the observations may be regarded as typical for the specific year and locality. The interruption of oviposition in the double-walled hive

lasted from October 1 to November 11, i.e. 41 days.

Deviation of the calculated population trend from the observed one

Our observations are based on a series of counts made at about a 21 day interval. The brood quantities observed at this interval are connected by straight lines and these lines permit a fairly accurate estimate of the total brood present in the hive at every day of the year. The graphic results are now compared at certain intervals with those of our dynamic calculations. The absolute and percentage ± deviations

TABLE 14

x	y	$y-d$	$k-(y-d)$	$\frac{k-(y-d)}{y-d}$	$\log \frac{k-(y-d)}{y-d}$
0	6,400	0	65,945	∞	∞
11	13,810	7,410	58,505	7.9	0.89763
21	20,830	14,430	51,515	3.6	0.55630
31	28,050	21,650	44,295	2.0	0.30103
42	36,680	30,280	35,665	1.1	0.04139
52	42,310	35,910	30,035	0.84	0.92428−1
62	47,740	41,340	24,605	0.59	0.77085−1
70	53,300	46,900	19,045	0.41	0.61278−1
80	58,530	52,130	13,815	0.26	0.41497−1
90	63,690	57,290	8,655	0.15	0.17609−1
101	65,945	59,545	6,400	0.11	0.04139−1

$$y = 6400 - \frac{65{,}945}{1 + e^{2.2154 - 0.0445x}}.$$

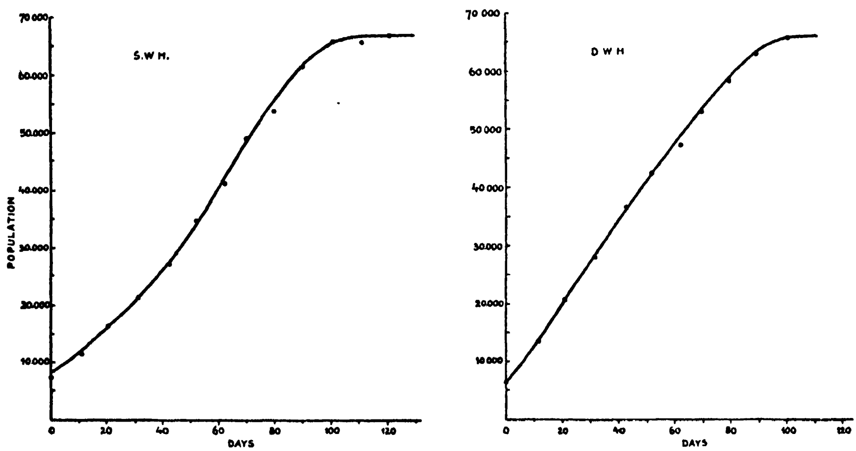

FIG. 7. THE GROWTH CURVE (LOGISTIC CURVE) OF A COLONY (A) SINGLE WALLED HIVE, (B) DOUBLE WALLED HIVE OF THE SYRIAN BEE NEAR JERUSALEM

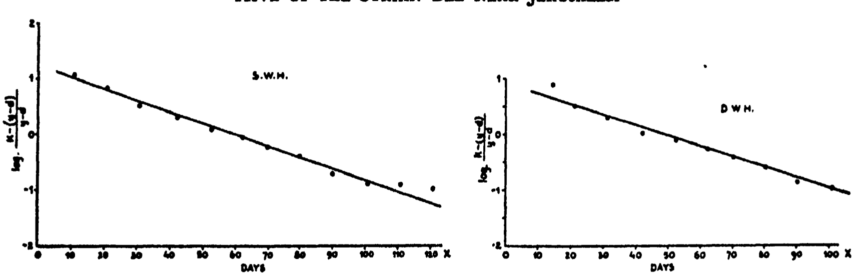

FIG. 8. THE LOGARITHMIC FITTING WITH THE STRAIGHT LINE FOR THE SYRIAN BEE (A) SINGLE WALLED HIVE, (B) DOUBLE WALLED HIVE

from the graphical observation are indicated in Table 15.

The average total deviations of the single walled hive are − 6045 = 1.3 per cent, and + 3670 = 0.8 per cent respectively. Their sum is 2.1 per cent, their difference 0.5 per cent.

It is obvious that our method of cal-

TABLE 15

DATE	TOTAL BROOD Graph	TOTAL BROOD Calculated	DEVIATION Absolute		DEVIATION Percentage	
Jan.						
1	6,600	6,465	−135	+	−2	+
11	9,700	9,765		65		1
21	11,200	11,265		65		1
Feb.						
1	13,250	13,955		705		5
11	16,900	16,965		65		—
21	20,350	20,205	145		1	
Mar.						
1	22,500	22,355	145		1	
11	24,700	23,745	955		4	
21	12,850	22,605	245		1	
Apr.						
1	21,225	21,090	135		1	
11	20,850	20,715	135		1	
21	20,610	20,425	185		1	
May						
1	20,710	19,040	1,670		8	
11	19,390	19,295	95		—	
21	19,680	19,585	95		—	
June						
1	17,970	18,155		185		1
11	13,350	13,435		85		1
21	8,800	8,885		85		1
July						
1	9,810	9,895		85		1
11	10,900	10,975		75		1
21	11,970	12,055		85		1
Aug.						
1	13,280	13,255	25		—	
11	13,800	13,805		5		—
21	12,825	12,810	15		—	
Sept.						
1	11,850	11,935		85		1
11	10,870	11,055		185		2
21	9,920	10,045		125		1
Oct.						
1	9,080	8,765	315		3	
11	8,820	8,915		95		1
21	8,620	7,185	1,435		16	
Nov.						
1	4,320	4,005	315		7	
11	520	1,220		700		133
21	1,840	2,540		700		38
Dec.						
1	2,995	3,095		100		3
11	2,945	3,025		80		3
21	3,200	3,295		95		3
	457,200		−6,045	+3,670		

culation approaches the actual population trend to a very high degree. All high percentage deviations are at the population minimum, where the absolute deviations are of rather small importance.

6. CONCLUSIONS WITH REGARD TO THE SEASONAL POPULATION TREND OF THE HONEY-BEE, WITH SPECIAL REGARD TO AGE-DISTRIBUTION.

(A) Discussion of the premises

The figures given here are apparently the first dynamic analysis of any animal population (excepting that of man). It is therefore important to discuss the reliability of the premises on which the calculations were based.

(1) The fixed duration of every age of the honey-bee, determined for the egg, larva and pupa kept at a constant temperature of 35°C. The respective ages have been confirmed by so many observers that they cannot be doubted. The duration of nurse-, house- and field-stage in the adult worker-bee is based on the observations of Roesch (1925) and Morland (1930). Ten days is the rule for nurse-bees. The house-bees, which Morland and others call control-bees, may serve some days longer as nurse-bees, should the structure of the colony thus require. However, 10 days are quite usual as the average duration of this stage. The least certain data are those pertaining to the longevity of the field-bees. We have accepted here the generally assumed figure of 7 weeks total longevity of the adult worker. Actual observations and experiments by Morland (1930), Phillips (1928), and others show that the longevity of the field-bee rather varies, following the environmental conditions and the working intensity of the field-bee itself. In summer, life duration is probably less (1–2 weeks), whereas in spring and autumn it may extend over 2–4

weeks. The still higher longevity (150–200 days) in the winter is (probably causally) connected with the interruption of the brood-rearing cycle. This fluctuation in the life-duration of the field-bee has not been expressed in our calculations. The mistake involved is of no vital importance as no worker bees of any age participate in normal reproduction. Corrections which could be made only after sufficient empirical data have been collected—the latter lacking at present—would affect a reduction of the worker bees present in the summer. A hypothetical correction has been attempted for the Palestine colony during the short interruption of the brood-rearing cycle. The dynamic trend of the colony is not influenced by such corrections of the workers' life-duration.

(2) Successfully developing eggs do not show a considerable mortality under normal conditions. This premise is also based on the experience of very many experienced bee-keepers. While there exists reliable evidence that—occasionally at least—the queen may lay more eggs than can develop, it is a rule that any egg having started development or having transformed into a maggot practically never dies during its development. The same is true for nurse- and house-bees provided, of course, that no diseases or predators invade the hive. In the latter case, however, a colony would no longer be regarded as normal. The mortality of the field-bees, as far as it is independent of age, activity and environment, depends also on the amount of predators present and on the percentage of bees which fail to find their way back to the hive. Here again we know little about the field-bee.

(3) The fertility of the queen does not differ considerably from day to day, but changes only gradually. This assumption would seem to be in contradiction with some results of our calculation. However, our calculation yields practicable values only in its accumulation and no responsibility can be taken for the *daily* oviposition obtained by it. In nature, the daily transitions are smooth, as far as observed. In order to come closer to reality the daily oviposition data should be smoothed (as averages of 3–5 days). However, since we use in this analysis only the accumulations of 3 successive days no such smoothing seems to be necessary. Actually abrupt changes are observed and these are induced by (a) abrupt changes of environmental conditions (weather, etc.), (b) a major change of the oviposition place affected by the queen, or lack of space for oviposition, (c) conditions preceding swarming in a colony. Such abrupt changes will find an adequate and significant expression by our method of observation and calculation.

Our premises are, therefore, sufficiently sound to serve as a basis for our calculations and analysis.

(*B*) *The seasonal shift of the age structure*

Our analysis makes the study of the age-structure and its seasonal shift in a bee-hive possible for the first time. Fig. 9 gives a bimonthly survey for the single walled hive in Palestine. The identity of the age-structure with that of human populations is obvious. The pyramid represents a growing population, the bell a stagnating and the urn a decreasing population. The bee-population in January shows a very broad base and relatively few individuals in the medium ages. This anormality is smoothed in March, when the population is at its quickest rise. In May the population has grown more or less stable (in reality it has just begun to decline) and shows a practically equal distribution of all ages (bell). The decrease has passed its peak in July. Whereas the

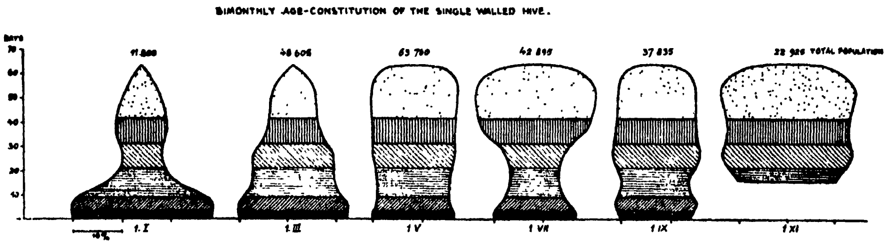

Fig. 9. Age Shift in a Palestine Colony of Honey-Bees, Illustrated by the Age Constitution of the Single Walled Hive near Jerusalem

(Black: eggs. Diagonal lines to right: larvae. Horizontal lines: pupae. Diagonal lines to left: nurse-bees. Vertical lines: house-bees. Dotted: field-bees.)

TABLE 16

DATE	NO. OF INDIVIDUALS PER AGE-DAY						PERCENTAGE OF EVERY AGE*					
	Jan. 1	Mar. 1	May 1	June 1	Sept. 1	Nov. 1	Jan. 1	Mar. 1	May 1	June 1	Sept. 1	Nov. 1
Field-bees	111	475	1,090	910	585	464	7	9	18	25	16	28
House-bees	166	632	1,039	835	639	418	11	11	17	22	17	25
Nurse-bees	124	947	1,039	461	667	455	8	17	17	12	18	27
Pupae	208	942	892	396	542	334	14	17	16	10	14	20
Larvae	442	1,227	886	543	623	0	29	23	15	14	17	0
Eggs	438	1,232	998	623	567	0	29	23	17	17	15	0

* The computation of the age- per day percentage of the bee-population does not change the picture demonstrated by the drawings. It is therefore omitted here.

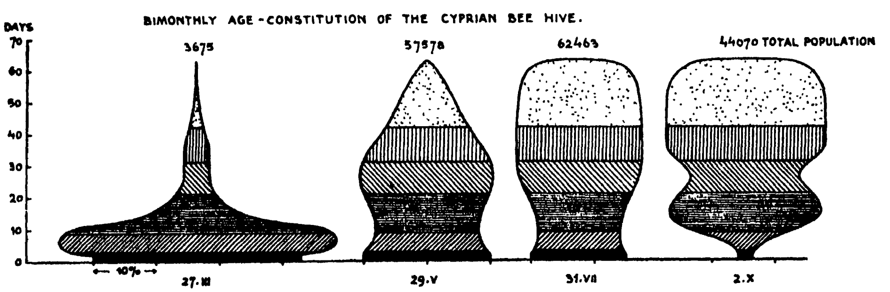

Fig. 10. Age Shift in a Colony of Cyprian Bees near Baltimore
(For explanation see Fig. 9)

TABLE 17

DATE	NO. OF INDIVIDUALS PER AGE-DAY				PERCENTAGE OF EVERY AGE*			
	Mar. 27	May 29	July 31	Oct. 2	Mar. 27	May 29	July 31	Oct. 2
Field-bees	5	504	1,096	851	0.5	8	19	25
House-bees	45	1,062	1,198	844	4	17	20	25
Nurse-bees	42	1,323	1,053	563	4	21	19	17
Pupae	172	1,041	743	804	15	17	14	24
Larvae	496	1,057	720	236	44	17	13	7
Eggs	370	1,270	868	70	33	20	15	2

* See footnote to previous table.

older and medium ages show a typical urnal distribution, the number of eggs and larvae shows a very recent rise, which reaches its peak about the beginning of September. In November, any increase is interrupted and the older ages (pupae to field-bees) show an urnal distribution.

The absolute number of individuals per day-age and the percentages of every stage are compiled in Table 16.

The same conclusions are correct for the age distribution of the Cyprian bee (Colony 1926, Nolan 1928), which is compiled in Table 17 and in Fig. 10.

The transition from pyramid to bell and from bell to urn is also obvious here.

It is worthwhile to stress the similarity in the age structure of bee- and human populations, because of the very great difference in the methods of reproduction. While it is concentrated in one individual in the bees, it is more or less evenly distributed over all the middle-aged human individuals.

(C) *General conclusions*

The seasonal growth of bee-populations follows closely the logistic curve which the classical studies of Pearl (1926) have shown to be of value for animal populations in general. In Palestine as well as in moderate North America the period of actual population increase of the hive is identical (ranging from 100–126 days, or from 14–18 weeks). The seasonal shift in age-structure of the bee-population likewise follows the shift known from human populations, despite the very different type of reproduction.

There are a series of problems in bee-populations which have not been treated in this first analysis. The more important are the influence of swarming on the dynamic trend and the comparison of total annual bee-production in countries with a prolonged hibernal interruption of the brood-rearing cycle with those where this interruption is absent or reduced to a very short period. More material will have to be collected, before the analysis of these and other problems can be astempted.

It gives the writer a great deal of pleasure to acknowledge the help of Mr. A. Ben Nerya, Dr. S. Peller and Mrs. H. Rubel in the preparation of this paper.

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A CRITICAL REVIEW OF SOME RECENT WORK IN SEX DETERMINATION. I. FISHES

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IN MY book on sex intergrades (1931) I reviewed the facts then known about sex determination in fishes and came to the conclusion that in a general way conditions seemed to be very similar to those in the Amphibia, but that important data were still missing. During the past few years considerable work has been done in this group, which unfortunately, however, has not only not clarified the situation but made it even more obscure. In my opinion this is not the consequence of a special complication of the case, but rather of the complicated way of reasoning of the authors who interpreted the facts. We shall therefore review the facts and try to find out whether they do not after all fit into the general scheme derived from other groups of animals.

I. KOSSWIG'S WORK

Kosswig and his student Breider worked with *Xiphophorus*, some species of *Platypoecilus*, and some species of *Limia*. When they started their work it was definitely known that *Platypoecilus maculatus* follows the Abraxas type of sex determination XY = female, XX = male, as proven by sex-linked inheritance of dominant color genes (Gordon, Bellamy). It was further known that *Xiphophorus* shows complicated sex behavior of the type known in Amphibia: the males pass an early female stage, as do the so-called undifferentiated races of frogs, and, in addition, a considerable number of adult females of different ages may still be transformed into males, corresponding to the so-called adult hermaphrodites of frogs. Kosswig proceeded to cross these two genera, which is feasible. (In the following account we shall not use the nomenclature employed by the author, which is in part very tedious and cumbersome and in part derived from preconceived ideas of interpretation. We call the heterozygous sex simply XY, and the homozygous XX.) The following are the actual results. The X-chromosome of *P. maculatus* was in most cases marked by the dominant gene for red color, Dr. There is also a gene for black color, N, within the X-chromosome of *P. maculatus*.

First group

1. *helleri* ♀ × *mac.* ♂: ♀♀ and ♂♂ in the ratio typical for *Xiph.* = 75% ♂♂
2. *mac.* ♀ × F_1 ♂: ♀♀ and ♂♂ 1:1
3. F_1 ♀ × *mac.* ♂: almost only ♂♂
4. *mac.* × *helleri*: ♀♀ and ♂♂ 1:1

Second group with color markers

5. *helleri* ♀ × red *mac.* ♂: ♀♀ and ♂♂ red
6. F_1 ♀ × *helleri* ♂: red ♀♀ and ♂♂ (more ♀♀), gray ♀♀ and ♂♂ (more ♂♂)
7. RF_2 red ♀ × *helleri* ♂: gray ♀♀ and ♂♂, red ♀ (only one case)
8. Red. *mac.* ♀ × *helleri* ♂: red ♂♂, gray ♀♀
9. Red *mac.* ♀ × red F_1 ♂: ½ red ♂♂, ¼ red ♀♀, ¼ gray ♀♀

From these crosses Kosswig draws very strange conclusions. Cross 3 does not contain a Y-chromosome from *Platypoecilus* and whereas no females (or only a few!) appear, Kosswig concludes that the female determiners in *Platypoecilus* must be contained in the Y-chromosome. The

X-chromosome of *Platypoecilus* is then supposed to contain no sex genes. This conclusion becomes possible only if it is assumed in addition that in *Xiphophorus* there are no sex chromosomes and no sex genes. *Xiphophorus* is assumed to be a hermaphrodite with no sex genes whatsoever, in which sex is determined phenotypically by external influences. But, of course, as the crosses of the second group prove, one of the *Xiphophorus* chromosomes is homologous to the X of *Platypoecilus*, giving a typical case of sex-linked inheritance of the Abraxas type (cross 8). Why does Kosswig then conclude that *Xiphophorus* has a phenotypic sex determination? First, because in RF_2 No. 6 red females appear also, which would not be possible if *Xiphophorus* females were XY; secondly, because such a red RF_2 female further backcrossed with a *Xiphophorus* male produces males and females without red; thirdly, because in further generations individuals may be produced containing both chromosomes from *Xiphophorus* supposed to be homologous to the Y of *Platypoecilus* (discerned by the absence of the markers); these are of both sexes.

There is a third unexpected conclusion: The male determiners in *Platypoecilus* are assumed to be scattered over the autosomes. The only reason for this assumption which one can find is the following: In F_2 of the cross *Xiphophorus* × *Platypoecilus* individuals are obtained with two *Xiphophorus* sex chromosomes (there are none such in Kosswig's terminology) and among these there are still more males than in pure *Xiphophorus*, namely, 90 per cent, which is laid to the action of autosomal male determiners derived from *Platypoecilus* and increased by recombination. Finally there is a fourth new assumption: If a *Xiphophorus* female and a *Platypoecilus* male are crossed, both sexes appear. But as it is assumed that *Xiphophorus* has no sex chromosomes but phenotypic sex determination and the *Platypoecilus* male is XX, it is concluded that this F_1 shows phenotypic sex determination, like the mother; therefore the anlage for phenotypic sex determination is inherited within the protoplasm. In the reciprocal cross, however, the protoplasm is derived from *Platypoecilus*, and in addition the Y-chromosome is at work and therefore normal genotypic sex determination takes place.

These are certainly astounding conclusions. Upon them Kosswig constructs additional general views regarding sex determination. We shall not report them here, not only because they contain too many contradictions but because they are of interest only if the foregoing conclusions can stand on critical examination. Let us first try to find out whether the results of the crosses, as far as they go, require any explanation different from the usual one. *Platypoecilus* has a perfectly normal sex determination of the Abraxas type, and there is therefore no *a priori* reason visible why the usual formulae should not be applied: FFMm = female FFMM = male with M within the X-chromosome, no sex gene in the Y, and F presumably in the autosomes, and the balance conditions $FF > M < MM$. *Xiphophorus*, on the other hand, behaves in regard to sex physiology exactly as the undifferentiated races of frogs, with transitory intersexuality of the males (a female phase) and adult transformation of a part of the females. Therefore the analysis of the frog case (mainly due to Witschi) ought first to be applied to this animal before new explanations are sought. There is *a priori* the possibility that male or female heterozygosis is present in *Xiphophorus*. The crosses with *Platypoecilus* ought to give information. Crosses 5 and

8 demonstrate female heterozygosis. A cross between both forms ought therefore to be parallel to a cross between differentiated and undifferentiated races of frogs, where, however, the male is the heterozygous sex.

The formulation which Witschi uses for differentiated and undifferentiated frog races, derived from our formulae for *Lymantria*, is:

Differentiated race: MMFf = ♂ MMFF = ♀
Undifferentiated race: $MMFF_1$ = ♂ MMFF = ♀

This means that M is situated within the autosomes and F within the X-chromosomes. In the males of the heterozygous sex the Y contains a relatively strong female gene F_1; in the differentiated races the Y either does not contain an F gene or only a very weak one, both cases being written f. Different races of different degrees of differentiation might then contain different grades of F from $F - F_1 \rightarrow F_2 \ldots f$. Witschi's results further lead to the assumption that with increasing strength of the F_1-f genes in the Y-chromosome the strength of the F within the X-chromosome decreases, and vice versa. The full formulae then would be (assuming F_1 to be a little weaker than F; F_6 = f; str. = strong; w = weak; Au = autosome):

M(Au) M(Au) F_{str} (X) f or F_6 (Y) = differentiated ♂
M(Au) M(Au) F_{str} (X) F_{str} (X) = differentiated ♀
M(Au) M(Au) F_w (X) F_1 (Y) = undifferentiated ♂
M(Au) M(Au) F_w (X) F_w (X) = undifferentiated ♀

Applying these formulae to our case, where female heterozygosis obtains, we get:

F(Au) F(Au) M_{str} (X) m or M_6 (Y) = ♀ *Platypoecilus maculatus*
F(Au) F(Au) M_{str} (X) M_{str} (X) = ♂ *Platypoecilus maculatus*
F(Au) F(Au) M_w (X) M_1 (Y) = ♀ *Xiphophorus helleri*
F(Au) F(Au) M_w (X) M_w (X) = ♂ *Xiphophorus helleri*

These are, of course, the formulae applicable to all animals and plants with the additional assumption of a more even balance in the case of more hermaphroditic undifferentiated forms (a complete balance MM = FF would, of course, mean an hermaphrodite).

Let us now apply these old formulae to the crosses reported above

Cross 1. *helleri* ♀ × *maculatus* ♂
$M_w M_1 \times M_{str} M_{str}$
$\frac{1}{2} M_w M_{str}$ ♂♂ — $\frac{1}{2} M_1 M_{str}$ ♀♀ with very strong male tendency, stronger than in *Xiphophorus*, therefore much sex transformation

Cross 4: *maculatus* ♀ × *helleri* ♂
$M_{str}m \times M_w M_w$
$\frac{1}{2} M_w m$ ♀♀ — $\frac{1}{2} M_w M_{str}$ ♂♂

Cross 2: *maculatus* ♀ × F_1 ♀ from 1
$M_{str}m \times M_w M_{str}$
$\frac{1}{4} M_w m$ = ♀♀ — $\frac{1}{4} M_w M_w$ = ♂♂
$\frac{1}{4} M_{str} m$ = ♀♀ — $\frac{1}{4} M_w M_{str}$ = ♂♂

Cross 3: F_1 ♀ from 1 × *maculatus* ♂
$M_{str}M_1 \times M_{str}M_{str}$
$\frac{1}{2} M_{str}M_1$ ♀♀* — $\frac{1}{2} M_{str}M_{str}$ ♂♂

* With very strong male tendency (stronger than in *Xiphophorus*) like above.

The formulae then express the results if we do not insist upon the exact number of females in crosses 1 and 3, with mostly males. But as Breider has shown that lines of *Xiphophorus* may be selected with a genetically different amount of sex transformation, i.e. percentage of males above 50 per cent, it is of no use to insist upon differences in the number of non-transformed females, as these crosses were made with unselected stock, among which lines occurred with as much as 95 per cent of males.

We come now to the second set of crosses, in which the color genes for red pigment are involved. Here another fact must first be mentioned which seems to have impressed Kosswig considerably. He finds that in broods containing the genes for red pigment comparatively many females appear; if, however, genes for black pigmentation are present males are predominant. The genes for black pigment in addition produce in the crosses and backcrosses to *Platypoecilus* melanotic tumors and high lethality. Strangely enough, Kosswig concludes that the red pigment genes act like female sex genes because the number of females is increased, i.e. the number of females not undergoing transformation. Upon this assumption he bases a phylogenetic speculation on the origin of sex genes, which however need not be reviewed, as the basic interpretation is already impossible consisting, as it does, in mistaking an action which either diminishes the amount of sex transformation in an undifferentiated (i.e. more balanced) form (or an action which affects the vitality of the males), for an action affecting primary sex determination.

Returning now to the further crosses, we notice that Nos. 5 and 8 are just what is to be expected on the basis of the color gene being a sex-linked dominant.

Cross 6: (*helleri* ♀ × red *maculatus* ♂) ♀ × *helleri* ♂.
(The letter *r* means the presence of the gene for red.)

$M^r_{str}M_1 \times M_w\,M_w$

$\frac{1}{2}\,M_w\,M_1$	$\frac{1}{2}\,M^r_{str}M_w$
gray ♀♀	red ♂♂

The actual result in a summary of all cases was, however:

♂♂ red	♀♀ red	♂♂ gray	♀♀ gray
55	105	152	57

This is the result which mainly led Kosswig to his interpretations. His argument is: If both reds and grays appear in both sexes, sex cannot be determined here by genetic means; it must be determined phenotypically (this in spite of the very typical numbers). Now the result for the grays corresponds to the normal sex ratio; therefore it is the *Xiphophorus* contribution which controls the type of sex determination, according to Kosswig. The preponderance of females among the red offspring show him once more, however, that the red gene acts like a female sex determiner.

Of course, Kosswig saw that these results look very much like a case of crossing-over between the gene for pigment and the sex gene. He dismisses this possibility, however, with a few remarks, being fascinated by his belief in phenotypic sex determination. But if we look at the facts impartially, the most obvious explanation is a complete (50 per cent) crossing-over between the pigment gene and the sex gene M, which would produce the four classes in equal numbers:

1. $M_w\ M_1$	2. $M^r_{str}M_w$	3. $M^r_1\ M_w$	4. $M_{str}M_w$
gray ♀♀	red ♂♂	red ♀♀	gray ♂♂

The gray females, however, have the genetic constitution of *Xiphophorus*, which means considerable transformation into males and therefore excess of males as is well known. The red males, however, have the constitution which is known already from the F_1 results to impair male vitality and to favor larger numbers of females. No reason is visible, therefore, for introducing new and improbable explanations. (Kosswig seems to think that a backcross involving a male heterozygous for the pigment gene and giving the same result excludes an explanation by crossing-over; but as a matter of fact exactly the same result is to be expected if the foregoing argument is accepted.)

Cross 7: As is easily shown, there is the same expectation as in cross 6. Only one brood was obtained in which the expected minority of red males was missing; probably a chance result, which is as good an explanation as those offered by Kosswig, involving perfectly gratuitous male determiners in the autosomes.

Cross 9: red *Platypoecilus* ♀ × red F_1 ♂
$M^rm \times M^rM$

1. M^rM^r	2. M^rM	3. M^rm	4. Mm
red ♂♂	red ♂♂	red ♀♀	gray ♀♀

which was obtained

Thus it has been shown that Kosswig's crosses gave in all cases with sufficient numbers exactly what was to be expected on the basis of the old explanation if *Xiphophorus* corresponds genetically to the undifferentiated frog races. The only fact which has to be added is the action of the red pigment genes detrimental to the survival of males (there is, by the way, a corresponding action of black pigment on females). There is no cogent reason—as a matter of fact, no reason whatsoever—for introducing one or all of the theories of Kosswig.

We shall now report a further set of experiments by the same author in which another species of *Platypoecilus*, namely, *variatus*, is involved, which has males dotted with black. The males of this form pass through a transitory female stage and consequently behave like the undifferentiated frog races. Unfortunately, hardly any information is given regarding the crossing of this form with the two other species. But there are a number of crosses between *variatus* and *helleri* × *maculatus* hybrids, producing triple hybrids. Regarding simple F_1 we learn only that *maculatus* females × *variatus* males produce both sexes (see Bellamy's results below). In all the following triple crosses the X-chromosome of *maculatus* was marked by the red pigment gene.

Cross 10: (*helleri* ♀ × *maculatus* ♂) ♀ × *variatus* ♂.
Red ♀♀ and ♂♂, gray ♀♀ and ♂♂, namely:
50 ♂♂ 23 ♀♀ red, 23 ♂♂ 24 ♀♀ gray

Cross 11: *variatus* ♀ × (*maculatus* ♀ × *helleri* ♂) ♂
39 ♂♂ 1 ♀ red, 9 ♂♂ 33 ♀♀ gray

These results lead again to complicated formulations. Now the supposedly empty X-chromosome of *maculatus* seems to act in a male direction; therefore it may contain male determiners, which, however, act only if *variatus* genes are present. The few red females and gray males need another complex explanation. The red gene was to act, as we learned, as a female determiner; therefore, in the author's opinion, the red female is regarded as the result of a feminizing action of the red gene. Regarding the four gray males, however, the *deus ex machina* is a series of male determining autosomes from the *maculatus* parent, supposed to be present and to act in some individuals. From a cross which we shall report later the further conclusion is drawn that in *variatus* the male is the heterozygous sex.

It is very difficult to give a concrete explanation of these results, because most of the elementary facts are unknown: neither is it proven that *variatus* is XY = male (see Bellamy below) nor has an analysis of the crosses *variatus* × the two other species been made. Under the circumstances we draw attention only to the following points: If the *variatus* male is homogametic like the *maculatus* male, the crosses ought to give the same results as the corresponding crosses with *maculatus*, which is not the case. Therefore some special feature must have been brought in by the *variatus* parent. We find in Kosswig's paper that *variatus* belongs to the undifferentiated races, all males starting development as females; genetically this means that the M genes within the

X-chromosomes are relatively weak (assuming female heterozygosis as in *maculatus*). But there is another important fact: The red triple hybrids show a different type of red color; further, the time of appearance and spreading of red color is different from that in *maculatus* and *maculatus* × *helleri* hybrids. Finally, the red hybrids show very slow sexual differentiation and *frequently do not become fertile at all.* This shows that the simultaneous presence of the gene Dr and something from *variatus* causes peculiar disturbances. We shall see at once that Bellamy's work shows that *variatus* is either homozygous as a female or that some *variatus* females are genetic males in the female phase (with male homozygosis). No final interpretation of the foregoing results can be given before this point has been settled. But in any case the features just mentioned will probably account for the peculiarities of the crosses, without recurrence to the new formulations of genetic sex determination.

There follow now a series of such triple crosses with rather strange results.

Cross 12: *variatus* ♀ × red ♂ from cross 10
  2 red ♂♂ 7 gray ♀♀

Cross 12a: *variatus* ♀ × red ♂ from 11
  56 red ♂♂ 79 gray ♀♀

Cross 13: *variatus* ♀ × gray ♂ from cross 10
  33 ♂♂ 34 ♀♀ gray

Cross 14: gray ♀ from 10 × gray brother
  43♂♂ 48 ♀♀ gray

Cross 15: *helleri* ♀ × gray ♂ from 10
  20 ♂♂ 10 ♀♀ gray

Cross 16: red *maculatus* ♀ with color genes RSp × red ♂ from 10 (color gene Dr)
  9 red ♂♂ 17 ♀♀ RSp 3 ♀♀ red

Cross 17: red ♀ from 16 × *maculatus* ♂
  ♂♂ almost exclusively

Of these crosses, 13–15 are without special significance; 12, 12a, and 16, however, are remarkable because daughters are like mothers and sons like fathers (in 16 a few daughters are like the father). We shall see at once that Bellamy obtained the same result with *variatus* female × (*variatus* × *maculatus*) male and explained it simply by female homozygosity of *variatus*. The difficulties of such an explanation will be pointed out, pending further information. But cross 16, where the mother is *maculatus*, ought not to give such a result. Here again is a difficulty which cannot be solved before it is known whether *variatus* has female heterozygosis or male heterozygosis with protracted female phase in the genetic males. Judgment of these results must therefore be suspended.

We do not need to review the additional assumptions which Kosswig has to make to cover these cases. So far as can be judged from the very incomplete series of crosses, everything may be explained on the basis of ordinary genetic assumptions, applying the old formulae for female heterozygosis to all these forms, keeping in mind that we are dealing partly with undifferentiated races and similar special features.

Recently the same problem was attacked by Bellamy. Through his courtesy I had access to his MS, from which I am permitted to quote. The results he had thus far after crossing *variatus* and *maculatus* are:

1. *variatus* ♀ × *maculatus* ♂: only ♂♂
2. *maculatus* ♀ × F_1 ♂: ♀♀ and ♂♂ in equal numbers
3. *variatus* ♀ × F_1 ♂: ♀♀ and ♂♂ in equal numbers
4. *maculatus* ♀ × *variatus* ♂: many ♂♂, few ♀♀, 1 intersex

The *maculatus* X-chromosomes were marked with a color gene and in cross 2 about ½ of the individuals showed

its presence, and $\frac{1}{8}$ did not. In cross 3 daughters were like the mother and sons like the father. Bellamy concludes from these preliminary facts that the *variatus* female is homozygous, and it is easy to work out the general results on this basis. But there are a number of difficulties and, in addition, the results are not in harmony with all of Kosswig's results. To mention only a few points: (1) The description of the race *variatus* shows it to be different from the form with which Kosswig worked. (2) Why are all individuals of cross 1 males? They might equally well be females, if the general formulation derived from *Lymantria* or *Drosophila* applies. (3) Why do Kosswig's crosses never yield only males? (4) In cross 2 why are there not produced equal numbers of wild type and colored individuals? Is there no alternative explanation? Thus far wherever unisexual crosses have been analyzed in these fishes and in Amphibia (see Aida below) it has turned out that one of the parents was a sex-reversal individual with the genetic formula of the original sex. As a late sex reversal was recorded in some of Kosswig's as well as Bellamy's material, it might be that in *variatus* genetic males in a protracted female phase (with a chance of late sex reversal) are found frequently and that Bellamy worked with such, which would give exactly the same results as were obtained, whereas Kosswig crossed both genetic females and males in a female phase. Further speculation on this point is not needed, as Bellamy has started experiments on a large scale which will furnish the information still lacking. Whether the scale will turn in favor of heterozygous females or males in a female phase, it seems certain that the explanation of the results will not require the complicated assumptions of Kosswig.

There is, finally, a set of experiments with another form of *Platypoecilus*, *P. xiphidium*.

1. Cross *xiphidium* ♀ × *maculatus* ♂, only ♂♂
2. Cross *xiphidium* ♀ × F_1 ♂, red sons and gray daughters
3. Cross triple hybrid ♀ with RSp genes and X from *variatus* × *xiphidium*, red and gray ♀♀ and ♂♂

From crosses 1 and 2 the author concludes that in *Platypoecilus xiphidium* the male sex is again heterozygous. To make this work it must be further assumed that the male genes from *maculatus* (supposed to be in many of the autosomes) are dominant. But cross 2 requires the assumption, as was the case before, that unknown genes in the X of *maculatus* act in this peculiar combination like male determiners. We notice at once that these crosses correspond exactly to Bellamy's crosses 1 and 2. The comment is therefore the same. There will be much more work required to elucidate these cases; until this is done it will be of no use to introduce complicated new assumptions, to claim that they have been proven when this is not the case, and to erect even phylogenetic speculations upon obscure facts.

Kosswig's student, Breider, has made a similar series of experiments with another genus, *Limia*. *L. nigrofasciata* is an undifferentiated race, the males having an early female phase, but finally with a normal sex ratio. *L. caudofasciata*: The conditions are not perfectly clear, though it seems that this also belongs to the undifferentiated forms, and *L. vittata* the same. The two latter forms behave somewhat like *Xiphophorus* in so far as sex changes seem to occur easily. For *vittata* a great variation in the sex ratio and typical juvenile hermaphroditism have

been shown recently. The results of all his crosses are summarized in Table 1, in which numbers are mentioned only when they depart from a 1:1 ratio.

Let us look at these crosses without prejudice. We find three types: (1) a majority of combinations with a 1:1 ratio of the sexes; (2) a considerable number with preponderance of females; (3) one cross, number 1, with only females. If we look at the second group all but one (26) of the combinations are backcrosses with double doses either of cau or of vitt. These two forms, however, are descr bed as undifferentiated races with inclination to the formation of the males after a female phase and considerable variation of the sex ratio, though the details are by no means clear and no exact data are given concerning later sex transformations. All these crosses point to very simple conditions; namely, female or male heterozygosis in all three forms, decision depending upon sex-linked genes and an additional hereditary inclination towards more or less late male differentiation after a female stage in *caudofasciata* and *vittata*. This is enhanced in a hybrid if two sets of chromosomes from undifferentiated forms are present. All this is within the limits of known occurrence among such fishes. There remains only one cross, No. 1, with nothing but females. It is worth while to compare this

TABLE 1

ni = *nigrofasciata; cau* = *caudofasciata; vitt* = *vittata*

NO.	CROSS	♀	♂	REMARKS
1	ni × cau	168	—	Up to 4 months of age.
2	cau × ni	50%	50%	
3	(ni × cau) × cau	219	50	
4	(cau × ni) × cau	176	60	
5	$(\text{cau} \times \text{ni})^2$	50%	50%	
6	(ni × cau) × ni	50%	50%	
7	(cau × ni) × ni	50%	50%	
8	ni × (cau × ni)	50%	50%	
9	(ni × cau) (cau × ni)	50%	50%	
10	cau × (cau × ni)	50%	50%	
11	vitt × cau	23	14	3 undiff.
12	cau × vitt	26	27	15 undiff.
13	vitt × (vitt × cau)	213	29	
14	vitt × (cau × vitt)	198	41	
15	(cau × vitt) × vitt	267	108	
16	(vitt × cau) × cau	50%	50%	
17	cau × (vitt × cau)	50%	50%	
18	cau × (cau × vitt)	50%	50%	
19	$(\text{vitt} \times \text{cau})^2$	50%	50%	
20	$(\text{cau} \times \text{vitt})^2$	50%	50%	
21	(cau × vitt) (vitt × cau)	50%	50%	
22	ni × vitt	50%	50%	3-4 months old ♂ still in ♀ phase!
23	vit × ni	50%	50%	
24	(ni × vitt) × vitt	81	12	
25	vitt × (ni × vitt)	21	2	
26	ni × (ni × vitt)	55	9	
27	(vitt × ni) × ni	50%	50%	

with cross 22, which is ni × vitt instead of ni × cau. There is *a priori* no reason why these two should not give the same result, whatever the interpretation. For cross 22 we find it noted that sexual differentiation is very slow, that some individuals do not become fertile after a year; in females 3–4 month-old ovaries were found with additional male tissue. All this means that the genetic males have a protracted female phase. I should therefore conclude that the exactly parallel cross 1 is of the same type and that the late transformation of genetic males has been overlooked because not expected. If this is correct, and all the facts point in this direction, the whole experiment is indeed a very simple one.

Breider, however, is of a different opinion. Fascinated by Kosswig's theories he takes cross 1 as the starting point and concludes that the species *nigrofasciata* has male heterozygosis, the two others, however, phenotypic sex determination. To explain all the other crosses he needs the whole arsenal of Kosswig's assumptions, dominance of one type of sex determination over the other, empty X-chromosomes, which are in some cases not empty, multiple autosomal genes, etc. As facts stand at present, however, there is no need for all these assumptions. We conclude: The experiments of Kosswig and Breider fall in line with what we know from other organisms, especially frogs. The few special features may well be explained on ordinary genetic lines, like crossover, lethal effects, etc. For an exhaustive analysis the material is far from sufficient. There is thus far no ground for Kosswig's interpretations and the whole body of his speculations based upon these (not reviewed here). There is especially no base found in the facts that such things exist as phenotypic sex determination, male and female heterozygosis within the same genus, empty X-chromosomes, female determiners in the Y, multiple autosomal male determiners, etc.

2. WINGE'S WORK

During the past few years Winge has published a number of papers on his crosses with the genus *Lebistes*. In this case also a theory of sex determination has been derived which he thinks is different from the older ones and which differs also from Kosswig's conclusions.

The actual facts in question are the following:

(1) In *Lebistes* the male is the heterozygous sex, as proved by sex-linked inheritance.

(2) The Y-chromosome contains many genes (for color), some of which cross over to the X, while others do not.

(3) Ordinarily the females do not show any effect of the color genes, which act in a sex-controlled way.

(4) *Lebistes* is one of the forms in which transformation of females into males occurs, parallel to the well-known cases in frogs. The details of this phenomenon are not sufficiently known to make a complete comparison with the frog case.

(5) In a cross with X- and Y-chromosomes marked by color genes three exceptional males appeared showing the colors required by the presence of both X-, i.e. XX males or, as we called them in *Lymantria*, males by transformation. This is not so very surprising, as such transformations are known to occur here, but in this case, fortunately, the markers prove the XX constitution. (In frogs and *Lymantria* the same proof was furnished by the later sex ratios only.)

(6) These three XX males crossed to unrelated females produced only females, which then is a repetition of Crew's experiment with frogs and of similar experiments with *Lymantria*.

(7) Some of these daughters were backcrossed to the father and again only females were obtained.

(8) Granddaughters again backcrossed to the grandfather gave 30 females and one XX male.

(9) The same granddaughters mated with their XX sons gave ½ females ½ XX males.

(10) Continued breeding from such females and XX males gave extremely different results: sometimes half females half males, sometimes only females, sometimes many females and few males, and sometimes also more males than females.

(11) There seemed to be an influence of the seasons, as one and the same female produced both sexes in summer and only females in winter.

(12) Crossing these XX males with unselected females produced only female offspring.

From this first set of facts Winge draws the following conclusions: The X-chromosomes contain a rather potent female determiner, and the Y-chromosome a strong male determiner. But in addition the autosomes contain many female and male determiners of different value. Ordinarily the total value of these latter ones is balanced and they do not exert any influence upon sex determination. But if, e.g. by selection their values are shifted to the female or male side, they may accumulate so as to override the action of the sex genes within the sex chromosomes. This is exactly what Winge believes to have happened. His males by transformation (XX) he believes—of course, without any proof—to have arisen by the fortuitous accumulation of male autosomal genes of high male value. The two backcrosses which he made with these he believes to have been a selection experiment with these autosomes. When in the following backcross an XX male appeared again, he thought that this selection was again successful. And when in the next generation he obtained ½ XX males he claimed that he had selected a very strong male autosome which henceforth would act like a sex chromosome. In his opinion he has proven the transformation of an autosome into a sex chromosome and of the sex chromosomes into autosomes!

This is certainly a most interesting claim which calls for a closer examination of its foundations. Winge does not have a very good opinion of the older views regarding sex determination (the FM-balance formulae). In his opinion "the formulas may perhaps be justified in a text-book for students where it is often necessary to present the phenomena in a simplified way, but they cannot be recognized as being perfectly valid scientifically." Thus it may be of interest to find out, first, where the alleged superiority of his formulation comes in.

(1) Winge needs the major F-genes in the X-chromosomes just like the older authors.

(2) He assumes a major M-gene in the Y-chromosome, just as Goldschmidt did for a long time in *Lymantria* (here F, of course) until he could prove experimentally that the action in question was protoplasmic. As a matter of fact, there is not a single experimental fact in Winge's papers which would prove the presence of a sex gene in the Y-chromosome.

(3) Winge assumes the presence of many female and male (+ and −) genes in the autosomes, which, however, are without effect under normal circumstances. In ordinary genetic terminology this would mean that many autosomal genes may act as modifying genes on either sex, either if mutated or if recombined in a definite way. One such autosomal modifier was also found a long time ago in *Lymantria* and analyzed genetically (the gene T).

At closer inspection, then, the only difference between the old formulation and Winge's view is that the older authors claimed the presence of autosomal modifiers only when they were actually found, whereas Winge introduces them in large numbers *a priori*. This would certainly not do for a textbook; but is it really justified scientifically? The answer will be given by considering whether the new facts found by Winge, namely, the reappearing of XX males after a few backcrosses to normal females and their daughters and granddaughters, are actually outside the reach of the old formulation.

The explanation applied formerly to cases of sex reversal of the present type (e.g. Crew's case, *Lymantria*) was that in the female MMFF, where F > M either a weak F or a strong M was introduced (either by presence in the stock, or by mutation, or by appropriate crosses, as

in *Lymantria*). The formula for such a male by transformation would be either $M_{str}MFF$ or $M_{str}M_{str}FF$ or MMF_wF or MMF_wF_w. As the cross with an unrelated female actually gives only females, it follows that the XX must be homozygous for the change in question, i.e. either $M_{str}M_{str}$ or F_wF_w. Both of these give the same results, of course, and we therefore apply only the one assumption, MMF_wF_w, which allows us to dispense with autosomal MM in writing the formulae:

1. $FF \times F_wF_w = FF_w$ = all ♀♀
2. $FF_w \times F_wF_w = FF_w, F_wF_w$ ½ ♀♀ ½ XX ♂♂

The actual result was that No. 2 gave only females the first time, one male the second time, and half males only the third time. This is all important to Winge because he wants to prove that in these three generations he has selected the autosomal male genes. But then he continues breeding within this line and he gets, as reported, sometimes only females, sometimes a few males, and sometimes both sexes, though the formulae must remain the same. I cannot understand why this is in any way different from the result of the backcrosses and why two perfectly identical results must have a thoroughly different explanation, one by selection of assumed autosomal genes, the other by phenotypic influences. In my opinion the results agree completely with the old formulation as applied above. To which, however, it must be added that the F/M balance in the XX males is so close to even that small influences, e.g. seasons or modifying genes or unknown phenotypic influences, may shift the balance to one or the other side. (I should expect intersexes also in these crosses; they are not mentioned, but probably no dissections were made.) There is, then, nothing in the facts recorded by Winge which calls for theoretical assumptions beyond the well-established old ones. And it must be emphasized most strongly that Winge's claim of having transformed a sex chromosome into an autosome has no foundation in the facts reported by himself. Moreover, his experiments give no information whatsoever upon eventually existing autosomal modifiers, and, further, they give no information whatsoever upon eventual sex genes in the Y-chromosome. They have changed nothing as yet in the aspect of the problem of sex determination.

There is another set of facts in Winge's publication. In one cross with marked chromosomes he found seven females showing a Y-chromosome character. (The accompanying picture shows this character to be very different, however, from the same in males! In addition, it is very exceptional in *Lebistes* that females show color at all.) It is therefore assumed that these are females XY, i.e. females by transformation (parallel to males by transformation in *Lymantria*). An explanation by crossing-over of the color gene ma from the Y to an X is held to be impossible because crossover of this gene is said never to occur. (But Winge reports later that Mr. Eloff, who received his material from him, claims to have found such a crossover, with which Winge disagrees. There usually is, of course, no crossover in *Drosophila* males, but nevertheless it occurs.) The further crosses obtained with these XY females give the expected results, including the segregation of ¼ YY males, which are perfectly viable. From this he concludes again that here female determining autosomes have been selected and, in addition, that the Y contains the male determiners.

Leaving aside these speculations, there

is proof afforded that the exceptional females were XY females, namely, by ascertaining that the YY males actually exist.

Winge reports one cross of the presumably YY male with a normal female giving only 42 males, which indeed proves the point. These facts, of course, have nothing to do with the mode of sex determination. If it is proven that the Ma females were actually XY, the same reasoning would apply to these females by transformation as before to the males by transformation, both being paralleled by the *Lymantria* cases. We shall return to this below.

3. AIDA'S WORK

Aida has obtained in *Aplocheilus* what amounts to practically identical results with those of Winge, but he offers a larger bulk of experiments. Here, also, the male is the heterozygous sex and the X- and Y-chromosomes are marked by color genes. He also finds exceptional males and proves by their female offspring that they are XX males by transformation. Parallel to Winge's results, he further states that such males sometimes produce only females, sometimes a few additional males, sometimes females and males, and sometimes even more males. In reviewing Winge's work we have already seen that a proper evaluation of these facts disposes of all his theoretical claims. Aida has made a special study of this point which completely corroborates my criticism of Winge's claims (corroborates, because my criticism was written a long time before Aida's paper appeared).

By crossing individual sex-reversal males to different females in many combinations, Aida finds the following in regard to the appearance of males in smaller or larger numbers:

(1) There is an individual difference in regard to different sex-reversal males, some producing more, some less, male offspring. (2) There is an influence upon the result of individual females of the same line, some tending to produce more or less male offspring. (3) Wild type females always give exclusively females with the XX males, but for one occasional male. (4) There is a decisive influence of temperature upon the production of males in these combinations. (5) By selecting lines with relatively many males, combinations with more males than females may be obtained.

Without further discussion these facts show at once that Winge's claim of transforming autosomes into X-chromosomes is without foundation.

Aida found also in his form the sex-reversal XY female. His analysis led to the same results as Winge's: XY $\times$ XY = XX XY XY YY = 3 males: 1 female and the YY male viable; these produce only males with normal females, and these results, identical with those of Winge, are in this case thoroughly established by many crosses and combinations.

Aida gives a theoretical explanation of his results, which he believes to be a new theory. In fact it is only a restatement of the explanation derived from the *Lymantria* and frog work in a little different language. He thinks that the autosomes contain genes for the production of primary sexual characters. The action of these must be stimulated by "stimulating" genes in the sex chromosomes, some of which stimulate the male, others the female, sex characters. (This is, up to this point, the old view of Correns, which Aida overlooked.) Now Aida supposes that these sex-exciting genes (the realizer in Correns's theory) act by their quantity, i.e. there is a threshold and a quantity above this "excites" male, below it, female differentiation, as it may be, and of course the X-chromosome mechanism provides these two quantities.

In the sex-reversal fishes the potency or quantity of this substance fluctuates for some unknown reason and therefore goes beyond the threshold, more or less, dependent upon external and internal influences. This, of course, makes all the results possible. Aida proceeds then to apply the same formulation to the *Lymantria* work and finds that he can explain this in the same way. But he has not realized why, namely, because his formulation is in essence, though not in words, identical with mine. What I call F-genes (male heterozygosis) he calls exciters; what I call different quantities of F (one F, two F) balanced against the same quantity of M he calls different quantities of the exciter working upon the always equal genes of sexual differentiation. What I call epistatic minimum for the balance F/M he calls the threshold value for the male or female exciting action of the exciters. What I call weak and strong M he calls fluctuating quantities of the exciter. Thus of course, he must also get the same results, though in different language. Aida's explanation is then essentially the same as the one which we derived from the *Lymantria* formulations, though couched in different language. And the facts found by Aida prove indeed that this interpretation is correct, whatever language we are using, and that Winge's theories are without foundation and unnecessary.

Thus we see that all the interesting work done in this group of fishes fits, as far as it goes, into the theory of sex derived from the *Lymantria* work, without a single additional hypothesis being required if the old formulation is applied logically.

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MORPHOGENESIS OF THE SHOULDER ARCHITECTURE

PART VI. THERIAN MAMMALIA

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IN SO FAR as concerns the architecture of the appendages, the step from living reptiles to therian mammals is a great one; and in most respects the latter differ almost as widely from prototherians. In fact monotremes are more properly comparable with reptiles than with therian mammals, and they will receive no consideration in the present contribution. Accordingly, in this paper the term Mammalia refers to Theria. The chief reason for the dissimilarity in the pectoral appendages of these groups is attributable to the difference in the manner in which the limbs are used. In the prone position of the reptilian body, the largely horizontal position of the humerus, with divergent elbows, requires a very different muscular arrangement and bony scaffolding from that suited to the needs of mammals. The typically reptilian architecture of the shoulder is unsuited to speedy or long sustained action. In order to change to the mammalian plan it was necessary for reptiles to bring the elbow beneath the body, involving an alteration of the shoulder joint and its controlling musculature, which Reptilia found so difficult of accomplishment that only a single group (the Theriodontia) ever succeeded in its accomplishment. The resulting improvement in function doubtless played no inconsiderable part in the development of the class Mammalia. The result, in improved form, is that mammals may operate the limbs for purposes of locomotion in a single plane, for extended periods, with the expenditure of a minimum of energy.

SKELETON

In mammals the only surviving part of the membranous girdle is the clavicle. This no longer occurs partially parallel with the border of the scapular girdle, as is frequently the case in reptiles, but is attached to the acromion at one end. It is the first bone in the body to ossify, and this takes place from two primary centers, marking the attachments of mm. cleidomastoideus and trapezius. From these points chondrification and subsequent ossification spread in both directions. A secondary center appears at the sternal end much later (at 20 years in man). The fact that the clavicle arises from both membrane and cartilage has been interpreted (Parker, Howes. *et al.*) as indicating that it is a compound bone, involving arguments which it seems unnecessary to repeat here. At the scapular end of the calvicle is often a meniscus-like cartilage, and at the sternal end, two others. That one of the latter represents the procoracoid, as was at one time claimed, is extremely unlikely. Rather should both be considered as belonging with the sternum. Except for the irregularity in its embryology, there appears to be no evidence in comparative anatomy to indicate that the clavicle is other than a single element of the mem-

branous girdle. In two respects it is rather unique; it is a radius rod rather than an anchorage, and it is the only "long" bone of membranous derivation. Perhaps these facts account for the noteworthy features of its development.

In its rôle of radius rod the clavicle is useful to mammals which employ the manus as a tool. Usually it is stated that clavicles are absent in those mammals which use the forelimbs for support only. In a way this is so, but no rule of this sort can be given. Rather is it a fact that the clavicle furnishes anchorage to a part of the accessory field of musculature and a part of the deltoid. When these two fields fuse in part, reflecting the need for a cephalohumeral muscle, the clavicle tends to disappear. In addition to this stimulus there doubtless are others of a mechanical nature, associated with the function of the clavicle as a strut.

Well developed clavicles are present in all the Marsupialia except the bandicoots (Peramelidae), in Insectivora except *Potomogale*, in Dermoptera, Chiroptera, Primates, and Tubulidentata. The Edentata are variable; sloths, armadillos, and *Cyclopes* have clavicles, while the rest of the anteaters, and the Manidae, lack them. The Rodentia are extremely variable; some forms have strong clavicles, others lack them, and in still others they are vestigeal (as in Leporidae) or ligamentous (*Castor*). In terrestrial Carnivora clavicles are rudimentary, while Pinnipedia, Cetacea, Proboscidea, Hyracoidea, Sirenia, Perissodactyla, and Artiodactyla are supposed to be entirely aclaviculate. When present the clavicle is variously curved, the noteworthy exception being in the bat *Diclidurus*, in which this bone is trifid (Miller, 1907). It is attached to the sternum medially and the acromion laterally. When the clavicle is absent or vestigeal a raphe is supposed to take its place; but this raphe marks the point of junction of the accessory with the deltoid fields, and may not concern the clavicle.

Scapular girdle

I have already discussed the scapular girdle in previous parts of the present contribution. Probably all that need be offered here is a recapitulation of my beliefs. The scapula proper originally provided attachment for the suprazonal group of muscles. As the cleithrum disappeared the homologue of the teres minor and most of the trapezius settled upon the scapula, and then a part of the deltoid shifted to it from the clavicle. Later still a part of the latissimus settled upon it, to become the teres major, and origins of the supra- and infraspinati migrated to it from the procoracoid. With the exception of the secondary change of the two muscles last mentioned, then, the scapula is concerned with dorsal muscles only.

The coracoid provided an area for the origin of the coracobrachial group of muscles and for the shorter head of the biceps as the latter developed from the brachialis. As the angle of the brachium altered, in the change from the reptilian to the typical mammalian position of the limb, the muscle origins contracted and the bone retreated, to adopt the form typical of the mammalian coracoid process. It is interesting to note, however, that in early developmental stages of Marsupialia, the coracoid cartilage reaches the sternum, as reported by Broom in a series of papers. It is useless to speculate on the significance of this item, however, until we know what embryonic muscles are concerned. At least in adult members of the class, there arises from the coracoid process all the mammalian representatives of those muscles which in lower tetrapods originated

from the broader coracoid, or posterior coracoid, and the two must be considered as homologous.

The primitive procoracoid provided an area for the origin of the group of muscles that I have termed coracohumeral. The origin of a part of this musculature shifted to a firmer anchorage on the sternum, to become the pectoralis major, and that of the remainder migrated to the scapula, to become the supra- and infraspinati. Hence, the procoracoid musculature left the procoracoid completely, and the bone retreated toward the glenoid. But meanwhile the origin of one division of the biceps brachii may have become associated with the posterior procoracoid, so the bone may not have disappeared entirely. If the procoracoid is represented in therian mammals it is by the supraglenoid tubercle, indubitably, from which arises caput longum biceps brachii. This detail having been determined, it is, in my opinion, fruitless to argue further as to the actual presence or absence of the bone. The fact that this tubercle usually develops from a separate center of ossification is without phylogenetic significance, for such centers merely reflect local stresses.

To a greater degree perhaps than any other bone in the body, the conformation of the scapula is influenced solely by muscular action. In this, some muscles are particularly influential, others play a minor rôle, and in some cases the influences at work are complex.

The scapula is quite variable in form, in conformation of the glenovertebral and coracovertebral angles, position and shape of the spine and acromion, and in the coracoid process. Chiefly influential in moulding the glenovertebral angle is the caudal part of the serratus anterior, and to a much lesser degree, the teres major, while infraspinatus, subscapularis, and teres minor appear to have every little to do with the conformation of the angle. The caudal part of the serratus anterior is a depressor (ventral rotator) of the scapula and is used in protraction of the arm. Increase in the effectiveness of this action is correlated with a falciform shape of this angle. Conformation of the coracovertebral angle of the scapula is determined chiefly by the levator anguli scapuli, to a minor degree by the subscapularis, and probably to a negligible extent by the supraspinatus. The width of the scapula appears to depend upon a considerable number of factors, and not solely, or even chiefly, upon the width of the supra- and infraspinatus muscles, for these may be much narrower than their fossae. Of more importance is probably the subscapularis, and still more so, the optimum angle of leverage of the two vertebral angles.

The spine of the scapula is extremely variable. Undoubtedly its most important influence is supplied by the trapezius, for where that muscle is greatly reduced or absent there is no spine of any consequence; but the deltoid also has a considerable influence. Both muscles, and the levator scapulae ventralis when present, affect the acromion, which may be bifid. Usually the spine is attached to the scapula along a line extending from near the vertebral border to the spinous notch, at the neck of the scapula. Occasionally (in such dissimilar genera as *Lagostomus*, *Loncheres*, *Georychus*, *Ochotona*) this notch is located midway of the scapula blade, so that the attachment of the spine to the scapula is very short.

The relative width of the supra- and infraspinous fossae is quite variable, even in the same order (about 1:3 in man to 1:1 in gibbons). Moles are perhaps the most bizarre in this respect, the infraspinatus fossa being a minute groove in

Uropsilus, while *Talpa* and *Mogera* lack an infraspinatus muscle, so their infraspinous fossa is really a teres fossa (Campbell, MS). In moles and most shrews both fossae are so narrow that the whole scapula is markedly rodlike; but in others of this order (as *Erinaceus*, *Solenodon*, *Tupaia*) it is of the usual mammalian shape. It is also narrow in ungulates. Eared seals have a very large supraspinous space, and cetaceans one of negligible size. Notably in some bears and edentates the infraspinous space is divided into an infraspinous and a teres fossa by a ridge between, attributable to the action of the long triceps, or that and the teres minor together.

In some mammals (Cetacea, some ungulates) the vertebral border of the scapula tends to remain distinct, first as a cartilaginous and in later life often as a calcified margin of variable width. To this is attached the suprazonal musculature, and it is of sufficient phylogenetic importance to merit recognition as a separate element of the scapula. A suprascapular notch, for the passage of n. suprascapularis, usually is absent, but may occur (many primates, *Choloepus*), or even be present as a foramen completely surrounded by bone (Bradypodidae, Myrmecophagidae). A well defined coracoid process appears to depend chiefly upon m. coracobrachialis, and to a lesser extent upon caput breve biceps brachii. At least when the former muscle is absent the process is virtually undefined. The mammalian glenoid cavity needs little description, and yet it embodies one of the most significant changes that took place in reptilian-mammalian phylogeny. This altered the directior in which the glenoid faced, from caudolateral to ventral—a simple change but one which very few reptiles were able to acomplish. This, and a corresponding change in the head of the humerus, allowed the plane of the thoracic limb to be held at a right angle to the ground, so that the body could be supported with the expenditure of a minimum of effort.

Ossification of the mammalian scappula varies between different groups. In some of the smaller and the weaker sorts the centers appear to number no more than three—body and spine together, coracoid, and acromion, possibly with a separate center for the glenoid. Man is said to have as many as nine centers. At least the one for the body may be considered as primary, but it is impossible to determine which of the remainder have phylogenetic weight, for centers can disappear with the reduction of stress, or new ones appear as muscular stresses increase. The supraglenoid tubercle may fall in the latter category.

The change in the glenoid from the reptilian to the typically mammalian position was made completely possible only by the extreme reduction of the coracoids. A change in the articular surface of the humeral head was, of course, a requisite, but this doubtless was very easily accomplished. With this exception there was little necessary alteration in the free portion of the appendage. This part of the reptile limb can be placed in the mammalian position, but the ulnar border of the manus will then be presented to the ground, and pronation of about 90 degrees, through the forearm bones, will be required in order to present the palm to the ground.

So no twisting of the humerus appears to have taken place in the change from reptile to mammal. In the very earliest amphibian stage there was such a twisting, as explained by Romer (1931). This accounts, at least largely, for the tetrahedral form of the bone in early tetrapods, but thereafter there was only a reshaping,

an alteration in the presentation of the articular surface, and a slight migration of the processes to conform to the change in the angles of leverage of the muscles following the bringing of the limb beneath the body. The appearance of twisting is given by the last vestige of the originally tetrahedral shape, and by the facts that m. brachialis and n. radialis spiral about the shaft.

NERVES

With the exception of primates, accurate and complete descriptions of the brachial plexus of very few mammals indeed are to be found in the literature. I have investigated this detail in a number of orders myself, but I do not wish to delay publication of the present contribution until all orders can be dissected. On the whole, the pectoral limb of the more generalized primates, and of man, is of fairly "primitive" organization, and as the human brachial plexus is in basic respects an almost exact counterpart of that of the opossum (*Didelphis*) and of rodents, it is considered that it is entirely satisfactory as an example of conditions in Mammalia.

The mammalian brachial plexus may be said usually to have its axis through sp. n. 7 (n. 10 in *Bradypus*). This contrasts with conditions in other classes, in which the axis is through C. 3 in *Rana*, C. 4 in *Necturus*, C. 5 in *Chamaeleo*, presumably with intermediate conditions in other reptiles progressing to C. 8 in *Iguana* and C. 9 in *Crocodilus*. Accordingly it is seen that there has been a caudal migration of the brachial plexus, less pronounced in some living reptiles than in mammals and more pronounced in others.

The general plexus plan in mammals conforms to that characteristic of vertebrates, comprising three trunks, the first and third of these being joined by an additional nerve each, so that typically five nerves contribute to the plexus. Usually these are the nerves from C. 5 to T. 1 inclusive. Occasionally, in a prefixed plexus, C. 4 may be involved (man), or regularly T. 2 (n. 10) *Macaca*, although in *Bradypus* it extends to n. 12. Or in mammals in which there is a restriction in the diversification of movements, the plexus may be almost (*Canis*) or entirely (*Bos*, *Galeopithecus*) confined to four nerves. In *Equus* (Sisson) there are three main nerves (C. 7–T. 1), with very small contributions from C. 6 and T. 2. In this case the plexus is very definitely postfixed. Partly on this sort of evidence but chiefly because of rather complicated neurological considerations, it is indicated that no greater number of myomeres contributed to the formation of a mammalian limb innervated by six nerves, than to that of an amphibian innervated by two, but that a multiplicity of contributing nerves is a reflection of habitual complication of the movements of the member. Thus a restricted plexus, accompanying restriction of diversity of movement, is a primitive character in *Necturus* and a specialized characteristic of some mammals.

It is well nigh impossible to stress too strongly the importance of the fact that the limb plexus is divisible into dorsal nerve elements, innervating the extensor groups of muscles, and ventral elements to the flexor groups. Separation of the plexus as a whole into these two components does not take place until the level of the cords, although every one of the high branches belongs with one or the other, and each group has its cell bodies located in predictable parts of the spinal gray matter.

In Mammalia there are typically a lateral and a medial cord carrying ventral fibers, and between them a posterior cord formed of dorsal fibers. In cold-blooded

vertebrates this statement hardly applies, not because of fundamental differences in the plexus, but for the reason that splitting occurs in a slightly different manner. Occasionally, as in some rodents and Cetacea, the three-trunk plan is obscured and a two-trunk plan results. In all vertebrates there is a slight tendency for the ventral components of the plexus to occupy more linear territory than the dorsal part.

Often, in analyzing the plexus, it is impossible to determine precisely which nerves contribute to a particular branch. In such cases an approximation is all that is possible. In the following pages conditions in the uniquely post-fixed plexus of *Bradypus* are not included.

N. *accessorius spinalis* arises from the upper cervical part of the cord as far caudally in some mammals (*Equus*) as the seventh, or only as far as the first cervical segment (*Phocaena*, Hepburn and Waterston). Some of the fibers of accessory relationship, however, adopt a short cut and emerge with spinal nerves C. 2 to C. 3 or 4, and an increasing tendency for the accessory fibers to follow a cervical pathway may be considered to be a progressive tendency in mammals, culminating in some individuals of the long-necked ungulates (giraffe, camel, llama), in which all accessory fibers follow this pathway and the ramus externus n. accessorii is no longer present. These fibers innervate mm. sternocleidomastoideus and trapezius of the branchiomeric group of muscles.

N. *omohyoideus* is the only "hypobranchial" or ventral nerve of the neck that concerns us. It is one of the lower branches of the ansa hypoglossi, single or double; to the two bellies of m. omohyoideus.

Dorsal (extensor) division

Suprazonal group

Nn. *thoracales posteriores* consist of three closely-related entities that may well be considered as three subgroups of the same nerve element. The basic one of these is n. thoracalis longus, to m. serratus longus. In man, in which the muscle is not continuous with m. levator scapulae, the nerves to the latter are considered as separated from the long thoracic nerve, but in those mammals in which the two muscles are continuous, innervation may be also, from C. 2 to 7 or 8, contributing in addition to m. levator scapulae ventralis when that is present. The third part of the posterior thoracic nerve element is n. dorsalis scapulae, to m. rhomboideus. When this muscle is confined to the lower nuchal and thoracic regions the innervation usually is derived from C. 5; in cases in which the rhomboid is more extensive an additional nerve may be concerned.

Shoulder group

N. *thoracodorsalis* is derived from the extreme posterior part of the plexus in *Necturus*, the anterior part in *Iguana*, and the middle part in mammals. The reason for this phylogenetic shift is obscure. In mammals it arises chiefly from C. 7, and usually partly from one or the other of the neighboring nerves.

Nn. *subscapularis*, usually double or at times occurring as four or perhaps more branches, typically arise from the three anterior nerves of the plexus, with some rather slight variation. They innervate mm. subscapularis and teres major, and it is possible that they represent two nerve elements, as discussed under the musculature.

N. *axillaris*, innervating mm. deltoideus and teres minor, typically arises from the

first three nerves of the plexus, with a tendency to contract to two, at least in some carnivores (*Canis*) and *Galeopithecus*.

Brachio-antebrachial group

N. *radialis*. It is particularly difficult to determine the exact number of nerve roots involved in this and the median nerve. Perhaps it should be said that typically the former is derived from C. 6–T. 1, with slight extension, one way or the other in prefixed or postfixed cases. Its extent may be reduced, either mainly or completely, to two nerves (*Canis*). The radial nerve innervates the dorsal musculature of the arm, and it is never double, as in cold-blooded tetrapods.

Ventral (flexor) division

Infrazonal group

N. *subclavius*, to the muscle of the same name, is derived usually from the upper two, or at times probably only the uppermost, nerves of the plexus.

Shoulder group

Nn. thoracales anteriores form the anterior thoracic loop, and in mammals is composed of two elements—n. pectoralis proprius and a part of the n. coracoideus of lower vertebrates. The muscles supplied by the former element are mm. (1) pectoralis minor, (2) pectoralis abdominalis, and (3) panniculus carnosus; and by the latter, m. pectoralis major. In man we know that (1) is supplied by C. 7–8; but man lacks (2) and (3), and in other mammals it is impossible to analyze the components of the anterior thoracic nerve without degenerative experiments. It is known, however, that the typical condition in mammals is for mm. panniculus carnosus and pectoralis abdominalis to be innervated by the last two nerves of the plexus. Thus the fibers of the anterior thoracic nerves that represent n. pectoralis proprius have maintained their primitive position at the caudal border of the plexus. On the other hand, the part of the anterior thoracic nerve innervating m. pectoralis major, derived from n. coracoideus, arises in cold-blooded vertebrates chiefly from the most cranial root of the plexus, possibly with the next root participating. In consequence probably of the fact that this muscle in mammals is used in a variety of ways, its motor nucleus has increased in size to the point where the muscle is innervated by at least four and probably at times by five nerves of the plexus (except in particular cases, as Cetacea, in which but three nerves may be involved).

N. *suprascapularis* represents the remainder of n. coracoideus of lower tetrapods. The element, to mm. supra- and infraspinati, has maintained its original relationship, from the first, or first two, nerves of the plexus. It is definitely a ventral nerve element, but apparently because of the long affiliation of its musculature with the dorsal muscles of the shoulder, the peripheral part of the nerve at least is in process of alteration, so that grossly the nerve appears to be dorsal just as frequently as ventral in its relationship.

Brachio-antebrachial group

N. *musculocutaneus*, from the first two or three nerves of the plexus, is the first branch of the common flexor nerve of the arm. It is a consolidation of n. coracobrachiales of lower tetrapods (sometimes distinct in mammals also), and the common flexor fibers that innervate mm. biceps brachii and brachialis.

N. *medianus* and *n. ulnaris* should be considered as two branches of the common flexor nerve to forearm and hand. The former supplies most of the muscles of the forearm and several in the hand,

while the latter supplies two or more elements upon the ulnar side of the forearm and most of the hand muscles; but the innervation of several of the hand muscles may be by one or the other, for the terminal branches of a nerve are variable. Occasionally (*Hylobates*) all the three flexor nerves of the arm, except for their terminal branches, are confined in a single sheath.

MUSCLES

Because of limitation of space the musculature of the shoulder cannot be treated in great detail, with complete description of the conditions in all mammals, but only the variations occurring, with mention of particular mammals in illustration. Similarly, it will be impossible to give credit to each of the authors who have reported upon the muscles of the region. Probably the best course to follow is to rely upon my own dissections as much as possible, and to list in the bibliography only those papers on myology, annotated where necessary, that have been used in preparation of the text, but without reference to the authors. The details that appear to be most significant will be included, trivial items omitted. A serious difficulty is presented by the fact that muscles often are ambiguously described, have been overlooked, or misinterpreted. Also it must be realized that general statements can be made only tentatively. Thus it may be said that a condition is general in Carnivora, whereas the dissection of additional species may prove the statement false.

Branchiomeric musculature

The accessory feld musculature should be considered as having developed from a single sheet, and occasionally (as in *Allactaga*) the sternocleidomastoid is separable from the trapezius only at origin.

M. trapezius is present in all mammals except the Odontoceti. Fundamentally it is a single muscle arising from the mid-dorsal line and lambdoid crest caudally to the posterior thorax, and inserting upon the lateral clavicle, acromion, anterior spine and the dorsal part of the posterior border of the scapular spine. Because of the diversity of direction in which the fibers extend and the fact that it is the most frequently used muscle of the shoulder, it tends often to separate into three parts—clavotrapezius, acromiotrapezius, and spinotrapezius. The facility with which these parts are separable depends upon the ways in which the muscles are used and the feature, without phylogenetic significance, is variable within individual orders. Usually the distinct parts are but slightly separated, although the hiatus at times is considerable (Pinnipedia, *Canis*). In the Mysticeti only is it severely restricted to the cervical region and arises from the cervical fascia; or it may be so extensive as to overlap the cleidomastoid (*Gymnura*, *Centetes*, some Carnivora).

So far as I know, an occipital origin is lacking in no order except Dermoptera, and it appears to be uniformly present in Insectivora, Chiroptera, Primates, Tubulidentata, Proboscidea, Hyracoidea, and Sirenia, although in some descriptions it is difficult to determine whether an occipital trapezius or a cleidomastoid is meant. In the remaining orders it is variable.

Possible confusion exists in the literature between the clavicular insertion of the trapezius and cleidomastoid. An insertion upon the clavicle, or its raphe in aclaviculates, is present in Chiroptera, Primates excepting gibbons, and Rodentia possibly with a few exceptions; and it should be considered as present in those orders having a cephalohumeral. It is absent in Dermoptera, Cetacea, and

Tubulidentata, while the feature is variable in marsupials, insectivores, and edentates.

Occasionally (*Zalophus*) insertion includes the presternum, and in particular cases (Mysticeti, Phocidae) extends to the tuberosities of the humerus, although usually where a humeral insertion is mentioned (as in *Dasyurus*) one suspects the involvement of the deltoid. An interesting situation occurs in the Microchiroptera, in which a part of the trapezius—the occipito-pollicalis—extends to the neighborhood of the thumb. In the pocket gophers (probably in all Heteromyidae) (Hill), and moles (Campbell, MS) a curious cutaneous retractor muscle is formed by fusion of a thin sheet of the trapezius with a portion of the platysma. In *Tatusia* a separate slip of this field, from the cranium, fuses with the panniculus sheet, and a similar situation should be sought in all mammals with particularly complicated panniculus system.

Perhaps the most noteworthy condition of the trapezius is that in which the anterior part of the sheet combines with the cleidomastoid to a variable degree and these fuse with the clavodeltoid to form a cephalohumeral (or mastohumeral) muscle, continuous from the mastoid region to the humerus. The identity of the units involved is determined by the innervation, and it is a logical development of those mammals needing vigorous protraction of the forelimb. Hence it is usually present in cursorial forms and those using the pectoral appendage for aquatic propulsion, as well as in some fossorial sorts. A cephalohumeral is present in Edentata excepting *Cyclopes* and *Chlamyphorus*, in Carnivora, Proboscidea, Hyracoidea, Sirenia, Perissodactyla, and Artiodactyla, and in many of them it is extremely robust. In Hyracoidea the deltoid part of the complex extends its insertion to the ulna, in *Equus* to the fascia of the forearm, and in *Viverra* to the tendon of the brachialis.

M. *sternocleidomastoideus*. This should be considered as a single complex representing the cranioventral margin of the accessory field, which often splits into several parts. Fundamentally it seems that these should be considered as consisting of sternomastoid-sterno-occipital, and cleidomastoid-cleido-occipital portions, but the first of these pairs is uniformly termed sternomastoid, even if its fibers encroach on the occiput. As might be expected, the distinctiveness of the three recognized divisions is extremely variable, and one or the other may be partially or totally suppressed. At least among the larger orders, the insectivores are the only ones in which a cleido-occipital seems to be always present, and it is present in some members of all the others except the bats. It inserts for a variable distance along the lateral part of the lambdoid crest.

Perhaps the cleidomastoid should be considered as present in all mammals, although at times it is rudimentary (some bats), with origin scarcely encroaching upon the clavicle (or clavicular raphe). At times (*Bradypus*, *Bos*) it arises from the first rib as well as the clavicle, from the scapular spine (*Cyclopes*), or even from the humeral head (some Cetacea), although when origin appears to be from the humerus the deltoid usually is involved. It may be fused with the sternomastoid (as in some Marsupialia, some Chiroptera), more often, perhaps, is perfectly distinct, or may occur double (sciuromorphs).

In aclaviculate mammals the cleidomastoid, and cleido-occipital when present, customarily fuse with the clavodeltoid, usually as a single muscle from the head to the humerus, and the cranioventral part of the trapezius may be involved in this. Occasionally the condition is

too complex for precise analysis, as in *Zalophus*, which appears to have a distinct sternomastoid, two muscles from presternum and deep pectoral to the occiput and nuchum, both joining an additional cephalohumeral portion. Leche (1900) considered that the cephalohumeral is basically divisible into acephalohumeral proper, formed by the fusion of cleido-occipital with deltoid, and a mastoideo-humeral, comprising cleidomastoid and deltoid. This scheme is useful to bear in mind but in many cases it is difficult to apply. In *Equus* both parts are fused, but in *Tapirus* only the mastoideohumeralis is present, and this inserts upon the deep pectoral (Campbell). Or, in Artiodactyla, the two are separate at origin but later fuse. *Manatus* and *Hyrax*, at least, are said to have only the cephalohumeral portion. The statement of Leche that in the horse there is fusion of the complex with splenius capitis and complexus minor, and of Sisson that attachment is also to the upper cervical transverse processes, are misinterpretations and refer to the levator scapulae ventralis.

Typically the sternomastoid extends between sternum and mastoid process, but it is subject to considerable variation. Usually it is quite well separated from its antimere, but there may be fusion in the midline (*Centetes*, some Carnivora), some of its fibers may contribute to the mastoideohumeralis, or it may be double (*Hyaena*, *Proteles*, some Chiroptera). It may have extensive origin from a midline raphe cranial to the sternum (*Potomogale*, *Phoca*), or arise partially (*Potomogale*) or wholly (Proboscidea) from the first rib.

The cranial attachment is usually the same for sterno- and cleidomastoid. Frequently it is upon the paroccipital process instead of the mastoid, or may be merely upon the undifferentiated surface of an enlarged audital bulla (*Dipodomys*). In mammals of ungulate affinity it frequently extends partially or wholly to the angle of the jaw, even piercing the parotid gland (Sirenia). In swine, hippos, sheep and goats, however, the paramastoid only is involved; it goes to both paroccipital and the angle of the jaw in Perissodactyla, Hyracoidea, and in Artiodactyla may even extend over the masseter muscle, or to the zygoma in Proboscidea.

Innervation.—The accessory field of musculature is supplied by the spinal accessory nerve, and in addition by one (usually 2) or more of the spinal nerves from C. 2 to C. 4, except in the long-necked ungulates (giraffe, camel, llama), in which the spinal accessory nerve at times is absent.

Myomeric musculature

Infrahyoid division

M. *omohyoideus* is the only muscle of this division of interest in the present connection. It is a unit of the ansa hypoglossi group, or ventral musculature of the neck, whose origin has migrated from the sternum along the clavicle to a final attachment, either to m. supraspinatus or bone, along the cranial border of the scapula, anywhere from the vicinity of the notch (most mammals) to the coracovertebral angle (*Didelphis*). Reflecting its derivation, it is frequently attached to the sternum by a fascial, tendinous, or even muscular, sling near its middle. It may be divided into two bellies by a stout, round tendon, which varies to the condition of complete absence. Parsons (1898) has suggested that the two-bellied condition has been brought about by a broadening of the shoulders (higher primates, bats), but this seems doubtful. Occasionally the insertion shifts from the hyoid to the root of the tongue (*Phascolarctos*) or the neighborhood of the symphysis menti (*Bathyergus*), and in such cases one suspects

that there has occurred a fusion with some suprahyoid slip.

The omohyoid is present in Marsupialia, Chiroptera, Primates (except some platyrrhines) and Perissodactyla; absent in Dermoptera, Proboscidea, Tubulidentata, and Hyracoidea. In Cetacea it appears to be present in Mysticeti and absent in Odontoceti. Among insectivora it is lacking in Talpidae, Chrysochloridae, *Solenodon* and *Myogale*. It is found in *Zalophus* but not in *Phoca*. In the Carnivora it is absent in Felidae and Canidae, present in Ursidae and Mustelidae, and variable in the other families (Windle and Parsons). It is variable, but mostly absent in Edentata. Among rodents it is usually present in the Sciuromorpha and Hystricomorpha, absent in Lagomorpha, and variable in Myomorpha. In Artiodactyla a curious situation obtains, for the more caudal portion is lacking in deer, giraffes, camels, and some sheep, and the cranial part fans out over the cervical fascia.

Innervation.—So far as exactly determined, by C. 2 and C. 3 via the ansa hypoglossi, possibly with some slight variation.

Dorsal (extensor) division of the appendage

Suprazonal matrix

The basic detail of this group is the serratus anterior. In the ancestral condition it expanded to include all the cervical vertebrae, and in this form it is encountered in many mammals. In many others, however, a part has dropped out—either the anterior cervical, posterior cervical, or slips from the middle ribs. The question of nomenclature therefore intrudes. By common usage the part from the anterior cervical region is termed levator scapulae (dorsalis). If origin be from the posterior cervicals only, what then? Or if the sheet from the ribs occurs in two portions, what should these be called? New names for muscles are not readily established, and any solution is arbitrary with the investigator. To my mind the proper course is to call the whole sheet serratus anterior, pars cervicis and pars thoracis. When an anterior cervical slip is separated this should be called levator scapulae dorsalis, and when the thoracic portion occurs in two divisions, these may be known as partes anterior and posterior. It is wiser to consider that the levator scapulae must always arise from cervical vertebrae, the neighboring fascia, or basi-occipital, and never from the lambdoid or mastoid. Because of the way in which the supraspinous fossa has developed it seems clear that the primitive point of insertion of the levator scapulae was in a position corresponding to the dorsal root of spina scapulae. At times it occupies this position (*Phascolomys*), but more often it has shifted to the coracovertebral angle of the bone. Basically, however, a part of the levator separated and its insertion migrated ventrally along the spine to the neighborhood of the acromion, and where it still occurs it is known by a great variety of names, but a desirable term is levator scapulae ventralis, as indicating its derivation.

The rhomboideus almost certainly arose by a spreading of the posterior serratus fibers to attach to the middorsal line, and thence by expansion cranialward, as is indicated by the situation in some of the cold-blooded tetrapods.

Mm. serratus anterior et levator scapulae, as indicated, should be discussed together. Typically they arise as a single sheet from the transverse processes of most, if not all, of the cervical vertebrae, and most of the ribs, the exact number being too variable, even individually, for consideration in this connection. Insertion is typically along

the medial aspect of the entire vertebral border of the scapulae, the parts to the two angles, chiefly the glenovertebral, being more robust than the rest.

This sheet is continuous or practically so in most Marsupialia (not phalangers, and beginning anywhere from C. 1 to C. 4), Insectivora (overlapping in some, as in *Chrysochloris*, and beginning C. 2 or 3), Dermoptera (C. 3), Primates (excepting higher forms, and in them including C. 1), many Rodentia (C. 1), most Carnivora (mostly C. 3, seldom from atlas (*Galera*)), Tubulidentata (C. 2), Hyracoidea (C. 3), Sirenia (C. 1), Perissodactyla and Arteriodactyla (both C. 1, 2, or 3), with some variation from the figures as given.

Among orders having the sheet in two parts are the Edentata (fused in *Manis*), mostly with the levator beginning at C. 3 or 4, but at C. 1 in *Cyclopes* and C. 6 in *Bradypus*. In the latter type it seems that the levator should be considered as absent, and there is an anterior and posterior part of the serratus, with a hiatus between. This is the condition usual in Chiroptera also, but at times these may show origin as far forward as C. 3. In Pinnipedia the parts are separable but adjoin, or even overlap, from approximately C. 3 to T. 3 and T. 3 to 10 or 12. In Cetacea there is a restricted levator (C. 1) and a serratus from several (3 or 4) ribs; while in Proboscidea the levator is similar (C. 1), but the serratus is double (T. 1–5 and T. 7–10).

At least occasionally (*Gymnura*) a slip of the levator may occupy its primitive insertion upon the dorsal root of the spine and the rest shift to the coracovertebral angle.

M. levator scapulae ventralis (atlantoscapularis inferior, omotrachelien, omoatlantic, atlanto-acromialis, cervico-humeralis, trachelo-acromial, acromio-atlantal, levator claviculae, occipito-acromial) is usually present and normally extends from atlas to acromion; so it appears to have been derived from the more superficial and higher fibers of the levator scapulae dorsalis. In the literature it frequently has been misidentified, and statements that it is absent, or present, are not always trustworthy. It has nothing to do with the spinal accessory musculature, but the fact that its attachment at the shoulder frequently joins that of the trapezius, and that occasionally the two secondarily fuse (Chrysochloridae), coupled with faulty dissection, has been responsible for the erroneous reports that it is at times (pig, peccary, brocket, sheep, hyrax) innervated by n. XI. Occasionally it arises from more vertebrae than the atlas (*Equus*, C. 1–4), or shifts caudally (Tubulidentata, C. 2–3; Chiroptera, C. 2–3 or C. 4–5; *Giraffa*, C. 5 or 7). It may have origin from the base of the atlas (*Gymnura*, *Erinaceus*), or even have a basioccipital attachment (*Cephalophus*, many Rodentia). It may insert upon the spine just dorsal to the acromion, extend to the fascia over the teres minor (*Hyrax*) or supraspinatus (Tabulidentata), or migrate to the clavicle (many Chiroptera and Rodentia and some Primates). An interesting condition obtains in the Perissodactyla, in which the muscle fuses with the deltoid to form the transversohumeralis, possibly with a small part of the cleidomastoid occasionally included. The same situation may be found to obtain in *Phoca*, in which the muscle inserts upon the greater tuberosity and deltoid ridge.

The muscle is said to be absent, or usually so, in Talpinae at least among Insectivora, *Plecotus* at least among Chiroptera, the higher Primates and *Perodicticus*, Edentata, possibly a few Carnivora, Proboscidea, sometimes in Cervidae and Giraffidae, and Sirenia (the cleidomastoid was called levator claviculae by Murie). This list is subject to revision, because of

ambiguity in the literature. The muscle may be present in Cetacea, also, but this is difficult to decide.

M. rhomboideus is extremely variable in its extent. The absence of a pars capitis is a character primitive for tetrapods, but it is very doubtful if this statement applies to Mammalia, and there probably has been a secondary regression in those forms in which the sheet does not reach the head. The muscle acts largely in synergy with the trapezius and its details may depend much upon its needs in thus functioning.

Levator scapulae and rhomboideus capitis are both developments, but from different borders, of the serratus anterior; thus their relationship is close. It is well, however, to have a criterion for differentiation. Perhaps the rhomboideus *can* shift to the atlas, but there is no evidence that it does so, and it seems logical to consider that a slip from the upper cervicals or basioccipital to the scapula (or clavicle) is to be considered as levator scapulae, and one from the lambdoid area, rhomboideus capitis.

Few generalities can be offered. Origin is usually upon the midline, at times extending to the lateral lambdoid area, and insertion upon practically the entire vertebral border of the scapula, occasionally (as in Proboscidea, *Phoca*) being restricted to the glenovertebral angle. A single sheet from thorax to head occurs in Marsupialia (not reaching occiput in *Dasyurus*), Tubulidentata, many Rodentia (separable into two parts in others), and most Carnivora (Felidae, Procyonidae, Canidae, Mustelidae, usually Ursidae, seldom in Viverridae, Hyaenidae, with separate occipital slip in Mustelidae (Windle and Parsons)). In Pinnipedia there is a capitis (barely in *Phoca*) plus cervicis separable from a dorsi part. Among Primates, prosimians and monkeys have a pars capitis separable from pars cervicis and pars dorsi, but the first is usually absent in anthropoids and man, and the pars cervicis (minor) may retreat caudally. *Hyrax* has three divisions, dorsi major, dorsi minor, cervicis plus capitis. A single sheet restricted to the thorax occurs in Chiroptera, Dermoptera, blended cervicis and dorsi in Artiodactyla (but to head in *Hippopotamus* and Suidae), while in Perissodactyla pars cervicis may be blended or separable. In Proboscidea separable major and minor are confined to the thorax. The Edentata are extremely variable in all respects. The Cetacea are usually considered to have a small pars dorsi only, but the mastoscapularis of Odontoceti appears more likely to be a specialized rhomboideus capitis than a levator scapulae ventralis.

The slip called rhomboideus profundus by Clark, Dobson, Leche, Macalister, and Meckel (*Chironectes*, *Gymnura*, *Tupaia*, Mustelidae, *Lepus*) seems clearly to be a part of the levator scapulae.

Innervation. The long thoracic nerve supplies the serratus anterior, and short cervical nerves—usually all but the first, except in those with levator element arising well caudalward—the remainder of the complex. Almost always C. 5 supplies m. rhomboideus.

SHOULDER GROUP

Thoracodorsal matrix

M. latissimus dorsi is constantly present in all tetrapods. The extent of origin is variable and not of particular significance whether it arises from spines, ribs, fascia, or all three. There is a tendency in forms needing strong retraction of the forearm for origin to include the iliac crest (in swimming (*Desmana*), climbing (*Sciurus*) flying (*Vampyrus*), and brachiating (great apes)), but the character is not uniform. The different orders may be briefly sum-

marized as follows: Marsupialia, very variable in origin and insertion mostly to teres major, but at times separate, inserting below it (*Dasyurus*) or above (*Didelphis*); Insectivora, variable, to teres major in many, plus a slip to the bicipital groove in *Tupaia*, *Centetes*, with marginal fusion with pectoralis in *Chrysochloris*, and two layers in *Talpa*. When insertion is separate it is situated unusually far distally on humerus; Dermoptera, fusion with teres major; Chiroptera, from spines but not from ribs; Primates, costal origin usually lacking in prosimians and monkeys, and a partly scapular origin normal in man only; Edentata, insertion usually separate; Rodentia, variable, to teres in some but not in others; Carnivora, usually two layers near insertion, with anterior to dorsal panniculus and posterior to teres or humerus, the two parts straddling the biceps; Pinnipedia, in two parts, in *Zalophus* to teres border and to insertion of pectoralis profundus, but both to humerus in *Phoca*; in *Balaenoptera* to teres major, but in Odontoceti usually fused at origin with rhomboid, and partly divisible into two layers, neither to teres; Proboscidea, fibers twist and then split, to insert partly above and partly behind teres; Hyracoidea, splits, one part joining panniculus and the fascia over the biceps, and the other to the teres and a second part of the panniculus; Sirenia, normal to teres; Perissodactyla, poorly, and Artiodactyla, well developed. In the latter order insertion is usually to teres, but often with fibers continuing to pectoralis, with an extra slip of insertion in brocket, sheep, antelope (Windle and Parsons). At times (pig) there is a fibrous tract continues to forearm, or this may be fleshy (some Edentata) (see m. triceps), forming a dorso-epitrochlearis. The fact that the latissimus may straddle the biceps is of interest in connection with my previous discussion of the muscle in Caudata and Reptilia.

M. teres major. There is no distinct slip in lower vertebrates that can be homologous with this muscle in mammals. Hence one must conclude that it has either dropped out of living representatives of the lower classes, or that it was unrepresented in them and has differentiated *de novo* from the latissimus dorsi. The latter appears to be the proper explanation.

The teres major typically arises from the glenovertebral angle of the scapulae, regularly from the lateral aspect when phenomenally developed, when it may encroach on the medial surface and even fuse with the subscapularis, or fuse with the latissimus and triceps longus (Tubulidentata). Occasionally it covers a considerable area (*Talpa*, *Balaenoptera*), or may occupy only the axillary border of the glenovertebral angle (*Zalophus*). In many mammals its area of origin is indicated by the "teres major fossa," as a subdivision of the infraspinous fossa, but this is not a safe criterion, as occasionally (*Zalophus*) this fossa is occupied by the teres minor.

Most of the salient points regarding this muscle are mentioned under the latissimus, but in addition it may be said that the insertion is straddled by the latissimus in Proboscidea; in Cetacea it may be fused with the teres minor (*Globicephala*); and in Rodentia it is almost too variable for generalities, but the insertion is often in front of the latissimus in hystricomorphs, behind in sciuromorphs, and in myomorphs frequently wraps around the latissimus tendon and has a common insertion with it.

Innervation: The latissimus is supplied by the thoracodorsal nerve, and the teres major by one of the lower subscapular nerves. The topography of the nerves of these two muscles and of the subscapu-

laris in Mammalia is so similar that their details tell nothing of interest in regard to their phylogeny.

Axillary matrix

The relationship of the deltoid and teres minor is extremely close, as indicated by their innervation. Primitively they arose as a single sheet from the membranous girdle. When the cleithrum disappeared the part arising from that settled upon the scapula, and the subsequent development of the supra- and infraspinatus displaced the muscle to the axillary border, where it now occurs as the teres minor.

M. *teres minor* has been reported absent in *Gymnura*, *Centetes*, *Talpa*, *Potomogale*, *Phascogale*, some Chiroptera, *Manatus*, Cetacea, and some specimens of *Bradypus* and *Tapirus*. It is true that it may not occur in some mammals, but often it is fused with adjacent muscles, particularly the infraspinatus, and cannot be detected except by its nerve supply. It is certain that in many, if not all, cases its reported absence is erroneus and due to faulty dissection. It may be extremely small, from the neck of the scapula only (Proboscidea, Hyracoidea, *Equus*, *Balaenoptera* reported as subdeltoid), from most of the axillary border of the scapula as usual, spread to originate partly from the scapular spine, or metacromion (*Dasyurus*), occupy the "teres major fossa" (*Zalophus*), or occur as the most hypertrophied muscle of the shoulder and cover the entire infraspinatus (*Tupaia*). Insertion is usually upon the greater tuberosity just distal to that of the infraspinatus, but not infrequently it is partly upon the capsule of the shoulder joint, or it may join the deep tendinous belly of the deltoid (*Phoca*).

M. *deltoideus*, originally from the clavicle only, has spread to acromion and scapular spine in mammals and now occurs either as three separate divisions, cleidodeltoideus, acromiodeltoideus, and spinodeltoideus, or in various combinations of complete or partial fusion. It may be said that the former condition obtains in Chiroptera, Dermoptera, Perissodactyla, and the majority of Insectivora, Carnivora, and Rodentia. The fibers from the clavicle usually insert lowest and those from the spine the highest. Occasionally (as in *Hyrax*) origin may be from the infraspinous fascia, or even the axillary border of the scapula (*Zalophus*, *Tapirus*), rather than from the spine, the scapular part may fuse with the triceps longus (*Sus*), or the medial margin of the clavicular head fuse with the pectoralis major (Chiroptera, prosimians). The cleidodeltoid is said to be absent in *Gymnura*, *Solenodon*, and *Manis*, as it is in *Phoca*; the spinodeltoid absent in *Chrysochloris*, *Tupaia*, *Tarsius*; and acromiodeltoid absent in many Rodentia and most Artiodactyla. The cleidodeltoid occasionally is completely (*Citellus*) or partly double, with one part joining the brachialis tendon (*Viverra*, *Citellus*), or a split insertion may embrace the biceps. Insertion may reach the ulna (*Hyrax*), and fascia of the forearm (*Equus*), join the biceps and so to the radius (*Bradypus*, *Orycteropus*, and many Artiodactyla), join both biceps and brachialis (Felidae, usually Viverridae, sometimes Hyaenidae and Canidae (Windle and Parsons)), or fuse with the brachioradialis (*Manis*, *Zalophus*).

When a cephalohumeral is present it is formed by the fusion of the cleidomastoid and usually cleido-occipital, with the cleidodeltoid, with or without a raphe at the line of junction. When a vestigeal clavicle is present, it is located deep to the muscle complex. The cephalohumeral is said to be present in some Marsupialia (as *Phascolarctos* and *Phalanger*), Edentata (except *Cyclopes* and *Chlamy-*

phorus), Carnivora, Pinnipedia (possibly the deltoid element lacking in Phocidae), Proboscidea, Hyracoidea, Sirenia, Perissodactyla, and Artiodactyla; but undoubtedly there are more exceptions than those mentioned above, and the complex should be sought in all mammals lacking a clavicle. In Perissodactyla the acromiodeltoid also fuses with the levator scapulae ventralis to form the transversohumeralis.

Innervation: The teres minor and deltoid are supplied by n. axillaris.

M. *subscapularis*. At least the more posterior part of the mammalian subscapularis clearly appears to be homologous with the muscle of the same name in Reptilia, and with the short slip of the axillary group in Caudata that I have termed scapulohumeralis. I have been unable, however, to abandon the belief that the anterior part has been derived by an encroachment onto the subscapular surface of a part of the supraspinatus. The two are often fairly continuous, and the subscapular nerves appear always to be at least double. Neither of these points carry much weight by themselves; but according to Dejerine there are two separated subscapular motor nuclei in the spinal cord, which, so far as I know, can be claimed for no muscle without dual derivation. Conditons in *Tatusia* (and to a lesser extent in some other mammals dissected) are extremely suggestive of this conclusion, for in this there are two parts of the subscapularis, the anterior being innervated by a nerve close to n. suprascapularis. Hence I regard it as likely, but as yet by no means certain, that the subscapularis has been formed by the union of two elements, a theory to which I can see no valid obstacle.

The subscapularis usually occupies practically the whole subscapular surface, although its area may be less (Hyracoidea, some Artiodactyla), or the muscle may be greatly hypertrophied (Chiroptera, Dermoptera, moles). Perhaps usually it is definitely multipennated, and occasionally this is carried to an extreme where the muscle is separable into a number of divisions (many Carnivora, Artiodactyla, Odontoceti). The occurrence of a subscapularis accessorius or episubscapularis is not unusual (some monkeys, horse (Testut), pig, cat, *Zalophus*, suggested in *Bradypus* and *Myrmecophaga*), and this character may possibly reflect the line of junction of the two possible muscle units contributing. Insertion is always upon the lesser tuberosity, although it may spread to the capsule of the joint.

Innervation: By two or more nn. subscapulares. The close association of these with the branches to teres major and latissimus probably has no greater significance than that all are of axillary group derivation.

ELBOW GROUP

This consists of the triceps complex, including dorso-epitrochlearis and anconeus. The derivation of the dorso-epitrochlearis and triceps longus is not entirely clear. There is considerable evidence (Amphibia) to suggest that originally fibers of the latissimus migrated down the arm to form a slip with triceps action. If this slip survived as a contributor to the triceps mass, then its innervation shifted to allocation with n. radialis in response to the law of fasciculation. At a later date (Mammalia) this process may have been repeated, to form the dorso-epitrochlearis. If this assumption be false, then the latter muscle must be merely a displaced slip of the triceps proper, a premise which I am loath to accept. Less doubt is attached to the history of the caput longum tricipitis. Clearly there was urgent need for a long triceps, as there was for a biceps, and both have been formed,

mainly if not exclusively, by migration to the girdle along a plane of fascia by slips of the original elbow matrices.

M. *dorso-epitrochlearis* is absent as a distinct muscle in Chiroptera, Cetacea, Sirenia, antelopes, man, and individually in the anthropoids. Sometimes it is represented merely by tough fascia. Normally it arises from the tendon of the latissimus and disappears over the elbow. Not infrequently it occurs in two, or occasionally more, parts, and then there is a question whether a particular slip may not be a subdivision of the triceps. In rare instances it may arise only from the axillary border of the scapula, but in the great majority of cases this shift is not complete, and origin is both from the bone (or teres major, some Insectivora, Carnivora, and pig; or infraspinatus, some Artiodactyla) and the latissimus (Perissodactyla, Proboscidea, Tubulidentata). Occasionally origin is partly from the panniculus (Proboscidea, *Galera*).

In man, which normally lacks a dorso-epitrochlearis, panniculus carnosus, and pectoralis abdominalis, there is not infrequently encountered an axillary sling or achselbogen muscle, which should be considered as a remnant of one or more of the above muscles, in various combination.

M. *triceps longus* is invariably present in Mammalia, extending from the axillary border of the scapula to the olecranon. Occasionally it arises from the entire length of the border (some Marsupialia, Viverridae, some Artiodactyla), but usually it is more restricted. It may be two-headed (*Thylacinus*, some Edentata), or occur in even three or four parts (Tubulidentata, *Tapirus*). In *Tatusia* a part of the origin has left the girdle and now arises from the dorsum. A similar arrangement in some mammals seems to have been mistaken for a part of the latissimus dorsi. An interesting condition obtains in Pinnipedia; in *Zalophus* origin is from the ridge between the infraspinous and teres major fossae, and thence to the glenovertebral angle; while in *Phoca* the origin of this part is similar, but with extension to the dorsal base of spina scapulae. In this genus there is a second head from the scapular neck.

Mm. *triceps lateralis et medialis* are usually distinct, occasionally with a third division, but they need scant mention in the present connection.

Mm. *anconei* should be included for the sake of completeness. They constitute distal extensions respectively of the lateral and medial triceps. One or both may be absent.

Innervation: By n. radialis. The dorso-epitrochlearis has been reported as innervated by n. thoracodorsalis but I have never encountered this condition and am unconvinced that this occurs.

Ventral (flexor) division

Infrazonal matrix

This matrix seems to be entirely unrepresented in Amphibia, but appears as a variable number of slips in living (podial) Reptilia, passing from the sternum and ribs to the coracoids. A comparable situation obtains in Mammalia, but it is difficult to state the basic arrangement of the complex, for all sorts of poorly defined combinations occur. The conoid and trapezoid ligaments are generally believed to be remnants of muscle slips of this group (Huntington). In mammals it is perhaps correct to consider that there are two divisions present, one from the sternum or ribs to the clavicle (subclavius), and the other with a similar origin but inserting upon the girdle—coracoid or acromion usually, farther dorsally occasionally—(sternoscapularis). Both will be considered together, for in many cases they are hardly separable.

Mm. subclavius et costoscapularis. Both divisions appear to be unrepresented in Cetacea, Sirenia, Pinnipedia, some Carnivora, camels, giraffes, and some Tragulidae, Cervidae, and Bovidae. The subclavius has been reported as absent in Hyracoidea, Myrmecophagidae and Manidae, and the costoscapularis as lacking in Chiroptera and some Insectivora; but this list is unreliable. In Rodentia the subclavius is present and the costoscapularis variable, in form and definition. The feature is also variable in Marsupialia, Edentata, and the more specialized Insectivora. Perhaps the most interesting condition is the extension to the supraspinatus fascia in *Didelphis*, *Thylacinus*, *Chrysochloris*, *Orycteropus*, *Perissodactyla*, and undoubtedly others.

Innervation: N. subclavius.

SHOULDER GROUP

Pectoral matrix

This consists of the muscles derived from the primitive m. pectoralis, and comprise the following three divisions.

M. panniculus carnosus is far too intricately variable for concise description here. Almost always, notably with the exception of the higher Primates, and some platyrrhines, as *Ateles* and *Lagothrix*, some part is present, usually a dorsal sheet, and often a ventral one in addition. The typical insertion is upon the humerus distal to the greater tuberosity. The dorsal part is best developed in *Erinaceus* and armadillos (and Monotremata). In many animals it extends onto the base of the tail. Often it has marginal fusion with the platysma, a fact that led the older anatomists to the erroneous conclusion that the two fields were phylogenetically related. In addition there may be marginal fusion with the latissimus and pectoralis major, as in *Orycteropus*, or with the pectoralis minor (*Hyrax*). In bats, flying lemurs and flying squirrels the musculature of the membranes is derived from the panniculus, and this element furnishes slips to control the marsupium in Marsupialia, of the prepuce in some Artiodactyla, and to compress the mammae in Cetacea. Small areas may become separated from the remainder, as over the hips in *Pedetes*. Occasionally the muscle occurs in two sheets (more in monotremes), as in *Hyrax*, and then separate insertions may bridge the biceps. Insertion may be partially or wholly upon the latissimus tendon (*Phalangista*, Hyracoidea, Artiodactyla), or occasionally extend to the fascia of the forearm (Pinnipedia); or it may even extend over the shoulder to the malar region (*Manatus*).

Mm. pectorales minor et abdominalis may be considered a muscle sheet that often separates into a part from the sternum or sternal ribs, and a part from the abdomen. The latter typically arises from the region of the umbilicus and is opposed by m. pyramidalis (Marsupialia) for the support of the abdomen, with much subsequent variation. As in the case of the panniculus, the pectorals vary too much for detailed description in the present contribution. In addition, there is no ready criterion for distinguishing pectoralis minor from major elements, and most authors reporting their dissections have not realized the distinctiveness of the two groups. Consequently descriptions usually are difficult or impossible of interpretation in this regard and generalities are unsafe.

It is probable that the true pectoral, or pectoralis minor, layer is represented in all mammals, although the costosternal part may have retreated and only the abdominal part exists in a number of mammals (some Edentata and Artiodactyla). There may be marginal fusion with the obliquus externus (Manidae). It is certain that frequently there are two layers.

Insertion is primarily upon the proximal humerus, but in perhaps most mammals the attachment has retreated to the capsule of the joint, either or both tuberosities (as in *Equus*), often the coracoid, or even the supraspinatus fascia (*Equus*).

Innervation: By fibers of the anterior thoracic nerve that have fused with those to the pectoralis major.

Anterior coracoid matrix

This consists of the muscles that have been derived from the original coracohumeral group, arising from the procoracoid. There is little or no doubt, I think, that the change in the position of the humerus, incident to the alteration from the reptilian to the mammalian posture, was the stimulus that started a part of this matrix in its dorsal migration that resulted in its becoming the supra- and infraspinati. The pathway was clearly to the ventral part of the cranial border of the scapula, a part of the muscle passing upon either side of the clavicular articulation, anterior to which the supraspinous fossa developed. The extreme cranial border of the muscle could well have encroached slightly upon the subscapular fossa, and I cannot help but believe that it did so, eventually becoming the more cranial part of m. subscapularis. This is but a tentative hypothesis, however, for satisfactory proof is as yet lacking.

M. *pectoralis major*. The more ventral part of the procoracoid musculature shifted its origin to the sternum and became the pectoralis major, in mammals over-riding the true pectoral or pectoralis minor stratum. The difficulty of precisely allocating all pectoral slips to the proper one of the two components has been mentioned. Where one of the elements is lacking it is more likely to be the major. This appears to be the case in Mysticeti, and some Odontoceti but not in others. It is variable in extent and number of slips, but encroaches on the clavicle in relatively few mammals, as in some Marsupialia, some Edentata and Insectivora, *Pteropus*, and the higher Primates except the orang. In *Potomogale* this part is represented but arises from the cleidomastoid. Insertion typically is on the pectoral crest of the proximal humerus, but it may shift somewhat proximally, partly to the deltoid and biceps (Tubulidentata), or wander to the fascia of the forearm (Hyracoidea, Sirenia, Perissodactyla, Artiodactyla). Where the latter condition obtains it is probably, but not certainly, the major rather than the minor division that is involved.

Innervation: By a part of the fibers of the anterior thoracic nerve.

M. *supraspinatus*. Usually the size of this muscle may be inferred from the appearance of the supraspinous fossae, with a few exceptions, as the Cetacea, in which the muscle is larger than the fossa. It is present in all mammals. Not infrequently (*Civetta*, Pinnipedia, Hyracoidea, Perissodactyla, Artiodactyla) it is partially or completely double, in which case the smaller division usually arises from the anterior surface of the scapular spine. The insertion regularly is upon the greater tuberosity, but some of the fibers may wander to the pectoralis minor (*Choeropsis*, *Sus*), or to the lesser tuberosity. The latter condition is marked in forms in which lateral movement or rotation of the limb is restricted and the spinati muscles are employed almost exclusively for protraction (Hyracoidea and ungulates), although the condition is suggested in *Zalophus*.

M. *infraspinatus*. Present in all mammals so far as known except *Talpa* and *Mogera* (Campbell, MS), although reported in the former erroneously. The

variability, except in size, is usually negligible, and insertion is invariably upon the greater tuberosity.

Worth mentioning in this connection are rare slips, separable near the axillary border (*Capra*), and m. intraspinatus, between the base of the spine and the greater tuberosity (*Macaca*). These appear to be inconstant even specifically.

Innervation: Mm. supra- and infraspinati are supplied by n. suprascapularis.

The possibility of a part of m. subscapularis having been derived from this matrix has already been discussed.

Posterior coracoid matrix

This comprises the coracobrachial division, arising from the true coracoid. In mammals three parts may be recognized, superficialis, medius, and profundus. In some Reptilia (*Iguana*) there are two main divisions, separated by the flexor nerve, each of which is subdivisible into two parts. Conditions suggest that in mammals the superficialis and the part of the medius in man that is superficial to the musculocutaneous nerve, are homologous to the superficial slips in *Iguana*, while the part of the medius deep to the nerve and the profundus in mammals are matched by the deep slips in the reptile. This is a tempting explanation, but because the piercing of a muscle by a nerve is so easily accomplished by a gradual migration of fibers around the nerve trunk via a fascial plane, and the condition of piercing is encountered regularly only in man, it is wiser not to try to homologize the individual slips too closely. In the mammals so far dissected some part of the coracobrachialis is present in all forms except *Gymnura*, Talpidae, *Cyclopes*, Manidae, many Mustelidae, *Ictonyx*, Pinnipedia, and *Viscaccia*.

M. *coracobrachialis superficialis*. This muscle presents a difficulty. Very many times its presence is recorded in the literature under the name pars longa, but almost always it is an unusually long pars media that is meant. The criterion is that the pars superficialis, when present, always passes from coracoid to condyle superficial to all vessels and nerves (except the cutaneous ones) and all other muscles except the dorso-epitrochlearis. On these points the descriptions seldom are explicit. It appears to have been encountered in *Galera*, some rodents, some Artiodactyla, and occurs as an anomaly in *Pan*.

M. *coracobrachialis medius* is often reported as pars longa (superficialis), so is more frequently present than the literature indicates. It arises from the coracoid and inserts below the tendon of the latissimus for a variable distance, not infrequently practically to the condyle. Besides those mammals lacking all divisions, the medius appears to be absent in most Carnivora, some Edentata, and perhaps Cetacea, in which it is no more than vestigial at the most. It is variable in Marsupialia and Rodentia, being absent in some and present in others.

M. *coracobrachialis profundus* (brevis) is frequently overlooked by inexperienced dissectors. It arises from the coracoid and inserts always above the tendon of the latissimus, so is deep and very short. Besides those mammals lacking any division, it is supposed to be absent in a number of Marsupialia, most Insectivora, Chiroptera, anthropoid apes and man, many Edentata, various scattered Rodentia, perhaps a few Cetacea, Tubulidentata, Proboscidea, Hyracoidea, Sirenia, individual Perissodactyla, and some Artiodactyla. It never fuses with pars media, at least at insertion.

Innervation: By n. musculocutaneous, or at times by a separate coracobrachial branch of the common flexor nerve.

BRACHIO-ANTEBRACHIAL MATRIX

Brachial group

M. brachialis. In basic condition this muscle appears to have been at least partially divisible into an extensive lateral head, and a more restricted medial one, going respectively to radius and ulna. The former arose, and usually still does so, from the back of the surgical neck of the humerus and spirals around the bone. The medial head usually arises just distal to the deltoid insertion, or not infrequently its origin forms a V with the lateral head, the two partly embracing the deltoid.

Aside from its possible absence in some Cetacea, the brachialis appears to be uniformly present in all mammals. The medial head is often much reduced, but it is difficult to decide whether it is absent, as is supposed to be the case in ungulates. The lateral head usually retains its high origin. The typical insertion in mammals is upon the ulna, but at times it goes partly (some Marsupialia) or entirely (some Insectivora (as *Erinaceus*), Sirenia) to the radius, or in some orders (Edentata, Perissodactyla, Artiodactyla), insertion is upon the radius in some cases, the ulna in others, or even both (*Bradypus*, *Tapirus*, *Choeropsis*, *Sus*). The insertion as stated in the literature, however, is not always trustworthy, because of faulty dissection. Not infrequently in Primates there is partial fusion distally with the brachio-radialis, and the innervation of a small part of the brachialis by the radial nerve, to be encountered in this order, should be explained on the premise that some of the fibers of the brachioradialis have become incorporated with the muscle.

M. biceps brachii. This muscle is one of great importance in tetrapod economy, and different classes have had considerable difficulty in evolving it. In Mammalia, and lacertilians, it appears without question to have been derived by a division of the brachialis which reached the shoulder by migrating along a fascial plane. There it may have fused with a slip of the coracobrachialis, as it did in *Iguana*, so it is possible that the biceps, muscle and tendon, is a product of both divisions, although the muscular part of the mammalian biceps was derived from the brachialis only.

The condition of the biceps that was primitive for mammals appears to have comprised two separable muscles, a coracoradial and glenoulnar, as is commonly encountered in Marsupialia. These may fuse, and one head or one insertion be suppressed, thus accounting for the variation existing in mammals. It is generally considered that when but one head occurs it is always the longum, this argument being based on the fact that the tendon of the remaining head usually pierces the capsule of the joint; but it does not always do so, and the argument is weak. Many times two heads have a continuous origin, or a single head may arise from the base of the coracoid or the glenoid border. Where but one occurs it usually means that both divisions have fused, arising from one spot or another as best suits function. If this be from the glenoid border, the curve of the tendon over the joint usually results in its partial piercing of the capsule.

Similarly, one or the other of the insertions may disappear, or one (usually the ulnar) shift from the bone to the intermuscular septum, finally reaching the surface of the forearm to become a lacertus fibrosus (some primates, some ungulates) or a lacertus carnosus (gibbons).

A single biceps is encountered in a few Edentata, most Carnivora (except Ursi-

			NECTURUS	RANA	IGUANA	ORNITHORHYNCHUS	MAMMALIA
DORSAL (EXTENSOR) DIVISION	Branchiomeric mm.		trapezius	trapezius	trapezius sternocleidomast.	trapezius (2) mesocleidomast. cleidomast.	trapezius cleido-occip. sternocleidomast.
	Suprazonal group		serrati levator scap.	serrati rhomboid. levator scap.	serrati scap. levator scap.	serrati scap. rhomboideus levat. scap. dors. levat. scap. vent.	serrat. anter. rhomboidei levat. scap. dors. levat. scap. vent.
	Shoulder derivs.	thoracodors. matrix	latissimus	latissimus	latissimus	latissimus teres major	latissimus teres major
		axillary matrix	dorsalis scap. proscap. hum. long. proscap. hum. brev. scapulohum. brev.	dorsalis scap. delt. superf. delt. prof. scapulohum. brev.	dorsalis scapulae deltoideus proscap. hum. subscap. subcoracoid.	teres minor scap. deltoid proscap. hum. clav. deltoid subscap.	teres minor deltoideus absent absent post. subscap.
	Elbow derivs.		dorsitriceps coracotriceps humerotriceps	scap. triceps humerotriceps	scap. triceps corac. triceps humerotriceps	dorso-epitroch. scap. triceps humerotriceps	dorso-epitroch. triceps longus humerotriceps triceps anconeus
VENTRAL (FLEXOR) DIVISION	Infrazonal group		absent	absent	costoscap. sternocor. super. sternocor. infer.	costocoracoid. sternocoracoid.	subclavius costoscap.
	Shoulder derivs.	pectoral matrix	pectoralis	pectoralis	pectoralis	pannic. carnosus pectoralis	pannic. carnosus pect. abdom. pect. minor
		anter. cor. matrix	coracohum. major coracohum. minor	coracoradial cor. hum. epistern. cor. hum., cor. acrom.	coracohum.	coracohum. superf. coracohum. interm. coracohum. prof.	pect. major infraspinatus supraspinatus anter. subscap. (?)
		post. cor. matrix	coracobrach. glenoantebrach.	coracobrach. (3)	coracobrach. superf. coracobrach. prof.	coracobrach. superf. coracobrach. prof.	coracobrach. superf. coracobrach. medius coracobrach. prof.
	Elbow derivs.		brachialis	caput super. flex. carpi rad.	brachialis biceps	brachialis biceps (2)	brachialis biceps
	Hypobranchial mm.		hypobranch.	hypobranch.	hypobranch.	omohyoideus	omohyoideus

dae and scattered genera), Pinnipedia, Tubulidentata, Proboscidea (individual exceptions), Hyracoidea, Sirenia, Perissodactyla, Artiodactyla, and some Lorisidae. Insertions in these are about equally divided between radius, ulna, and both. Orders that are particularly variable in all respects are Insectivora, and Rodentia (usually, however, a glenoulnar division is present). Not infrequently an additional humeral or capsular head occurs, as seems to be usual in *Bradypus* and as a variant in others, and a humeral head replaces caput breve in the gibbons. In the Talpidae the muscle is specialized in bizarre fashion, passing over trochleae so as to occur in three planes.

Innervation: The brachialis and biceps are supplied by n. musculocutaneous, and the former may receive fibers of n. radialis in addition, as already explained.

For the convenience of the reader a summary of the muscle homologies in the tetrapods discussed in the several parts of the present contribution is given in the table on page 461.

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ESSAYS ON EVOLUTION

I. ON THE EFFECTS OF SELECTION ON MUTATION RATE

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NEW recessive lethal mutations arise constantly without any artificial treatment. The results of Muller (1928) and others summarized by Schultz (1936) indicate, for *Drosophila melanogaster*, a frequency of about 0.18 per 100 gametes in the X chromosome, and of about 0.5 per 100 gametes in the second chromosome. It is probable that the third chromosome gives about the same frequency as the second; we may conclude that the normal frequency, for all chromosomes taken together, is about 1 per 100 gametes. Timoféeff-Ressovsky (1934) shows that there is also a high frequency of new mutant genes that cause an appreciable reduction in viability. In the X-rayed X chromosomes that he studied, such viability mutants were from 2 to 2½ times as frequent as were lethals. There is also a class of mutations that are lethal or reduce viability even when heterozygous. It is difficult to judge the frequency of the dominant lethals, but there is no doubt that they are present. Perhaps the largest class of dominant genes that lower viability is that of the minute-bristled types, known to be rather frequent. Finally, there is another type of mutation, as yet little studied, that brings about sterility (Berg, 1937).

In general, therefore, one may conclude that each normal, untreated individual of *D. melanogaster* is subject to a lowering of its potential reproductive value through the occurrence of new mutations in its germ tract. Quantitative estimates of this reduction must be approximate, since the recessive autosomal lethals and viability and sterility mutants will have an unpredictable effect. This arises from the fact that they operate only when homozygous, and the frequency with which they become homozygous will depend on the degree of inbreeding—a factor not yet evaluated for wild populations of *Drosophila* or any comparable organism. It is also to be observed that these recessives will operate only after recombination has reduced the correlation of their distribution in the population with that of the other genes present in the individual in which the original mutation occurred.

It seems probable, however, that the loss in potential reproductive value for the average individual is not far from one per cent, taking into account not only the immediate offspring but also later generations.

A body of data has been gradually accumulating that shows that different strains may deviate significantly from the usual values for mutation rate. This was indicated by Muller (1928), and has more recently been shown by Dubrolskij (1935) for material treated with X-rays and with heat, and by Demerec (1937) for untreated

material. Suggestions of similar differences may be found in still other papers (see Schultz, 1936). It appears that, for the X chromosome at least, some strains give lethal mutation rates as much as five or six times as high as those shown by other strains. Demerec reports that in the strains studied by him the chief difference was due to something carried by the second chromosome, though the new lethals studied were themselves in the X.

There are, then, genes that affect general mutation rate, and stocks differ in their constitution with respect to such genes. It follows that, in wild populations, such genes must be subject to selection. If a population has the mutation rate arrived at above, which leads to an average of 1 per cent decrease in the potential reproductive value of each individual, then a gene causing a 10 per cent decrease in the general mutation rate will have a selective advantage of 1/1000. The data suggest that decreases even greater than 10 per cent may be found; but a selective advantage of 1/1000 is sufficient to be effective (Haldane, 1924).

Any external agents that increase the general mutation rate will evidently increase the average loss in potential reproductive value, and will therefore increase the intensity of the selection for genes that lower the rate. Such selection will always be present, but it must be more intense in regions with high natural ionization, and also in regions with high temperatures. (For a critical review of the effects of temperature on mutation rate see Timoféeff-Ressovsky (1934a).) It may, therefore, be expected that, as a general rule, strains found in regions of high ionization and high temperature will have more uniformly low mutation rates (under standard conditions) than will strains found in regions of low ionization and low temperature. These relations will probably be difficult to verify experimentally; both because the sign of the selective process is the same in both situations, and because ionization intensity commonly shows such great local differences that it will be difficult to judge the intensity to which a population has been subjected.

It is probable that different strains will have accumulated different genes lowering the mutation rate, and that most strains (in cross-fertilizing organisms) will be somewhat heterogeneous for genes affecting that rate. If this be so, then any interference with the operation of the usual selection for low rate will have the effect of increasing the rate. The situation may be compared with that which obtains with respect to genes affecting the general viability of the organism. Under natural conditions selection keeps the members of a population up to par, but on relaxation of selection a slow deterioration sets in. Such deterioration is, as is well known, greatly speeded up by inbreeding. If the analogy here is valid, it may be concluded that any artificial conditions, maintained over a number of generations, are likely to raise the general mutation rate of a strain. Are all mutation rates so far determined experimentally too high to be applicable to natural populations?

The method of action of genes affecting the general mutation rate is still unknown. Probably they act in various ways; but it remains possible that they have something in common, such for example as an influence on the relative duration in time of specific stages of mitosis, or some other general property. In this case it might well be that such effects would have greater selective value through other results than through their effects on mutation rate. While this possibility must be kept open until more is known about the way in which the genes in question affect

mutation rate, it seems unlikely that it will invalidate the conclusions derived in this paper.

It seems at first glance that there should be a counter-selection, due to the occurrence of favorable mutations. It is true that favorable mutations furnish the only basis for improvement of the race, and must be credited with being the only raw material for evolution. It would evidently be fatal for a species, in the long run, if its mutation rate fell to zero, for adjustment to changing conditions would then not long remain possible. While this effect may occur, it is difficult to imagine its operation. It is clear that the vast majority of mutations are unfavorable, and Fisher (1930) has shown that the rare favorable ones must, in general, be genes with slight effects. In other words, for every favorable mutation, the preservation of which will tend to increase the number of genes in the population that raises the mutation rate, there are hundreds of unfavorable mutations that will tend to lower it. Further, the unfavorable mutations are mostly highly unfavorable, and will be more effective in influencing the rate than will the relatively slight improvements that can be attributed to the rare favorable mutations.

This raises the question—why does the mutation rate not become reduced to zero? No answer seems possible at present, other than the surmise that the nature of genes does not permit such a reduction. In short, mutations are accidents, and accidents will happen.

It may be noted that differences in general mutation rate may be characteristic of whole species. It has been the experience of all who have studied *Drosophila funebris* that it gives many fewer mutations than does *D. melanogaster* (see, for example, Timoféeff-Ressovsky, 1936). It has been suggested that this difference is due to failure of the same mutant gene to produce as distinct and easily seen a character in *funebris* as it does in *melanogaster*. My own experience leads me to disagree with this conclusion, since I found four types in *funebris* similar to known ones of *melanogaster*, and these four were at least as extreme and easily classifiable as the corresponding *melanogaster* types. How it has happened that *funebris* has come to have a lower mutation rate than *melanogaster* is not clear; but further analysis of this case may throw light on some of the questions raised in this paper. Possibly one might obtain more mutations by first crossing widely different races and then subjecting the hybrid lines to close inbreeding.

There are several characteristics of the organism that will influence the effectiveness of the selective process discussed in this paper. As in other types of selection, change will be more rapid in cases where selection can operate on haploid individuals, since here the large class of recessive mutations will be selected at once instead of only in the homozygous individuals. For this reason, the action of the suggested mechanism should be, in general, most marked in plants (where the gametophyte generation is haploid), and in animals, such as the Hymenoptera, with haploid males. In many animals and some plants there is one chromosome that is effectively (or actually) haploid in one sex. The number of genes that are carried by such a chromosome should show a positive correlation with the effectiveness of selection for low mutation rate.

Another general property that should influence this type of selection is the degree of inbreeding, since this will influence the rapidity with which unfavorable recessives are eliminated; and the sooner they are eliminated the greater will be the correlation between their elimination and

the presence of the genes that stimulated the original mutation.

In polyploid organisms the elimination of unfavorable recessives will be prevented or greatly hindered, and in polyploid plants the gametophytic elimination of unfavorable types will be greatly reduced; this condition should therefore raise the mutation rate. However, polyploidy also makes the detection of mutations more difficult, so experimental determination of this relation can hardly be expected.

One may surmise, then, that minimum mutation rates may be expected in diploid self-fertilizing plants (excluding balanced lethal types, such as *Oenothera*, which constitute a special case), and in lower plants, where the haploid generation is well developed. Maximum rates (per individual, not per unit of time) may be expected in cross-fertilizing animals that have no sex-chromosome mechanism or in which the sex chromosomes are relatively small. By way of examples, the rate should be low in Bryophytes and in legumes, high in Lepidoptera and in mammals—to cite groups that furnish familiar objects for genetic study.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to Dr. Raymond Pearl, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *1901 East Madison Street, Baltimore, Maryland, U. S. A.*

BRIEF NOTICES

EVOLUTION

IRRTUM UND WAHRHEIT DER BIOLOGIE. *Kritik der Abstammungslehre.*

By Friedrich Andermann. Verlag für Medizin, Weidmann and Co., Vienna, Leipzig and Bern. Rm. 12. (paper); RM. 14. (cloth). $9\frac{1}{4} \times 6\frac{1}{4}$; 282; 1937.

This is a work in the philosophy of biology, approaching the subject not with modern instruments of logical analysis and from the modern point of view of the Vienna Circle, so ably developed by Carnap, formerly of Vienna, and his associates, and by Ayer in England, nor from that of the English philosophers of science headed by Whitehead and Russell, but rather from the older idealistic standpoint, now happily falling into disrepute. Compared with the penetrating and fecund productions of Woodger, for instance, it stands in sorry contrast.

The particular biological topic it considers is the theory of evolution, which it attempts to refute as a whole. It is not concerned with one or another evolutionary theory, but claims to demolish the very notion of descent. Not only does it maintain that the concept of evolution is untenable in itself, but also that the theory of descent is incompatible with the erection of a systematic science of zoology and botany. Nevertheless one must not infer from this that Andermann favors a return to a religious explanation of the origin of species. He believes in the fixity of species, but not in religious fundamentalism.

Andermann modestly credits Constantin Brunner for the ideas herein expressed, and merely wishes to apply these ideas to the domain of biology. But to do so, the author finds it necessary to step beyond the boundaries of this domain and to soar in the dizzy atmosphere of metaphysical speculation.

The origin of species is especially subject to attack, and in particular the author rejects the historico-chronological approach of the biological scientist. The theory of evolution, he believes, contradicts the facts of experience and outrages reason. Only by misinterpreting the facts is the theory apparently based on comparative anatomy, embryology, and paleontology. Indeed, as far as experience and observation can tell, species are essentially constant. Species are fixed, with definite lines of demarcation between them, and every variation occurs within the species themselves. One does not merge or develop into another. Their existence rests upon a "super-individual cosmic reality" in which they are recognized as being manifestations of One unchangeable Being in which all appearances are rooted. That there exists such a great similarity of structure and function among species of animals (and plants) is because together they "constitute a basic element of Nature, the primeval phenomenon of Life."

This phenomenon expresses itself in countless variations of species all of which, however, are on an equal plane and are of equal value. Species can be arranged in order of complexity, but not in order of precedence, importance, or value. Nature, in short, has no favorites.

It is, of course, not entirely fair to treat an argumentative work such as this one in a few words, and to tear the more purple passages of philosophical fantasy from their context for the edification of readers of this review. Nevertheless, these passages are enlightening in that they clearly display the point of view of the author and the futility of his endeavor.

PALAEONTOLOGY. *Invertebrate. Seventh Edition.*

By Henry Woods. The University Press, Cambridge; The Macmillan Co., New York. \$3.25. $7\frac{1}{4} \times 4\frac{3}{4}$; 475; 1937.

The classical work on paleontology is of course Zittel's textbook from which the present work differs in being smaller, and therefore cheaper and more portable. Also, it has been ten years since the last edition of Zittel appeared, so that Woods' work is more up to date, yet the taxonomy of the mollusca given by Woods is not so modern as that of Zittel.

On the other hand the introductory material in Woods is better expressed than that in Zittel, the discussion of convergence being particularly good. The genus *Gryphaea*, for instance, has been evolved separately three times, once in the Lias, once in the Oölite, and once in the Cretaceous, but there is no genetic connection among these stocks, as the strata in which they occur are separated by other strata in which they do not. It is a question, however, whether we should consider this as an example of real convergence, since each stock has been derived from the same origin, the genus *Ostrea*, which has existed from the lower Mesozoic to the present substantially unchanged. If *Gryphaea* had been derived from different origins, such for instance as *Aetheria*, *Chama*, or *Rudistes*, all of which bear a superficial resemblance to *Ostrea*, such a type of convergence would constitute a true involution, but there is no evidence that any natural group of species has ever had a polyphyletic origin. There is no apparent reason why the property possessed by one genus of evolving another should not be perpetuated through paleozoic periods like other generic characteristics, and forms so derived may appropriately be considered congeneric.

The index covers twenty-two and a quarter pages.

PALEOCENE FAUNAS OF THE SAN JUAN BASIN, NEW MEXICO. *Transactions of the American Philosophical Society held at Philadelphia for Promoting Useful Knowledge. New Series, Volume XXX.*

By William D. Matthew. American Philosophical Society, Philadelphia. Distributed by University of Pennsylvania Press, Philadelphia. \$5.00. $12 \times 9\frac{1}{4}$; viii + 510 + 65 plates; 1937 (paper).

The San Juan Basin of New Mexico lies largely in the northwestern corner of that state, a small section extending over into Southern Colorado. The faunas, first reported by Cope in 1880, belong to the two earliest phases of the Paleocene epoch, a transitional period following that of the great reptiles of the Mesozoic era, and preceding the Lower Eocene phase when the direct ancestors of the horses, tapirs, ruminants, and other progressive placental families first became established in western North America. The present report, an important contribution to palaeontological literature, has been many years in the making (it was commenced in 1916) but one can scarcely find fault on this score in view of the high quality of the work and its completeness in every detail.

Two very distinct faunas are recognized: the Puerco (17 genera representing 7 families and 6 orders), and the Torrejon (26 genera representing 14 families and 7 orders). No species (of the two families) passes through from one to the other unchanged; most of the genera are different, and in those genera that pass through (*Periptychus*, *Anisonchus*, *Ellipsodon*), the representative species in each horizon are very distinct, those of the Torrejon in all cases much more specialized. The genera of certain families, closely allied and primitive in the Puerco, are decidedly more specialized and divergent in the Torrejon.

The general character of the vertebrae, limbs, and feet are suggestive throughout of animals either of arboreal habits or derived from arboreal ancestry.

The volume includes a chronological list of publications dealing with Paleocene faunas of the San Juan Basin; a group of 65 plates giving many very fine illustrations of the material studied and a map of the region; and an index. It is unfortunate that the death of the author in 1930 prevented his seeing the completed *opus*, the publication of which the American Philosophical Society generously supported.

Evolution Out of Doors. *A Study of Sex Differences and Animal Coloration.*
By Henry J. C. Molony. Edited and with an appendix by J. R. de la H. Marett. Photographs by Cherry and John Kearton. Hutchinson's Scientific and Technical Publication, London. 15s. net. $8\frac{1}{2} \times 5\frac{1}{2}$; xx + 248 + 30 photographs; 1937.

The author considers evolution from four aspects, as follows:

Process A is the failure *as individuals* that are born with disadvantageous variations, or the elimination of the unfit. It is responsible for both concealing and bold colour patterns, and for an inherited immunity from many diseases. Process B is the failure *as parents* of individuals that are born with disadvantageous variations. Parents of certain types fail to leave the normal number of representatives in later generations and these types gradually die out. Process C is the failure *as mates* of individuals that are born with disadvantageous variations. It has produced the bright colours in flowers that are fertilised by pollen-bearing insects. Of course, the success of individuals born with advantageous variations could rightly be described as a case of natural selection; though how far this contributes towards evolution is doubtful. Albino and melanistic forms occur from time to time in most, if not all, species, both in the wild state and in domestication. In nature it usually happens that they disappear without influencing the race, but occasionally the change of colour seems to supply a long-felt want, and the new form flourishes and increases.

A very large number of facts, many of them gathered from the author's own experience, are drawn upon to support the theories that are advanced. The full page photographs are exceptionally good ones. There is a short bibliography, but no index.

GENETICS

The Evolution of the Australian Merino.
By E. W. Cox. Angus and Robertson, Sydney, New South Wales. 21s. $10 \times 7\frac{1}{4}$; xxii + 160 + 31 plates; 1936.

An interesting book. In 1765 Spain first permitted exportation of the Merino. About thirty years later, a few of these remarkable sheep found their way to New South Wales and were bred with existing flocks. Early in the nineteenth century when it became apparent that the climate and grazing conditions of this continent were not only suitable to the well-being of the Merino but that the high quality and quantity of its wool did not deteriorate, a systematic effort was begun to bring to Australia the best of the various Merino strains for breeding purposes. This is the story of the successes and failures which have produced the Australian Merino of today—a type entirely different from the Merino in any other land and quite unsurpassed both in the quantity and quality of its wool. The numerous illustrations (entirely of sheep) show all the great Australian rams and the different evolutionary stages of the present day animal. The volume has an index.

Erbpathologie. *Ein Lehrbuch für Ärzte.*
By O. F. von Verschuer. Verlag von Theodor Steinkopff, Dresden and Leipzig. RM. 8. (paper); RM. 9.20 (cloth). $9\frac{1}{4} \times 6\frac{1}{4}$; vi + 213; 1934.

This book by an eminent geneticist should find a niche in the medical practitioner's library. About half the book is devoted to a concise discussion of the essential principles of human genetics. The second half presents short notations on the frequency of occurrence, mode of inheritance, records from the literature, and other pertinent matter of a long list of diseases and anomalies. These are arranged in a handy manner, alphabetically under headings such as nervous diseases, diseases of the inner organs, deformities. Included also is a brief treatment of blood groups, finger prints and other tests utilized in attempts to establish paternity. A bibliography and an index have been provided.

EXPERIMENTELLE MUTATIONSFORSCHUNG IN DER VERERBUNGSLEHRE. *Beeinflussung der Erbanlagen durch Strahlung und andere Faktoren.*
By N. W. Timoféeff-Ressovsky. Verlag von Theodor Steinkopff, Dresden and Leipzig. RM. 15 (paper); RM. 16.50 (cloth). 9 x 6; x + 181; 1937.

This monograph is intended as a brief and critical survey of the mutation work done since the first attempts of H. J. Muller to produce mutations in *Drosophila* by X-rays. By far the greater part of the book is devoted to the effect of X-rays on the chromosomes and the various mutations produced. Chapters 8, 9, and 10 deal respectively with temperature and chemical influences on mutations, the nature of nutations, and the usefulness of experimental mutations. There is an extensive bibliography and an index.

GENERAL BIOLOGY

ECOLOGICAL ANIMAL GEOGRAPHY. *An Authorized, Rewritten Edition Based on Tiergeographie auf oekologischer Grundlage.*
By Richard Hesse, W. C. Allee and Karl P. Schmidt. John Wiley and Sons, New York; Chapman and Hall, London. $6.00. 9 x $5\frac{7}{8}$; xiv + 597; 1937.

Here is an example of coöperative book production. Dr. Hesse wrote the original work, Dr. Schmidt translated it, and Dr. Allee completely rewrote it, changing the order in which the material originally was presented, and altering it when in his judgment the work might be improved thereby. Thus the present work may aptly be compared to a revised edition.

The book is divided into four parts. The first deals with general ecology, the next three with the special ecologies of the ocean, the inland waters, and the land.

The oceans of the world are continuous, with little to obstruct the spread of species except local changes in temperature, while by way of contrast the various freshwater systems are so isolated from each other that one would suspect, if segregation be an important factor in evolution, that every river system would have its own endemic fauna. As is well known exactly the opposite occurs, and perhaps the most interesting part of the book is its discussion of fresh water faunas. The explanation is of course that while fresh water systems are isolated in space they are not so in time. Geological agencies are consequently making and unmaking river systems. The Great Lakes have had eight different outlets to the sea since their formation in the pleistocene. The Hwang Ho has had even more in historic times. Furthermore, lakes are only a temporary feature of physiography, and they fill up too rapidly for any considerable amount of evolution to take place in them. To develop an endemic fauna, a lake must have long duration in geologic time, and be isolated from other bodies of water. Such lakes are extremely few, and consequently endemic faunae are rare. The only lakes with endemic faunae are Lakes Tanganyika, Baikal, Ochrida, and three small lakes in Celebes, all of which are deep enough to offer real abyssal conditions. It is strange that no mention is made of Lake Lanao in the Philippines with its well known endemic molluscan fauna.

There is a chapter dealing with ecological changes brought about through human agency, which today outnumber all other ecological changes, and a comprehensive index of 41 pages.

Perhaps the most helpful feature is the style. It is necessary in discussing ecology to make use of a great many terms unfamiliar to any reader except the specialist in ecology, but these are all carefully explained in the text. The documentations are voluminous, sometimes running up to over 150 footnotes to the chapter. Everything considered, this is a work of great significance, and one that should be widely read.

GENERAL BIOLOGY.
By Leslie A. Kenoyer and Henry N. Goddard. Harper and Bros., New York. $3.50. $8\frac{1}{2}$ x $5\frac{1}{2}$; xxiii + 630; 1937.

A MODERN BIOLOGY.
By Ernest J. Holmes and R. Darnley Gibbs. Foreword by Ralph H. Crowley. The University Press, Cambridge; The Macmillan Co., New York. $1.32. $7\frac{1}{4}$ x $4\frac{7}{8}$; xv + 272; 1937.

METHODS IN BIOLOGY.
By Alfred C. Kinsey. J. B. Lippincott Co., Chicago. \$2.50. $8 \times 5\frac{1}{4}$; x + 279; 1937.

ANIMAL BIOLOGY. *Revised Edition.*
By Michael F. Guyer. Harper and Bros., New York. \$3.75. $9\frac{1}{4} \times 6\frac{1}{8}$; xx + 735; 1937.

The authors of *General Biology* have produced an excellent elementary textbook for the college student. The material is not original but has been carefully selected from a wide range on the basis of years of experience in organizing and teaching the subject. Outstanding features of the book are the balance maintained between the plant and animal types described, the large number of illustrations with their detailed captions, and the comprehensible presentation of an adequate physical and chemical background for the basic physiological processes. The appendices include an outline of classification of plants and animals, a glossary, and a list of references. There is also an index.

A Modern Biology was designed for high school seniors and intended to serve as a sound foundation for education "in the art of living the healthful life." This results in a somewhat unusual organization and emphasis of the material, although the material itself does not differ appreciably from that presented in other modern biology texts. The book is illustrated with drawings and photographs and contains an index.

Methods in Biology is a book for the teacher. It contains much good advice and many helpful suggestions and should prove useful to the veteran in a rut as well as to the novice.

The first edition of *Animal Biology*, a complete elementary text, was noticed in these columns in Vol. VII, No. 3. In the revised edition the synoptic survey of the animal kingdom has been expanded into an entire section, and there are other minor changes and a few additional facts.

LE VOYAGE DE LA PÉROUSE SUR LES CÔTES DE L'ALASKA ET DE LA CALIFORNIE (1786). *Avec une Introduction et des Notes. Historical Documents Institut Français de Washington Cahier X.*
By Gilbert Chinard. The Johns Hopkins Press, Baltimore. \$3.00. $10\frac{1}{4} \times 7\frac{1}{2}$; xlix + 144 + 22 plates; 1937.

The study of geographic exploration is always fascinating, and such names as those of Marco Polo, Paul Knutsen, Fernando de Magellan, and Cabeya de Vaca never fail to stir the imagination. La Perouse does not exactly belong to this group, for he was not so much of a pathfinder as a pioneer; he had been preceded in Alaska by Bering and in California by Cook.

Much has been written of the exploration of the West Coast by the Spanish, the Portuguese, the English, the Russians, and even the Japanese, but it is not so generally recognized that the French, through the expedition of La Perouse, also had a large hand in this work. This is principally because the publication of La Perouse's narrative did not take place until the decade following that of George Dixon, although the latter did not arrive on the ground until a year later than his French contemporary. As a result, the earlier place names given by La Perouse have had to fall within the synonymy of those of Dixon by the law of priority.

To remedy in some degree this lack of knowledge of the French pioneer the French Institute of Washington has issued this beautifully prepared volume, containing the text of La Perouse's own narrative of his voyage from Hawaii to Mount Saint Elias, thence to Monterey and return, together with an extensive introduction. Here one can read the account of the storm which destroyed two of his ships and took the lives of twenty-one of his men, and the reader naturally wonders if during this experience he had any premonition of the fate that was to be his two years later, when he and all his band disappeared between Botany Bay and Vanikoro, never to be heard of alive again.

La Perouse's scientific interests covered the entire field of natural history. All three kingdoms of Linnaeus were represented in the collections made by his party. In addition he made an investigation of the general health of the Indians with whom he came into contact, made anthropometric measurements on them, prepared vocabularies of their languages,

discussed the history and economics of the Spanish missions in both Californias (although he himself never got south of Monterey) and took down some of their vocal music. Incidentally it may be noted that he reports part singing by the natives of Alaska. Similar observations have since been made by Chavez and Marcelli on the Indians of Chile and Peru, yet most historians of music state that part singing is indigenous only to the Keltic peoples of western Europe, and that wherever else it occurs it has been derived ultimately from this source. The matter is in need of further investigation.

The illustrations of scenery and of natural history objects are on the whole superior to those of people. Those of the Alaska Indians, for instance, indicate Nordic features with expanded lower lips, such as are commonly associated with certain aboriginal Africans. It is conceivable that the Alaskan Indians may have looked like that, but it seems most unlikely. This is no reflection on the present work which merely reproduces the illustrations from the original text.

The index covers four pages.

Faisceau Énergétique et Biologie *Biogenèse et Pathogenèse. Pression Solaire.*

By G. Froin. Girardot et Cie, Paris. 30 francs. $9\frac{1}{4} \times 6\frac{1}{4}$; 327; 1937 (paper).

Using Hegel's phrase *Das Wahre ist das Ganze* as a title-page quotation, the author proceeds to explain the origin of life and disease by drawing upon chemistry, physics, neurology, physiology *and* astrology, etc. The author is difficult to follow in some of his arguments. Perhaps the best idea of his train of thought may be gained from the translation of a few excerpts from the last chapter entitled *Influence of the stars on life and terrestrial maladies.*

The red corpuscles live 42 days, that is 6 weeks. Their duration of life is governed for 4 weeks by the Earth and the Moon and by Mercury and Venus for the remaining 2 weeks.

Hematolysis is released by terrestrial magnetic pressure during the new moon and the first quarter. In 14 days the 6 magnetons that cover each surface of the red corpuscles are liberated into the blood. During the 14 days of the full moon and the last quarter the 6 photons that cover each surface of the red corpuscles are eliminated by the blood. There remain 24 noctons; 12 are eliminated by Mercury and 12 by Venus during the last 14 days. Such is the process of total hematolysis in the male, utilizing noctons and photons, liberated and very active, for the building up of spermatozoids.

For the growth of the foetus, the normal astral pressures are not sufficient. The nourishment of the foetal tissues by the generative nocto-magnetons is produced through the medium of the hypophysis, which fixes a large mass of these elements, absorbed by the eyes and the optic nerves. If they are predominant in the blood of the woman, and especially if they penetrate the placenta in great numbers, a male child develops. For that, it is necessary that together with a strong solar pressure an equally strong counter-pressure is produced on all the axes of the dihydrol through the medium of Jupiter. . . . If the pressure of Jupiter is less strong and if the photonic lines are activated by the Asteroids and the magnetic line by Mars, a less abundant migration of prolan into the placenta is produced and a female is created.

The author claims this to be the first book published to date on biological physics.

In Quest of Gorillas.

By William K. Gregory and Henry C. Raven. The Darwin Press, New Bedford, Mass. $3.50. $9\frac{1}{4} \times 6\frac{1}{8}$; xvi + 241 + 111 plates; 1937.

Dudley J. Morton, at the College of Physicians and Surgeons of Columbia University, found from his studies on orthopedic problems that a great deal of both theoretical and practical knowledge could be acquired about the mechanism of the human foot by viewing it in the light of its long evolutionary history. The American Museum of Natural History could supply him with all the specimens he needed with the exception of adult gorillas and primitive unshod humans. In pursuit of the material lacking, the museum and Columbia University sent out an expedition under the leadership of Mr. Raven.

This is the story of their journeys and adventures in the mountains and jungles of Equatorial Africa. In a pleasant style the authors tell of the African towns, and of the lives of the natives with whom they came in repeated contact in their efforts to secure and photograph foot prints.

The tale of getting the gorillas, although at times daring and exciting, is by

no means entirely one of active adventure. The creatures, which were to be embalmed and sent home, had to be shot in the head only, as a body wound might cut many blood vessels and prevent the preserving fluid from reaching all parts of the body. As they had permission to shoot five specimens only, two in the Belgian Congo and three in West Africa, the hunters had to miss many chances of merely slaughtering their prey, and wait tedious weeks for precisely the right moment to kill.

Abundantly illustrated with many interesting photographs and an occasional sketch from Gregory's note-book, this volume is a charming account of a scientific expedition.

HANDBOOK OF MICROSCOPICAL TECHNIQUE. *For Workers in Animal and Plant Tissues. Second Edition, Revised and Enlarged.*

Edited by C. E. McClung. Paul B. Hoeber, Inc., New York. $8.00. $9\frac{1}{4}$ x 6; xvii + 698; 1937.

It is obvious that no individual person could gain, in the course of a normal life time, the experience required to work out with such precision and detail, the numerous microscopic techniques described in this volume. The editor gives us the very broadest general methods of preparing materials for microscopic study. He then turns the enterprise over to numerous specialists in the different fields of microscopic investigation, and each of them describes the methods and techniques that have evolved as a result of many years experience in a special field.

The book contains many illustrations provided to present the structure of pieces of apparatus and manipulative procedures. Because the volume is a hand book for practical use, there is no historical treatment of the subject, and generally only such bibliography as is necessary to complete an understanding of the indicated methods. It is assumed that the book will be of interest particularly to workers in bacteriology, botany, cytology, embryology, histology and pathology, but the presentation of general methods in Part I makes it useful to any student using the microscope.

THE LIVING WORLD.

By Samuel H. Williams. The Macmillan Co., New York. $3.60. $8\frac{1}{2}$ x $5\frac{1}{2}$; xxii + 704; 1937.

The teacher who has tried to organize a course in nature study knows the scarcity and consequent need of text books in this field. It has been the desire of the author to fill this need by giving us a text that he has prepared primarily for use in courses of nature study, field biology, and the elements of ecology. The book is organized around four large topics, namely; (1) The Biological Aspects of Living Things, (2) Animal Study, (3) Plant Study, and (4) Methods of Study. The author has approached the subject with emphasis on the method of study, rather than on types or forms of organisms as is generally the case. The method suggested and emphasized is that of the study of living things in their natural surroundings. Emphasis is also placed on the study of the relations and reactions of living things to each other, and to their surroundings, as observed in nature.

The book is well planned, well written, and well illustrated. Many chapters contain extensive bibliographies on the topic under consideration. There is an appendix of some two hundred questions and answers on general knowledge of the living world. The book will be of great value to every student and teacher of nature study, as well as to those few laymen who love and appreciate life in all of its various forms.

CODE UNIVERSEL DES COULEURS. *720 Couleurs.*

By E. Séguy. Paul Lechevalier, Paris. 60 francs. $6\frac{1}{4}$ x $4\frac{3}{8}$; lxviii + 55 plates; 1936 (paper).

Strictly speaking, this is not a book at all. It consists of forty-eight cards, each with fifteen colors arranged like the samples of a paint salesman. All the colors are blends of red, blue, and yellow, the proportions of which are constant on each card, but vary from card to card. Also, the colors are diluted with varying amounts of white and black, the proportions of white being constant in the hori-

zontal rows and those of black constant in the columns, except that in some instances other colors than black are used, the proportions remaining the same. There are also seven cards of solid color, each with a rectangular hole in the center, for the purpose of covering the other cards except for the one color that is being compared. There is also an explanatory booklet printed in French, with condensed summaries in six other European languages. Whether or not it is superior to Ridgway's Chart will be a matter of personal taste.

Das Gefüge des Lebens.
By Ludwig von Bertalanffy. B. G. Teubner, Leipzig and Berlin. RM. 6.80; outside of Germany RM. 5.10. 9 x 6; iv + 197; 1937.

This book is a well written and stimulating discussion of the phenomenon of life, based on the principles of dynamic biology. A thorough study of the structure and functions of the cell, the physiology of metabolism, the manifestations of irritability, and the powers of growth and development, has shown the author that any living being is a system of fundamental activities, all working together for the existence of the ultimate well-regulated organism. The mathematical interpretations of certain aspects of life are in keeping with the author's well known organismal theory, and his devotion to exact biology.

The volume is well illustrated. The bibliography of 114 titles and the complete index are an adequate conclusion to the work.

University of Colorado Studies, *Volume 24, Number 2.* Containing following articles: *The Birds of Boulder County, Colorado,* by Gordon Alexander; *Population Studies of the Trout of the Gunnison River,* by Harold Pratt; *Algae of some Thermal and Mineral Waters of Colorado,* by Mildred Hallberg Jones; *A Working Bibliography of Daylength and Artificial Illumination as Affecting Growth of Seed Plants,* by Francis Ramaley.
University of Colorado Press, Boulder. $1.00. 10 x 6⅞; 48; 1937 (paper).

HUMAN BIOLOGY

Anthropology. *An Introduction to Primitive Culture.*
By Alexander Goldenweiser. F. S. Crofts and Co., New York. $3.75. 9 x 6; xxi + 551 + 30 plates; 1937.

Of all the various sciences that go to make up biology that most in need of systematization is anthropology, the science in which *Homo sapiens* becomes the studied object as well as the studying subject. The present work does not achieve the synthesis of ethnology and archaeology but it goes a long way in that direction. It is a work which may be profitably read several times, each time disclosing some new item of interest.

It is characterized by the inclusion of two chapters, which the author says are unusual for this type of book. The first is on how anthropologists work—a chapter that might well have been expanded into several. Many statements made by anthropologists do not carry conviction because the average man knows so little of the technique of the science. The anthropologist who discovers two cultures in widely separated localities and who asserts confidently that the one is older than the other may be absolutely correct in what he says, but except to those who know something of the underlying factors that led to the decision the simple statement is not likely to seem very convincing.

The need for the other chapter is not so apparent. It is an account of the contacts between the white man and the Indian and appears to have been written hurriedly with no attempt to verify statements. Thus in speaking of the activities of the present administration he writes "The response of the Indians to this opportunity for home rule has been enthusiastic, even overwhelming." As a matter of fact, the most uncompromising opposition to the innovations of the present administration come from the Indians themselves. It is true that the American bison has been saved from extinction by the

creation of bison ranges, but after all the Indian is a human being, and who can blame him for not wanting to be made into a living museum specimen on a reservation like the bison? While there is much in Indian culture that is superior to that of the white man, the fact remains that out of a population of about one hundred and thirty millions in the United States, the Indian (together with the mestizo) constitutes less than half a million, and if the Indian is to survive at all he must do so by becoming adjusted to the environment in which he must live; therefore the policy of Ex-commissioners Rhoads and Scattergood (not mentioned by Mr. Goldenweiser) of breaking up the reservations as quickly as possible consistent with justice to the Indian seems to be not only the more humane but the more practical course.

Apart from this chapter the book is amply documented with foot notes, and has three indices covering ten pages.

Recollections of My Life. *Memoirs of the American Philosophical Society held at Philadelphia for Promoting Useful Knowledge. Volume VIII, Parts I and II.*

By Santiago Ramón y Cajal. Translated by E. Horne Craigie, assisted by Juan Cano. American Philosophical Society, Philadelphia. Distributed by University of Pennsylvania Press, Philadelphia. \$5.00. $9\frac{1}{4}$ x 6; xi + 638 + 21 plates; 1937 (paper).

So long as Nobel prizes are awarded, specialists in the fields of award are going to look askance at the judgment displayed in some of the bestowals. This is merely because the Swedish Committee, able, fair, and high-minded as it is, falls a little short of divine wisdom. But surely no award they ever made has been less criticised, and none deserved criticism less, than that to Santiago Ramón y Cajal. For he was an honest and a very great man, who single-handed worked out and *proved* beyond question or doubt the main features of the minute structure of the nervous system. That he did it under working conditions of extraordinary difficulty is of no real importance in assaying the real merits of the achievement, as he himself was aware. What is of importance is the simple fact that he did it. But over and above that great achievement stands his whole life. For he embodied and exemplified throughout every one of his many days the highest ideals of science, in conduct and in thought. He was simple not smart; he was honest not devious; he was a scholar not a politician; he held opinions because they were his not because they were soundly orthodox and likely to get him forward with important people; he was modest not self-seeking; he was forthright not diplomatic; he never defamed another that his own eminence might be the surer; he persistently and irrevocably sought the truth and found it. In short he had all the virtues that are but little esteemed in academic preferment.

The translator and the American Philosophical Society deserve great credit for making Cajal's autobiography available to readers of English. If any book can, in this day and age of the world be an inspiration to young biologists just starting a career, when things of the spirit are a bit out of fashion, this book will be. From a literary point of view the translation is pedestrian in spots, but it is accurate. We like it for just that reason. Ramón y Cajal was an accurate man.

Reasons for Anger.

By Robert Briffault. Simon and Schuster, New York. \$2.50. $8\frac{3}{8}$ x $5\frac{5}{8}$; 265; 1936.

It seems curious that the same man who can write articles such as *Family Sentiments*, and *Recent Anthropology*, should be the author of the bombast that appears under the label of *The Human Mind in Revolution*. In the last named essay occurs the following passage:

> The ideologies of morons, of well-to-do old ladies, of Babbitts, the pseudoscientific forgeries of a Professor Keller, or a Professor Osborne, the idiocies of a Millikan, to cite but some fragments of the falsification of all thought under the Dictatorship of the Bourgeoisie, would be impossible and unthinkable under the Dictatorship of the Proletariat. They would be impossible, not because they would be censoriously repressed, but because they could not arise; they would be devoid of motivation. Such stupidities and mental contortions cannot occur in Russia.

We wonder. The moral, if one can be drawn, is perhaps that an anthropologist should stick to anthropology, and leave prophecy and novel writing to the literati.

Family Sentiments is an able attack upon the position of those anthropologists who are advocates of the theory of social origins from the paternal family. In Briffault's opinion, "The whole notion that social organization is founded upon kinship is erroneous. The truth is the exact reverse: kinship is founded upon social organization."

Lest any of our readers should be misled by the title of the book as to its content, we feel it our duty to say that a nicer perception of the precise use of language would have made it *Reasons for Mr. Briffault's Anger*.

MITLA: TOWN OF THE SOULS *and Other Zapoteco-Speaking Pueblos of Oaxaca, Mexico.*
By Elsie C. Parsons. University of Chicago Press, Chicago. \$4.00. 8 x $5\frac{1}{2}$; xix + 590 + 51 plates + 3 folding maps: 1936.

Mitla is a town with a population of about 2000 located in southern Mexico, in the region of Oaxaca where ancient sepulchres and other ruins represent the remains of civilization of the Zapotecans conquered by the Aztecs. The author established herself in Mitla for the purpose of determining to what extent Spanish customs had superseded or been superimposed on the native culture and behavior patterns. In a discursive style, she describes the town, the kinds of people therein, their economic and their social life. She discusses in some detail their political activities, their religious behavior and their superstitions. The population she portrays is of a people predominantly of Indian stock, easy-going, tolerant, primarily interested in trading. The attempt to individualize or measure the Spanish influence on the *mores* of this people is rather unsuccessful. In part this is due to an apparent lack of sufficient knowledge of Spanish customs either past or present. This is especially noticeable in her emphasis on certain behavior patterns which, while they may be common to most southwestern Indians, are also common to Spanish and other peoples. While the author's conclusions presented in a very tentative fashion may be disregarded, the simplicity and fullness of her descriptions render this book an important contribution to this particular field of ethnology.

MEN OF MATHEMATICS.
By E. T. Bell. Simon and Schuster, New York. \$5.00. $9\frac{1}{2}$ x $6\frac{1}{4}$; xxi + 592 + 30 portraits; 1937.

These twenty-eight essays on the great mathematicians, starting with Zeno, and ending with Georg Cantor, make fascinating reading. Professor Bell is primarily interested in the mathematical principles developed by the men who have been included, and biographical matter is used to the extent that it is necessary to make clear the intellectual development of the mathematician whom he is discussing. However, we may add, it is biography of the first rank. In none of his earlier works has the learned and witty author, in our estimation, written with comparable brilliance.

The mathematicians have been selected for the importance of their contributions to the mathematical sciences. Legendre, admittedly a great mathematician, has been excluded because, in Bell's opinion, his work has been supplanted by that of Gauss, Abel, and Jacobi. While the non-mathematical reader will find a great deal to interest him, it is quite obvious that the volume is addressed to the mathematician, professional and amateur. It is a little difficult to see how a person who has never got beyond trigonometry can be in any way impressed by assertions to the effect that a given mathematical demonstration possesses great "beauty." And if the same reader, after having gone through the author's brief account of Riemannian surfaces, claims that he now understands why Riemann is so important a figure in the history of mathematics, he is pretty much of a humbug.

There is an index, and twenty-nine full page portraits.

WE, THE TIKOPIA. *A Sociological Study of Kinship in Primitive Polynesia.*

By Raymond Firth. Preface by Bronislaw Malinowski. American Book Co., New York. $6.00. $9\frac{1}{4}$ x 6; xxv + 605 + 25 plates + 6 folding charts; 1936.

"Tikopia lies in the extreme east of the British Solomon Islands Protectorate, and is inhabited by twelve hundred healthy and vigorous natives." Firth lived among these people for twelve months, the only white man on the island. Previous acquaintance with other Polynesian tongues reduced his language difficulties to a minimum, and he seems to have been quite successful in establishing himself as a trusted member of the community.

The Tikopia are a people without much interest in the elaborations of technology or of decorative art, but they have built up a complex system of social and religious activities, to which they attach great importance. They have a tradition of hospitality, a respect for birth and for wealth, a sensitiveness to public opinion, and a realistic attitude to social intercourse. This book is a sociological analysis of family life and kinship. The treatment is detailed, partly because in these days of rapid change of custom and break-down of ancient cultures it is essential to preserve as much as possible of the records of native institutions; partly because an anthropological study of kinship in primitive society needs much more than the summary account of the principles of organization which it usually receives.

Students of human biology will be grateful to Dr. Firth for his lucid and objective account of the customs of an interesting race. An excellent index has been provided.

RHYTHM FOR RAIN.
By John L. Nelson. Houghton Mifflin Co., Boston. $3.25. $8\frac{1}{2}$ x $5\frac{1}{2}$; xi + 272 + 54 plates; 1937.

The seventh child of Sikyanömsi was about to be born. It was to be a boy—the rebirth of the tiny son whose soul had been drawn back to the spirit world the preceding year and had been awaiting since then the opportunity to be born again.

Throughout the narrative woven around the birth, life and development of Sikyanömsi's twice born son the drama and ancient culture of the Hopi Indians is beautifully portrayed. The Kachinas, or Masked Gods, play a dominant rôle in the lives of these people, and the author in his descriptions of their ceremonies and dances has given a realistic picture of this form of American life. The title of the book is drawn from the dances performed during the period of the great drought to appease the angry rain gods. For nearly three years while this desert race became nearly annihilated, in dance upon dance they gave of their last measure of strength in a vain and heart-rending attempt to bring the rain. The book may well be classed as an epic.

Additional features include 30 pages of plates and a glossary of Hopi words.

PASCAL *The Life of Genius.*
By Morris Bishop. Williams & Wilkins Co., Baltimore, in Collaboration with Reynal and Hitchcock, New York. $3.50. $9\frac{1}{4}$ x 6; xi + 398; 1936.

Bishop's vivid portrayal of Pascal is characterized by insight and understanding. The figure of this genius emerges distinct, alive and without distortion. Although he himself is sympathetic, the author does not attempt to evoke either pity or unbounded enthusiasm for the great intellect that was unable to apply its distinctive powers of ratiocination to personal problems of behavior. Contrary to most biographers, the author believes that there was a woman in Pascal's life. At least, it seems that besides his sister, Pascal succeeded partially in directing another woman towards the life of asceticism and religious fervor that he so fanatically desired for himself and those he loved. The author gives a sound evaluation of the scientific and literary contributions of his subject; and in the description of the religious controversy in which Pascal was a protagonist, he is not swayed by his sympathies.

The human biologist, to whom the personality of Pascal is always of particular interest, will enjoy the elegant prose of this scholarly work.

REDISCOVERING ILLINOIS. *Archaeological Explorations in and around Fulton County.*
By Fay-Cooper Cole and Thorne Deuel. University of Chicago Press, Chicago.

$2.00. 9½ x 6¾; xvi + 295 + 36 plates; 1937.

Scattered throughout Illinois are hillocks of various sizes constructed in by-gone times by the Mound Builders. Since 1925 the University of Chicago has been conducting an archaeological survey of these mounds. In 1930 the University of Illinois joined in the task. As the work progressed rather extensive excavations were undertaken. Six distinct cultures have been unearthed; two of these, the "Black Sand" and the "Red Ochre," were previously unknown. These and others are described.

Only the introductory chapter and the conclusions in this book are intended for the general reader. The remainder deals with "the more technical aspects of excavation and classification on which the general account is based." The text is interestingly illustrated with photographs and diagrams and is supplemented with numerous tables and appendices. There is also a glossary and an index.

GREAT FARMERS.

By J. A. Scott Watson and May E. Hobbs. Foreword by Right Hon. Walter E. Elliot. Selwyn and Blount, London. 12s. 6d. 8½ x 5½; 287 + 16 plates; 1937.

This volume is made up of a series of quaint and enjoyable stories concerning the great men of agriculture in England during the third quarter of the 19th century. The period, almost co-extensive with Victoria's reign, is known as the "Golden Age of British Agriculture," and is noted for its "Kings of the Farm." The authors have given us something of the individuality, craftsmanship, and artistry of the farmers of this period, and have shown us why the period is worthy of study.

The early chapters of the book are devoted to the general land and cropping conditions of the British Isles. In the later chapters, we find something of the importance of live-stock raising to the welfare of the nation. The book is well written and beautifully illustrated by a number of half-tone and line prints. It will be of great interest to those who desire a more thorough knowledge of the past struggles and present status of agriculture in England.

LA SÉLECTION DU PERSONNEL. *Dans les Entreprises de Transport le Laboratoire du Travail du Réseau de l'État. Actualités Scientifiques et Industrielles, 376. Biologie du Travail et Biotypologie, I.*

By Pierre Lévy. Preface by M. R. Dautry. Hermann et Cie, Paris. 10 francs. 10 x 6½; 37; 1936 (paper).

The purpose of this monograph is to point out the need for routine physio-psychological examinations of applicants when selecting the personnel for industrial and commercial enterprises. The practical importance of such examinations is illustrated by observations made in the laboratory, recently established for this purpose, of the French State Railways. Here it has been noted that employees, regarded as able workers by their superiors or having a record of comparatively few accidents, obtained above average scores in the examination. In contrast, those who had had a relatively large number of accidents or whose working ability was considered poor also responded poorly to the various examinations. These consist of determination of certain somatological characteristics, tests of precision and coordination of movements, of reaction to sensory stimuli, of attention, memory and mechanical aptitude. A description of the examination technique is not given here.

COÖPERATION AND COMPETITION AMONG PRIMITIVE PEOPLES.

Edited by Margaret Mead. McGraw-Hill Book Co., New York. $4.00. 9 x 6. xii + 531; 1937.

One of the present-day trends in the field of sociological research is to trace the development of our very complex social order from its simplest beginnings among the primitive peoples through to its present day status. In this manner, an attempt has been made to find some of the fundamental principles underlying all social orders, and to show the ways and means of living and functioning of these

orders in relation to the individuals within them.

The present book is an intensive study of the way in which human relations are organized among thirteen different tribes along coöperative, competitive, or individualistic lines. The study, drawn from primitive materials, covers a broad range in the scale of social organizations. The volume as a unit, with the introduction and the interpretative statement, may be used as a basis for a systematic approach to the problems of culture and personality.

METHODOLOGY OF SOCIAL SCIENCE RESEARCH: *A Bibliography. Publications of the Bureau of Public Administration, University of California.*

By Dorothy C. Culver. University of California Press, Berkeley. $2.00. $9\frac{1}{4}$ x 6; x + 159; 1936.

This bibliography is an attempt to present a selected and annotated guide through the masses of new material of direct value to social science research which have been published in English since 1920. 1509 books and articles are here classified into nearly a hundred sections in which they are arranged alphabetically according to author. The table of contents consists of a classification outline of the subjects covered, and includes: Research Methodology, Selection of Problem, Sources of Material, Techniques of Analysis and Interpretation of Data, and Preparation of Manuscript. Two complete alphabetical indices cover both authors and subjects and refer to the separate items in the bibliography.

FIJI. *Their People, History and Commerce.*

By Nancy Walker. H. F. and G. Witherby, London. 7s. 6d. net. $8\frac{1}{8}$ x $5\frac{1}{2}$; 167 + 8 plates; 1936.

Impresssions received during a three months sojourn among the Fiji Islands and their peoples are recorded in this little book which makes pleasant reading. We find that towns which are often unprepossessing in appearance on approach, usually have their beauty spots and individual characteristics; that modes of travel are sometimes unique; that there is a racial problem developing with the steady growth of the Indian population which now nearly equals the native population; that life on the Islands is highly dependent upon its sugar trade. A brief review of Fijian history brings out the great contrast between the life of the present day on this Archipelago and that of less than a century ago.

THE OTHER HALF. *The Autobiography of a Tramp.*

By John Worby. Lee Furman, Inc., New York. $2.50. 8 x $5\frac{1}{2}$; 307; 1937.

The author (who had little schooling) writes vividly and entertainingly of his experiences in "hoboing." Starting out at the age of sixteen after having escaped from a Canadian farm where he was placed by the authorities of an orphan asylum, he travelled far and wide in this country and England. Except for changes in spelling and punctuation and the omission of occasional passages the book is said to have been printed as it was written. It reveals much of the viewpoint and ethical standards of a man whose goal is to live well on his wits. With great candor the author describes at several points in great detail just what a lousy person he was. [This statement means exactly what it says.]

LIFE AND DEATH. *The Autobiography of a Surgeon.*

By Andrea Majocchi. Translated by Wallace Brockway. Knight Publications, New York. $2.75. $8\frac{1}{4}$ x $5\frac{1}{2}$; 300; 1937.

The very first sections where Dr. Majocchi describes his extraordinary experiences as an interne at the Milan Obstetrical Station are the best, but from then on the trailing off in interest is rapid. A short visit to America, during which the hospitals of the East and Middle West were toured, left the author very unfavorably impressed by the amount of specialization among American surgeons. There is an uninteresting chapter about a Congress held in Tripoli, and others, scarcely better, on dueling, and on a pilgrimage to Lourdes.

Descriptions of operations, of course, take up a great deal of space. Some of Majocchi's superiors at the Milan Obstetrical Station would have delighted Balzac, and it is a pity that they rate so few lines here.

EUGENIK.

By Hermann Muckermann. Ferd. Dümmlers, Berlin. M. 4.40. 9 x $6\frac{1}{8}$; viii + 173 + 8 plates; 1934.

The author, one of the leaders of the eugenic movement in Germany, presents a treatise of eugenics in the specific sense of the word. In this regard it is held to be the first of its kind. The material is based essentially on Muckermann's work as the Director of the Division of Eugenics in the Kaiser Wilhelm Institute for Anthropology, Human Genetics and Eugenics from 1927 to 1933. It is written in a clear, direct and interesting style,. is illustrated, indexed and contains a biliography.

DIE VERSTÄDTERUNG. *Ihre Gefahren für Volk und Staat vom Standpunkte der Lebensforschung und der Gesellschaftswissenschaft.*

By H. F. K. Günther. B. G. Teubner, Leipzig and Berlin. RM. 1.60 (in Germany); RM. 1.20 (outside of Germany). $8\frac{3}{4}$ x $5\frac{3}{4}$; 54; 1936 (paper).

This brochure is dedicated to Alfred Rosenburg, representative of the *Führer* and guardian of the education and growth of the national socialist movement. It is about the unquestionable superiority of the Nordics, with particular reference to the German Nordics. There is much musing, and dreaming and poetry mixed up with some racial anthropology.

STUDIES ON THE MUSCLES OF THE PELVIC APPENDAGE IN BIRDS.

By George E. Hudson. American Midland Naturalist. University of Notre Dame, Notre Dame, Ind. 50 cents. 9 x 6; 108; 1937 (paper).

Representatives of 16 orders of birds were used in this study. All of the muscles of the pelvic limbs were examined in 32 species and numerous other species were observed for specific points. The work was mainly by gross dissection but in a number of species celloidin cross sections were prepared and studied at various levels on the hind limb. Included in the study is a brief historical account of earlier work, a list of references and of abbreviations, and a group of 26 plates giving excellent illustrations of the main features of the investigation.

TRENDS IN DIFFERENT TYPES OF PUBLIC AND PRIVATE RELIEF IN URBAN AREAS, 1929–35. *U. S. Department of Labor, Children's Bureau, Bureau Publication No. 237.*

By Emma A. Winslow. Government Printing Office, Washington. 15 cents. $9\frac{1}{4}$ x 6; vi + 143; 1937 (paper).

The monthly expenditure from public and from private funds for different types of relief in 120 urban areas from January, 1929 to December, 1935 is given in tabular form. The various types of relief are analyzed separately for each of five large geographical subdivisions of the country, the classification being in accordance with the average monthly relief per family, and the average monthly number of families receiving aid.

A GRAPHIC SUMMARY OF FARM TAXATION. *U. S. Department of Agriculture, Miscellaneous Publication No. 262.*

By Donald Jackson. Government Printing Office, Washington. 5 cents. $9\frac{1}{4}$ x $5\frac{3}{4}$; 17; 1937 (paper).

Certain of the more important facts about United States farm taxes are shown in the twenty-four graphs and maps of which this pamphlet largely consists. These include real estate taxes, farm property taxes, gasoline and automobile license taxes, net returns and tax delinquency for all the individual states.

WHERE ARE WE GOING?

By E. C. Harwood. American Institute for Economic Research, Cambridge, Mass. \$1.00. 9 x 6; 88; 1937 (paper).

Such subjects as unemployment, the price of gold, the New Deal and World War inflations, future of the New Deal, wholesale prices, the influence of Maynard Keynes on our governmental policy of spending, etc. are discussed in a series of brief articles.

COMMERCIAL SHIPYARDS AND THE NAVY.
National Council of American Shipbuilders, New York. 10 x $6\frac{3}{4}$; 105; 1937.

ZOÖLOGY

CHECK-LIST OF BIRDS OF THE WORLD. Volume III.
By James Lee Peters. Harvard University Press, Cambridge. \$3.50. 9 x $5\frac{7}{8}$; xiii + 311; 1937.

THE DISTRIBUTION AND HABITS OF MADAGASCAR BIRDS. *Summary of the Field Notes of the Mission Zoologique Franco-Anglo-Américaine à Madagascar. Bulletin of the American Museum of Natural History, Volume LXXII, 1936, Article V.*
By A. L. Rand. American Museum of Natural History, New York. \$2.50. $9\frac{3}{4}$ x $6\frac{1}{4}$; 357; 1937 (paper).

BIRDCRAFT. *A Field Book of Two Hundred Song, Game, and Water Birds. Ninth Edition.*
By Mabel O. Wright. Illustrations by Louis A. Fuertes. The Macmillan Co., New York and London. 89 cents. $7\frac{3}{4}$ x $5\frac{1}{4}$; xxiii + 317 + 80 plates; 1936.

THE ABC OF ATTRACTING BIRDS.
By Alvin M. Peterson. The Bruce Publishing Co., New York. \$1.50. $7\frac{3}{4}$ x $5\frac{1}{4}$; v + 146; 1937.

Volume 3 of the *Check List* maintains the same high standard of previous volumes (Vol. 1 reviewed in Q. R. B. Vol. 7, No. 3). For those unacquainted with this series it should be said that it is designed to give a

"complete, authoritative, and up-to-date list of all known forms of recent birds, together with their ranges and all principal synonyms since the beginning of the century." The present volume "covers 142 genera and approximately 1675 forms, divided among the Sand-grouse, Pigeons (including Doves and Fruit Pigeons), and Parrots (including Lories and Macaws)."

For two years the members of the *Mission Zoologique Franco-Anglo-Américaine à Madagascar* were in the field making a collection of the birds and mammals of the island. The first part of their report on birds is devoted to "a consideration of the correlation between the habits and habitats, the distribution of the birds, a brief summary of the affinities of the avifauna, and a discussion of the migration and the breeding season in Madagascar"; the second, "to an account of the distribution and habits of the species, based on our observations." There are a number of illustrations but no index.

Birdcraft celebrates the twentieth anniversary of its existence by appearing in its ninth edition—a sufficient recommendation of its usefulness as a manual to bird lovers. The 80 full-page reproductions are from the paintings by Fuertes. The volume concludes with a key (23 pages) and indexes of both English and Latin names.

The last book in this group gives many practical hints on methods of attracting birds to become one's neighbors, and on how to establish an effective bird sanctuary.

WONDERS OF THE SEA: SHELLS. *15 Colour Plates Painted from Nature.*
By Paul A. Robert. With an Introduction by Julian Huxley and Introductory Text by Arnold Masarey. B. T. Batsford, Ltd., London. 5s. 6d. 11 x 8; 8 + 15 plates; 1937 (paper).

THE BEAUTY OF BUTTERFLIES. *Art and Nature in Colour.*
Introduction by Julian Huxley. Introductory text by Adolf Portmann. B. T. Batsford Ltd., London. 5s. 6d. net. 11 x 8; 8 + 12 plates; 1937 (paper).

The best possible way to write a review of these books would be to quote verbatim that part of Julian Huxley's introduction that is common to them both. The culmination of this is to be found in these words: "Here, certain facts of nature are presented. They are part of the raw material of science; however, they have been chosen, not for the scientific principles which they illustrate, but for the beauty which they embody." Why the scientist

should be concerned with the beauty of the objects he studies is adequately discussed.

Unfortunately, it costs money to produce such beautifully colored illustrations as comprise these books. Only a limited number of species may be figured without making the cost prohibitive. When we recall that authorities estimate the total number of described species of mollusca as between seventy and one hundred thousand, and that in contrast with this number only forty-five have been figured, we can realize that such books as these cannot go very far in imparting scientific knowledge. To do adequately would require about a thousand illustrations in each one, and then they would rank with Audobon's *Birds of North America*, which is the finest series of colored paintings of natural history objects that has yet appeared.

Each of these books contains a limited amount of explanatory printed matter, but the illustrations really constitute the books, and they cannot be too highly recommended to those who love the beautiful objects of nature and contemplate them with intelligence from the aesthetic standpoint.

KEYS TO THE FISHES OF WASHINGTON, OREGON AND CLOSELY ADJOINING REGIONS. *University of Washington Publications in Biology, Volume 2, Number 4.*

By Leonard P. Schultz. University of Washington Press, Seattle. 75 cents. 10 x 5; 126; 1936 (paper).

MARINE GAME FISHES *of the Pacific Coast from Alaska to the Equator. A Contribution from the Santa Barbara Museum of Natural History.*

By Lionel A. Walford, with paintings by Link Malmquist and photographs in natural color by Ralph Emerson. University of California Press, Berkeley. $5.00. 11 x 7¾; xxix + 205 + 69 plates; 1937.

The first of these publications is on both marine and fresh water fishes. In the keys, the statements are so arranged that one must consider alternative characters. With a little practice this system will be found most useful. The volume is illustrated and contains a glossary, and indices to common and scientific names occurring in the keys.

While the second of these books was written more for the angler it by no means falls into the "popular" class. The three authors aboard the *Haida* worked for two months studying, painting and photographing the fishes (mostly while still alive). Five new species were discovered and described. Detailed measurements and notes were made on at least five specimens of every species collected for anatomical variations. The volume contains 69 plates (36 colored), a glossary, keys, and index.

MY BIG GAME-HUNTING DIARY. *From India and the Himalayas.*

By Count Henrik Apponyi. Foreword by Viscount Halifax. Selwyn and Blount, London. 18s. net. 9 x 6; 254 + 157 plates; 1937.

In the foreword Count Apponyi expresses a naive suprise that his Indian hunting diary, written about his melodramatic and harrowing escapes, solely for his "dear mother" at home should ever be published. He shows himself to be a warm enthusiastic person who loves his friends above all else and prides himself on not being superstitious. Yet he is not fool enough to light "three on a match," and when he runs across home town friends in India, no matter how pleased he may have been, he realizes that their visit is limiting his killing, a thing unbearable to "all true sportsmen."

So eager is he to tell of the number of animals he shoots that in his boyish enthusiasm he sometimes quite naturally forgets to describe them, and his sportsman's love of hunting gives him little time to observe his prey before he shoots. When he does, however, he is not without appreciation for the beauty of the beast, and notes that the lovelier it is the stronger is his urge to kill. His 87 head of game present an enviable list of a winter's sport, and even exceeds the list of the celebrities he met, the palaces he stayed in, the banquets he attended, and the number of Rolls-Royces he rode in.

Numerous excellent photographs accompany the text and these not only in-

clude snaps of banquets and Maharajahs' automobiles, but also interesting ones of the scenery, old buildings, and native Tibetans.

The Practical Bee Guide. *A Manual of Modern Beekeeping. Eighth Edition.*
By Rev. J. G. Digges. The Talbot Press, Dublin; Simpkin, Marshall, Ltd., London. 4s. 6d. (paper); 6s. (bound). $8\frac{1}{2} \times 5\frac{1}{2}$; viii + 305; 1936.

Hives and other equipment involved in the handling of bees have become much more simplified in the United States than in many foreign countries. For this reason the bee keeper of the United States will have but little of practical value to gain from this treatise which is mainly devoted to descriptions of the more complicated apparatus and methods employed in the British Isles. While the book cannot be recommended for a beginner in this country, it is, however, interesting reading for one who is already acquainted with bees. Those chapters devoted to bee anatomy and bee behavior seem to be accurate and fairly inclusive. The book has an index but there is no bibliography.

Monograph of the Genus Erebia.
By B. C. S. Warren. British Museum (Natural History), London. £2. 10s. 11 x $7\frac{1}{2}$; vii + 407 + 104 plates; 1936.

This very fine monograph, on the brown Ringlet butterflies of the family Satyridae, gives testimony to the meticulous care with which its author made his study. His working basis was as follows:

(1) To supply definite proof, on an anatomical basis, of the real affinities of every hitherto described species, subspecies, form and aberration of *Erebia*; (2) to establish the correct use of every valid name, and to eliminate those that are only synonyms or homonyms; (3) to provide reliable data for the identification of species; (4) to treat the variation of each species comprehensively, but at the same time in as simple a manner as possible; (5) to give the distribution of each race accurately, but not in great detail, avoiding long distribution lists which would encumber the work without being of compensating value.

Included in the survey is a bibliography, a check list and an index of trivial names. The 104 plates give 1648 views of male and female species and subspecies of *Erebia*, of neuration and of male armatures.

Snakes of Maryland.
By Howard A. Kelly, Audrey W. Davis and H. C. Robertson. The Natural History Society of Maryland, Baltimore. 60 cents. $9\frac{1}{4} \times 6\frac{1}{4}$; 103; 1936 (paper).

This little book casts an interesting light upon one of the many and diverse interests of that amazing man, Dr. Howard A. Kelly. Interested in snakes from childhood, their collection and study has constituted a hobby throughout a long life. In *Snakes of Maryland* there are included descriptions of twenty-six non-venomous and two venomous species, with notes on habits, range throughout the United States and records of findings in Maryland. The text is supplemented with about thirty good clear photographs, eleven full page color plates beautifully painted by R. F. Deckert and with small diagrammatic drawings of the head and anal regions of several species.

Land-Improvement Measures in Relation to a Possible Control of the Beet Leafhopper and Curly Top. *U. S. Department of Agriculture, Circular No. 416.*
By R. L. Piemeisel and J. C. Chamberlin. Government Printing Office, Washington, 5 cents. $9\frac{1}{4} \times 5\frac{7}{8}$; 23; 1936 (paper).

This circular, based upon reconnaissance surveys of weed hosts and leafhoppers throughout southern Idaho, is an attempt to show that certain land-improvement measures would also help to control the beet leafhooper, vector of the curly top disease of sugar beets, beans, tomatoes and other cultivated crops.

As well-farmed lands and desert range in good condition do not produce economically significant numbers of this insect, the author believes that the leafhopper and curly top can be controlled if all lands, not continuously and well-farmed, can be restored and maintained as good desert range. The measures proposed for this accomplishment are similar to those

for land conservation, for in this the author sees not only increased forage and prevention of soil erosion, but also the control of the destructive beet leafhopper.

ECOLOGICAL STUDIES ON SELECTED MARINE INTERTIDAL COMMUNITIES OF MONTEREY BAY, CALIFORNIA.

By Willis G. Hewatt. American Midland Naturalist, University of Notre Dame, Notre Dame, Ind. 25 cents. 9 x 6; 46; 1937 (paper).

This report presents both qualitative and quantitative analyses resulting from an ecological investigation of the animal species occurring over a defined stretch of rocky littoral on the southern margin of Monterey Bay, California. The paper is of importance since it is the first to deal primarily with the sociological aspect of the littoral communities in this region. A definite zonation of the intertidal animals was found and various physical and biotic factors are considered in relation to this limited range.

There is included a systematic list of all the species (70) identified during the course of the work.

RESTLESS JUNGLE.

By Mary L. J. Akeley. Robert M. McBride and Co., New York. $3.00. $8\frac{3}{4}$ x $5\frac{3}{4}$; xiii + 313 + 32 plates; 1936.

Mrs. Akeley spent eight months in South Africa, principally in the Transvaal, Natal, and in Zululand. The expedition was undertaken for the purpose of studying the great game herds and the primitive people. The "hunting" was done entirely with camera.

Some of the most striking photographs are of Carl Akeley's groups in the African Hall of the American Museum of Natural History. A large portion of the book and perhaps the better portion, is description of native life, particularly that of Swaziland and Zululand.

MYTILUS. *L.M.B.C. Memoirs on Typical British Marine Plants and Animals. Edited by R. J. Daniel, XXXI.*

By Kathleen M. White. University Press of Liverpool, Liverpool. 9s. net. $9\frac{1}{2}$ x 6; vii + 117 + 10 plates; 1937.

The series of L.M.B.C. Memoirs was begun in 1899 for the purpose of recording types of "common Irish Sea animals and plants of which no adequate account already exists in the textbooks." This thirty-first memoir deals with the mollusc *Mytilus*. A detailed description is given of the external characters and the internal systems. The history, habitat, food, bionomics, economic importance, fossil record, life history, and rate of growth are discussed. The text is illustrated with excellent plates, each of which is thoroughly explained, and there is an extensive bibliography.

ON JUNGLE TRAILS.

By Frank Buck and Ferrin Fraser. World Book Co., Yonkers-on-Hudson, N. Y. 96 cents. $7\frac{3}{4}$ x $5\frac{1}{4}$; vii + 280 + 29 plates; 1936.

This is the typical *Bring Them Back Alive* sort of book, with adventure as its primary interest and science its secondary. The volume presents eleven regular Frank Buck adventure stories, a list of animals he has brought to America, and a twenty-page outline of information concerning Asiatic mammals, birds and reptiles. Numerous photographs of animals and traps enhance its value as an adventure book.

LEÇONS DE ZOOLOGIE. *Prochordés. Amphioxus, Tuniciers (I. Ascidies). Actualités Scientifiques et Industrielles, 379.*

By M. Prenant. Hermann et Cie, Paris. 15 francs. 10 x $6\frac{1}{2}$; 77; 1936 (paper).

LEÇONS DE ZOOLOGIE. *Prochordés. Tuniciers (II. Pyrosomes), (III. Doliolides), (IV. Salpes), (V. Appendiculaires). Actualités Scientifiques et Industrielles, 380.*

By M. Prenant. Hermann et Cie, Paris. 12 francs. 10 x $6\frac{1}{2}$; 49; 1936 (paper).

These two numbers of a series of zoological texts, previous parts of which have been noticed from time to time in these columns, present an adequate, conventional survey of the development and structure of *Amphioxus* and five genera of

tunicates, as listed in the second title above. Both books are illustrated and selected bibliographies follow the discussion of each genus.

THE FURTIVE FOLK. *Feuds of Fur and Feather.*

By Dan Russell. Illustrated by Richard Ogle. Heath Cranton Ltd., London. 6s. net. $8\frac{1}{2} \times 5\frac{1}{2}$; 186; 1937.

Although at times he tends toward the poetic and sentimental, Mr. Russell has given an excellent picture of the wild life of the English countryside. The stories of the hunter and the hunted, are simply told in this series of forty short sketches, and cleverly illustrated in black and white.

SPIDERS AND THEIR KIN. *Cornell Rural School Leaflet, Volume 30, Number 2.*

Prepared by E. Laurence Palmer. The New York State College of Agriculture at Cornell University, Ithaca. Free. 9 x 6; 32; 1936.

The present volume of the *Leaflets* is devoted to a résumé of the spiders and other forms closely related to them. The discussion, which is very elementary, includes the classification, behavior, and economic value of many of the common species. The photographs and drawings add considerably to its value as a teaching device.

DAS SAMMELN, KONSERVIEREN UND AUFSTELLEN VON WIRBELTIEREN. *Leitfaden für Sammler, Liebhaber und Fachleute nach neuzeitlichen Gesichtspunkten.*

By Gerhard Schröder. Verlagsbuchhandlung Paul Parey, Berlin. RM. 4.80 (less 25 per cent in foreign countries, except Switzerland and Palestine). $8\frac{1}{2} \times 5\frac{1}{2}$; viii + 93 + 32 plates; 1936 (paper).

SAMMELN UND PRÄPARIEREN VON TIEREN. *Eine Anleitung zum Anlegen von zoologischen Sammlungen.*

By Georg Stehli. Franckh'sche Verlagshandlung, Stuttgart. RM. 2. $7\frac{7}{8} \times 5\frac{1}{4}$; 95; 1936.

The first book listed is primarily intended for museum conservators and professional taxidermists. A great deal of emphasis is laid on stuffing each animal in a way that will make it look as nearly life like as possible and whenever possible to bring in some portrayal of its natural environment. More attention is given to birds and mammals than to any other vertebrates.

The second book is a simple instruction book for collectors, amply provided with illustrations. It is done with great thoroughness, though only the simpler preparations of forms of each species are described. The author begins with a discussion of the mammals and works backwards to the lower forms through birds, reptiles and snakes, fish and invertebrates.

ZOOLOGICA. *Scientific Contributions of the New York Zoological Society. Volume XXII Part 1, Numbers 1–6. Containing following articles: The Electric Discharge of the Electric Eel, Electrophorus electricus (Linnaeus)*, by C. W. Coates, R. T. Cox and L. P. Granath; *The Templeton Crocker Expedition. II. Introduction, Itinerary, List of Stations, Nets and Dredges*, by William Beebe; *The Templeton Crocker Expedition. III. Brachygnathous Crabs from the Gulf of California and the West Coast of Lower California*, by Jocelyn Crane; *The Templeton Crocker Expedition, IV. Porcellanid Crabs from the Gulf of California*, by Steve A. Glassell; *The Templeton Crocker Expedition, V. A New Chrysomelid Beetle of the Genus Monoxia from Lower California*, by Doris H. Blake; *A New Species of Caulolatilus from Trinidad, British West Indies*, by William Beebe and John Tee-Van.

New York Zoological Society, New York. \$1.60. $10\frac{1}{2} \times 7$; 95 + 11 plates; 1937 (paper).

BOTANY

THE IDENTIFICATION OF TREES AND SHRUBS. *How to recognize without previous knowledge of botany wild or garden trees and shrubs*

native to the north temperate zone, with 2500 diagrams made by the author.

By F. K. Makins. E. P. Dutton and Co., New York. $4.00. $9\frac{3}{4}$ x 6; vii + 326; 1937.

FOREST TREES AND FOREST REGIONS OF THE UNITED STATES. *U. S. Department of Agriculture Miscellaneous Publication No. 217.*

By Wilbur R. Mattoon. Government Printing Office, Washington. 15 cents. $9\frac{1}{4}$ x $5\frac{3}{4}$; 54 + 14 plates; 1936 (paper).

GROWTH OF DOUGLAS FIR TREES OF KNOWN SEED SOURCE. *U. S. Department of Agriculture Technical Bulletin No. 537.*

By Thorton T. Munger and William G. Morris. Government Printing Office, Washington. 10 cents. $9\frac{1}{4}$ x $5\frac{7}{8}$; 40; 1936 (paper).

The rules for the use of the key for the identification of trees and shrubs are easy to master. There are six pages of simple botanical terms alphabetically arranged, with which one must first have an acquaintance. The key itself is confined to one page where trees and shrubs with compound leaves or with normally simple leaves are sub-divided into leaf shapes and leaf arrangement. Having there found the most likely sounding group, one turns to the figures indicated and searches for the diagram most closely resembling his specimen. The name under the diagram may then be looked up in the index which refers to page of description. We should like to be able to recommend this key unqualifiedly because of its simplicity, but we have to admit we found some difficulty in checking up with specimens from the botanical gardens.

In the second publication all the tree species of continental United States are listed and classified according to the forest region in which they are found. Both common and scientific names are given with brief notes as to geographic range and distinguishing characteristics. Each list is preceded by a brief description of the forest region and a summary of the names of its principal trees. There are similar descriptions and summaries of the forests of Alaska, Puerto Rico and Hawaii.

Douglas fir being the outstanding tree of the commercial timber zone of western Washington and western Oregon, it was selected for experimental use in testing and comparing the growth made from seeds which had matured under one set of conditions (involving age, health, growing space and quality of site of the parent trees and the altitude at which they were found), when they were grown under other, varying conditions but still within the principal range of the Pacific coast form of Douglas fir. This report contains a description of the experimental procedures—which were initiated in 1912 and 1913—and includes an analysis of the results to date.

MANUAL OF THE GRASSES OF THE WEST INDIES. *U. S. Department of Agriculture, Miscellaneous Publication No. 243.*

By A. S. Hitchcock. Government Printing Office, Washington. $1.25. 9 x $5\frac{3}{4}$; 439; 1936.

BRITISH GRASSES *and Their Employment in Agriculture. Third Edition.*

By S. F. Armstrong. The University Press, Cambridge; The Macmillan Co., New York. $5.25. $8\frac{1}{2}$ x $5\frac{1}{2}$; ix + 350; 1937.

These books both describe various grasses in considerable detail. In the volume on the grasses of the West Indies Hitchcock devotes only a couple of pages to a very brief and general summary of the economic uses of these plants. The rest of the volume is comprised entirely of scientific descriptions, in accordance with the International Rules of Botanical Nomenclature, of all the known species of West Indian grasses. On the other hand, the author of the volume on British grasses has produced a completely practical book written primarily for students of agriculture. The first part, the botanical section, deals with the morphology, general biology, grouping according to various characters, and botanical description of species. All the native species are described but more space is given to those of greater abundance or of greater economic importance. The second part, the agricultural section, deals with the economic, agricultural, and commercial aspects of grass crops and also includes a chapter on lawns and greens. Both books are well indexed and illustrated.

OKLAHOMA FLORA.

By Thomas R. Stemen and W. Stanley Myers. Harlow Publishing Co., Oklahoma City. $6.00. $7\frac{1}{2}$ x $5\frac{1}{4}$; xxix + 706; 1937.

This fine volume is a key for the classification and identification of all the flora of Oklahoma, exclusive of the grasses, sedges, and rushes. The book was prepared primarily for use in Oklahoma, but it describes species common to Kansas, Arkansas, and Texas, and so may be used in identifying the flora of these neighboring states.

The volume includes a preface, a table of contents, the analytical key to families, appendices of (a) Wild Flowers that should be protected; (b) Hay-fever plants; (c) Poisonous plants; (d) Drug producing plants; (e) Native shore and water plants useful as food and shelter for pond fish, frogs, tadpoles, crayfish, wild ducks, etc.; (f) Edible plants; a glossary; an index of scientific names; and an index of English names.

Many species are illustrated with drawings of roots, stems, leaves, flowers, and fruit, or by a drawing of the entire plant. The *Flora* will be of service to students of biology and botany as well as to the nature loving public.

WINE GRAPES. *Their Selection, Cultivation and Enjoyment.*

By Philip M. Wagner. Harcourt, Brace and Co., New York. $2.50. 8 x $5\frac{1}{4}$; xii + 298 + 8 plates; 1937.

There are chapters on "What the Vine Is," "How the Vine Grows," "Climate and Soil," "Propagating the Vine," "Establishing the Vineyard," "Cultivation and Harvesting," etc. The various grape-growing districts scattered over the United States are described, and the vine varieties that are best supported by each are indicated. The long list of grape varieties, giving under each the type of wine that the grape produces, kind of soil to which it is best adapted, pruning, disease prevention, etc. should be of value to anyone interested in vine culture. The language is non-technical. It is worth noting that the grapes which produce the best wines are difficult, if not impossible, to obtain outside California. Some Californians, at any rate, have access to excellent domestic wines—but that does not make the rest of us feel any more cheerful.

The book is well illustrated and indexed.

METHODS FOR THE MEASUREMENT OF CERTAIN CHARACTER PROPERTIES OF RAW COTTON. *U. S. Department of Agriculture, Technical Bulletin No. 545.*

By Howard B. Richardson, T. L. W. Bailey, Jr., and Carl M. Conrad. Government Printing Office, Washington. 15 cents. $9\frac{1}{8}$ x $5\frac{7}{8}$; 77; 1937 (paper).

This paper summarizes the latest experimental work on determining the strength of raw cotton fibers. The authors have pointed out the errors and difficulties that were present in the older methods of measuring character properties, and have eliminated, or corrected them in such a way that a more efficient technique has evolved. Methods for calculating the biometric constants describing the different characteristics of raw cotton have been given in detail. The work is well illustrated by tables, charts, and graphs, as well as photographs of apparatus, and micro-photographs of the different types of cotton fibers. A bibliography of 70 papers, and an appendix of tables and formulae pertinent to the discussions are valuable additions to the paper.

BUD SELECTION IN EUREKA AND LISBON LEMONS AND PROGENY TESTS OF BUD VARIATIONS. *U. S. Department of Agriculture, Technical Bulletin No. 531.*

By A. D. Shamel, C. S. Pomeroy and R. E. Caryl. Government Printing Office, Washington. 10 cents. $9\frac{1}{4}$ x $5\frac{7}{8}$; 44; 1936 (paper).

According to the authors, practically all of the lemons grown commercially in the United States have been developed from two original strains: the Lisbon and the Eureka. Apparently, bud variations of individual fruits, limbs, and entire trees are fairly common. In this pamphlet some of these variations, the frequency of

their occurrence, their significance, and their relative inherent stability are discussed. Data have been drawn from the Citrus Experiment Station orchard and from a commercial orchard, and the importance of the results to the industry is indicated.

Zur Soziologie der Isoëtetalia. *Pflanzengeographische Kommission der Schweizerischen Naturforschenden Gesellschaft. Beiträge zur geobotanischen Landesaufnahme der Schweiz, Heft 20.*

By Max Moor. Hans Huber, Bern. 6.50 francs. 9 x $6\frac{1}{4}$; 148 + 7 plates + 2 folding charts; 1936 (paper).

Five species of *Nanocyperion* and one of *Isoetion* are treated in this book. The former group is found in Central Europe, the latter in the Mediterranean regions of southern Spain, France, the islands west of Italy and northern Africa. Information for each species includes general and floral characteristics, structure, periodicity, ecology and geographic distribution. A six-page bibliography is included.

MORPHOLOGY

Histoire des Origines de la Théorie Cellulaire. *Actualités Scientifiques et Industrielles, 328. Exposés d'Histoire et Philosophie des Sciences, III.*

By Marc Klein. Hermann et Cie, Paris. 15 francs. 10 x $6\frac{1}{2}$; 72; 1936 (paper).

This short but informative work relates the history of the cellular theory from Hooke's *Micrographia* through Max Schultze's famous article *Ueber Muskelkörperchen*, with a brief mention of more recent writings up to about 1904. It describes clearly and succinctly the chief courses of thought in the development of this subject, and mentions the more important disputes and points of disagreement. It is to be hoped that this monograph will be translated into English, for it would be of considerable value to undergraduate students as collateral reading in even rather elementary college courses in biology.

PHYSIOLOGY AND PATHOLOGY

Who Gave the World Syphilis? *The Haitian Myth.*

By Richard C. Holcomb. Introduction by C. S. Butler. Froben Press, New York. \$3.00. 9 x 6; 189; 1937.

The intention of the author is to demonstrate that there is no factual foundation to the theory which considers Haiti as the place of origin of syphilis. The greater part of this monograph is dedicated to a chapter by chapter summary of a book by Ruiz Diaz de Isla, the Spanish surgeon whose writings are generally accepted as *prima facies* evidence of the importation of syphilis from Haiti. Ruiz Diaz stated that in Barcelona, in 1493, he had treated for syphilis a pilot from Palos. This statement, according to Holcomb, was later cited to read that Ruiz Diaz treated one of the Pinzon brothers after the return from the first voyage made by Columbus. This was proof that the first case of syphilis had come from America. Very neatly, Holcomb shows that it is highly improbable if not impossible that the pilots treated by Ruiz Diaz and Pinzon were one and the same person. Similarly, when all the so-called evidence is closely scrutinized, the numerous internal contradictions are clearly apparent and indicate the weakness of the theory. Holcomb comments that probably, in the years that followed the discovery of America, a number of misstatements were deliberately made by physicians and other authorities as part of a sales campaign for the introduction of Haitian Holy Wood or Guaiacum, supposedly a specific for the disease. Moreover, Holcomb notes that description of syphilis as given by Ruiz Diaz is not consistent with the disease as known now, nor was that writer certain whether it was a new disease, or Pliny's *Mentagra*.

The book is interesting but suffers from disorder in presentation and attempts at irony and sarcasm which miss the mark. These and other minor defects, that could have been avoided, detract somewhat from the value of this book, which otherwise demonstrates unusual scholarship and clear thinking.

THE PATIENT AND THE WEATHER. *Volume IV, Part I. Organic Disease. Cardio-Vascular-Renal Disease, including a Chapter on Experimental Endocarditis by Alexander J. Nedzel.*

By William F. Petersen and Margaret E. Milliken. Edward Bros., Ann Arbor, Mich. \$10.00. $10\frac{3}{4} \times 8\frac{1}{4}$; xxxiv + 663 + 19 plates; 1937.

Petersen continues to develop the Hippocratic thesis regarding the relationship between atmospheric changes and physiologic alterations (*cf.* reviews of earlier volumes in Q.R.B., Vol. 10, No. 3., Vol. 11, No. 2, Vol. 11, No. 4). In this volume he discusses especially the influence of meteorologic shifts on the onset and exacerbation of clinical episodes in cases of cardiovascular-renal diseases, although other conditions are also surveyed *en passant.* The evidence is furnished by clinical histories of patients more or less observed by the author as well as cases reported in the literature. Moreover the author presents statistics of death from cardiovascular-renal diseases, alleged to show that days having the most frequent deaths followed or were near periods of significant weather changes.

As has been noted in previous reviews, the evidence so far presented is not adequate to permit either agreement or disagreement with the author's viewpoint. The weather is in such a state of fluctuation that it is not difficult, to find periods in which, for example, some clinical condition does not precede, follow or coincide with a polar episode. Until definitive evidence is presented, one may only admire the author's eruditions, philosophical viewpoint and his broad general knowledge of physiology, pathology and clinical medicine, but deplore the lack of a refined analytic technique in this investigation.

ANLEITUNG ZUR KONSTITUTIONSDIAGNOSTIK BEI KINDLICHER TUBERKULOSE. (*An Hand von praktischen Beispielen.*) *Praktische Tuberkulose-Bücherei; Beihefte des deutschen Tuberkulose-Blattes, Heft 17.*

By Kurt Klare. Georg Thieme, Leipzig. RM. 4.80. $9\frac{5}{8} \times 6\frac{5}{8}$; 56; 1937 (paper).

Twenty years experience with childhood tuberculosis leads the author to conclude that this condition is essentially a manifestation of an innate constitutional anomaly. He notes that in most instances tuberculous children will also show the characteristics of a so-called exudative-lymphatic constitution. So he believes that when the disease appears in such children it should be regarded simply as another expression of their particular diathesis. In these cases prognosis is good. Instead, when tuberculosis appears in children who possess constitutional characteristics other than these, prognosis is only fair.

To illustrate his point, the author summarizes the clinical and laboratory observations made on 16 tuberculous children. Notwithstanding the apparent severity of the lesion in some cases, the children with an exudative-lymphatic constitution turn out rather well, while those with other constitutional characteristics do not.

The author's thesis agrees with the general theory advanced since the time of Hippocrates by physicians interested in the individual constitution. However, lack of a precise definition of the constitutional characteristics and of sufficient evidence does not permit definite conclusions on the subject.

SPORT UND ARBEITSSCHÄDEN. *Eine Zusammenfassung klinischer Beobachtungen und wissenschaftlicher Erkenntnisse zur Biologie der Arbeit und Pathologie der Funktion.*

By Wilhelm Baetzner. Georg Thieme, Leipzig. RM. 8 (paper); RM. 9.60 (cloth). $10\frac{3}{8} \times 7$; 136; 1936.

The author of this book is in somewhat of a dilemma to reconcile his findings with the national socialist movement and all the strain it puts on the young through enforced gymnastics, hikes, *Arbeitsdienst*, etc. So at short intervals through the book he is at pains to state that his findings are not a criticism of the youth movement in Germany, and that sensible exercise is always excellent for building good bodies. In general except for a short history of the gymnastics, the first section of the book is made up of truisms and generalizations. By far the greater part of the book is devoted to descriptions of

pathologic changes in the various organ systems. The author shows several examples from contestants in the 1936 Olympic games, particularly, of the bone changes in the knees, femur, tibia, and foot bones, arising out of jumping and sprinting. There are numerous X-ray pictures showing these effects.

On the whole the book is excellent, and contains a lot of useful information for the medical man. There is a bibliography and index.

Physiological Hygiene.

By Cleveland P. Hickman. Prentice-Hall Co., New York. \$3.25. 8 x $5\frac{1}{2}$; xxvi + 493; 1937.

This is another book devised to help in the teaching of hygiene and physiology to college students. The author's experiences convinced him that students want to understand the underlying reasons for "cultivating good health behavior. In other words, they want the physiological background of why they should form a particular habit." Doubtless to a large extent Hickman is right. Most of us would hate to put ourselves out and change our habits of life unless we were given good reasons for doing so.

It is hard to tell even after careful inspection of a book like this whether or not it has distinct advantages over other books which are already on the market. Our impression is that it is better than a good many. The illustrations showing the pelvis are not anatomically correct and in a later edition should be done over.

Dictionnaire des Bactéries Pathogènes *pour l'homme, les animaux et les plantes.*

By P. Hauduroy, G. Ehringer, A. Urbain, G. Guillot and J. Magrou. Masson et Cie., Paris. 120 francs (paper); 140 francs (bound). $9\frac{1}{2}$ x $6\frac{1}{4}$; 598; 1937.

In this work, the authors have made no attempt to give an exhaustive list of the pathogenic bacteria, but have given, in a brief and precise manner, some of the characteristics of the more common bacteria of plants and animals, as well as of man. Some 650 bacteria have been listed alphabetically by order, family, genera, and species, with a brief summary of the morphology, culture methods, biochemical and biological properties of each. The important literature concerning the different bacteria is listed in the foot-notes. The book is concluded with a number of tables describing the characteristics of bacteria, a complete index, and a 50-page list of *Ouvrages de Médecine Récents*. The volume will, no doubt, be of value to the medical student and to the student of bacteriology.

Entstehung und Biologische Bekämpfung Typischer Infektionskrankheiten. *Vorlesungen auf Grund der Ergebnisse experimenteller Untersuchungen. Erste Folge.*

By Richard Bieling. Johann Ambrosius Barth, Leipzig. RM. 6.60. $9\frac{1}{4}$ x $6\frac{1}{4}$; vi + 119; 1937 (paper).

This little book is a condensation of a lecture course given by the author. Three infectious diseases, diphtheria, pneumonia and poliomyelitis are discussed in detail. Some further publications by the same author covering other diseases will appear sometime during the next two years.

The purpose of the lectures was to portray as exactly as possible the biological picture or significance of each stage of the development of each of the diseases from the moment of the entering of the infecting agent into the body until the death of the individual or the death of the infecting organism. Active and passive immunity for each of the three diseases is discussed in detail. There is a detailed bibliography for each disease.

Handbuch der Biologischen Arbeitsmethoden. - *Lieferung 463. Gehörsinn, Hautsinne, Kraftsinn (Muskelsinne), Geschmack- und Geruchsinn, Statischer Sinn, Stimme und Sprache.* Containing following articles: *Die Herstellung eines praktisch vollkommen geräuschlosen Raumes*, by G. van Rijnberk; *Verfahren zur Prüfung der Leistungen des Geruchssinnes; Neue Verfahren zur Prüfung der Leistungen des Geschmacks-*

sinnes; Neue Verfahren zur Erforschung der Leistungen des Druck-, Temperatur-, Schmerz- und Kraftsinnes; all by Emil von Skramlik. *Urban und Schwarzenberg, Berlin.* RM. 9. 10 x 7; 160; 1937 (paper).

The first paper in this number described the construction of the new sound-proof room in the Physiology Laboratory at Amsterdam. This description is very thorough, including as it does the construction of screen doors to allow fresh air to enter but to keep out the mice, and of the equipment (telephone, buzzer, axe, etc.) for getting assistance when the experimenter has locked himself in or the doors stick. The other papers discuss sensory tests conducted at Jena. The last paper is not complete. All are illustrated and annotated.

AUTONOMIC NEURO-EFFECTOR SYSTEMS.

By Walter B. Cannon and Arturo Rosenblueth. The Macmillan Co., New York. $4.00. $8\frac{1}{2} \times 5\frac{5}{8}$; xiv + 229; 1937.

In the preface the authors state:

> Our central interest in writing this monograph has been to render an organized account of recently acquired evidence regarding the chemical step which intervenes between the nerve impulse and the effector in the functioning of the autonomic nervous system. A wider range of interest, however, is implied in the title, *Autonomic Neuro-Effector Systems.* This title was chosen because we have considered not only the processes in the junctional region but also those occurring in the effectors themselves.

The volume is illustrated, contains an excellent list of references and is indexed.

TOD UND TODESURSACHEN UNTER DEN BERLINER JUDEN.

By Franz Goldmann and Georg Wolff. Foreword by E. Seligmann. Reichsvertretung der Juden in Deutschland, Berlin-Charlottenburg. RM. 1. $9\frac{1}{4} \times 6\frac{1}{8}$; viii + 82; 1937 (paper).

This investigation of causes of death among Berlin Jews covers 14,083 cases during the years 1924–26 and 1932–34. These cases were compared with a group of Aryans for the same period and considerable differences were found. Infant mortality, tuberculosis, and to a lesser extent cancer and apoplexy showed much lower death rates among Jews than Aryans. On the other hand diabetes, circulatory diseases, particularly arteriosclerosis and to some extent suicide take a higher death toll among the Berlin Jews. Suicide has risen 50 per cent for the years 1932–34. There are twelve tables showing causes of death by different classifications, age groups, sex, etc.

MATERIA MEDICA, TOXICOLOGY AND PHARMACOGNOSY.

By William Mansfield. C. V. Mosby Co., St. Louis. $6.75. 9 x 6; 707; 1937.

This book is attractively gotten up and well illustrated with photographs. It is not designed for reading because it is so brief and schematic. It is a textbook and a work of reference. With few exceptions each drug is given but one page of description. At the end there are about 120 pages on the various poisons, the symptoms produced and the treatment indicated. The book is obviously designed for the training of druggists and medical students.

TRAVAUX PRATIQUES DE BACTÉRIOLOGIE.

By Henri Bonnet and Armand Nevot. Preface by Robert Debré. Masson et Cie., Paris. 38 francs. $7\frac{1}{8} \times 5\frac{1}{2}$; vi + 178 + 6 plates; 1936 (paper).

For its size this is the most promising looking and the best illustrated book on bacteriology that we have seen. There are many excellent colored plates showing stained bacteria, cultures, and the Wassermann and other reactions. Translated into English this little book would probably be much appreciated by American medical students.

HANDBUCH DER BIOLOGISCHEN ARBEITSMETHODEN. *Leiferung 462. Quantitative Stoffwechseluntersuchungen (Ergänzung zu Abt. IV, Teil 13).* Containing following articles: *Ein Gaswechselapparat für Kleintiere,* by K. Junkmann; *Methodik der thermoelektrischen Hauttemperaturmessung,* by H.

Pfleiderer and K. Büttner; *Elektrische Gaswechselschreibung an Tier und Mensch*, by H. Rein; *Neuere Methoden der Spirographie und Spirometrie*, by M. Schneider and W. Schoedel; *Neue Geräte für direkte und indirekte Kalorimetrie*, by W. Bothe and H. Wollschitt.

Urban und Schwarzenberg, Berlin. RM. 14. 10 x 7; 236; 1937 (paper).

Discussions of new methods for measuring and recording gaseous metabolism, skin temperature and body temperature. Detailed descriptions and illustrations of apparatus and examples of their application are presented.

BIOCHEMISTRY

La Température Critique du Sérum. *I Viscosité et Phénomènes Optiques. Actualités Scientifiques et Industrielles, 401. Biophysique Moléculaire, I.*

By P. Lecomte du Noüy. Hermann et Cie, Paris. 18 francs. 10 x 6½; 84; 1936 (paper).

La Température Critique du Sérum. *II Phénomènes Optiques et Phénomènes Ioniques. Actualités Scientifiques et Industrielles, 402. Biophysique Moléculaire, II.*

By P. Lecomte du Noüy. Hermann et Cie, Paris. 20 francs. 10 x 6½; 104; 1936 (paper).

La Température Critique du Sérum. *III Fixation d'Ether-Tension Interfaciale et Spectre d'Absorption Ultra-Violet. Actualités Scientifiques et Industrielles, 403. Biophysique Moléculaire, III.*

By P. Lecomte du Noüy. Hermann et Cie, Paris. 10 francs. 10 x 6½; 41; 1936 (paper).

Until the present time, the problems of blood and of blood serum have been mainly studied only in connection with the general physiological or pathological condition of an organism. It has been the attempt of the author to point out the value of the study of serum apart from physiology and pathology, and to encourage further investigation along this line.

In this series of three small volumes du Noüy has given us some new facts, based on experimentation and observation, which are intended to clear up some of the fundamental problems on the mechanism and reactions of immunity. By the application of certain statistical and mathematical principles, a norm for serum has been determined. A comparison of the characteristics of "normal" serum with the characteristics of similar sera at different temperatures has brought to light many interesting facts. The work marks a step forward in the field of immunology.

The Little Things in Life. *The Vitamins, Hormones, and Other Minute Essentials for Health.*

By Barnett Sure. D. Appleton-Century Co., New York. \$2.50. 8 x 5½; xi + 340; 1937.

This discussion of the rôle of vitamins, hormones and minerals in health and disease is one of a series of books intended to familiarize laymen with recent advances in medicine. Sure has abstracted a large mass of technical literature but, unfortunately, he has left out the details that a physician would like to know and he has put in too many research findings for most laymen to digest. It is far from certain that these sketchy notes on the very latest vitamin and hormone research will be any protection to the layman against the quackery that the author deplores.

Home economics students will be able to use it as a reference book advantageously. The best chapters deal with the rôle of vitamins during pregnancy and the nursing period, and with malnutrition in children. There is a convenient tabulation of the vitamin content (including vitamins E and G) of two hundred odd foodstuffs. Although the preface contains the promise that the book is written in "non-technical language" the author found it necessary to include a five page glossary of the technical terms he did use. There are both author and subject indexes.

La Coacervation et Son Importance en Biologie. *Tome I Généralites et Coacervats Complexes. Actualités Scientifiques et Industrielles, 397. Exposés de Biologie, VI.*

By H. G. B. de Jong. French version by N. I. Joukovsky and Jean P. E. Duclaux. Hermann et Cie, Paris. 12 francs. 10 x $6\frac{1}{2}$; 54; 1936 (paper).

La Coacervation et Son Importance en Biologie. *Tome II Coacervats Auto-Complexes. Actualités Scientifiques et Industrielles, 398. Exposés de Biologie, VII.*

By H. G. B. de Jong. French version by N. I. Joukovsky and Jean P. E. Duclaux. Hermann et Cie, Paris. 15 francs. 10 x $6\frac{1}{2}$; 66; 1936 (paper).

In 1929 the author coined the word *coazervation* for a phenomenon in colloidal systems which resembles the separation of phases. It is applied in cases where the disperse phase appears in a concentrated and colloid-rich fluid and in which the factors of capillary electric charge and solvation are involved. He distinguishes between simple coazervation in which only one type of colloidal particle is concerned, and complex wherein two or more kinds are involved. It is with the latter types that these two volumes are particularly devoted as these are the ones most often found in living matter. After a discussion of general principles, specific applications are described and the differences between synthetic models and the living cell brought out. This is an important, but very technical, contribution in a highly specialized branch of physical biology. The bibliography is confined to works of the author and his co-workers.

Action des Lipoïdes sur les Phénomènes de la Lyse. (*Saponine, Bactériophage, Rayons X*). *Actualités Scientifiques et Industrielles, 404. Biophysique Moléculaire, IV.*

By Baruch S. Levin. Hermann et Cie, Paris. 15 francs. 10 x $6\frac{1}{2}$; 83; 1936 (paper).

This is the report of a series of experiments made to determine the influence of lipoids in cytolysis and hemolysis produced by chemical and physical means. The experiments were conducted by the author and several collaborators and had for their main purpose to extend previous observations. In agreement with these, it was found that lecithin and cholesterin inhibited to some extent the cytolytic action of saponin and x-rays on paramecium. In other experiments, the presence of these lipoids increased resistance to the cytolytic action of bacteriophages. Experiments regarding hemolysis gave similar results. Here again, the presence of either lecithin or cholesterin reduced the lytic action of saponin and of x-rays. The results of these experiments seem to be definitive, but the experimental procedures are not described as completely as is desirable.

The Metabolism of Living Tissues.

By Eric Holmes. The University Press, Cambridge; The Macmillan Co., New York. \$2.25. $7\frac{1}{4}$ x 5; x + 235; 1937.

The author's purpose in writing this book is to present a survey, biological more than purely chemical, of the present range of knowledge in dynamic biochemistry. The volume is based on a great volume of sound experimentation and observation. The emphasis is on the activity of living tissues rather than on the substances which their actions transform. This is an unusual treatment of the subject, but one that reflects a modern tendency in the methods of research and teaching in biochemistry. The book has been prepared not only for biochemists, but for those who are beginning the study or are otherwise interested in recent scientific developments.

Stoff und Leben. *Bios, Band VI.*

By Hans Schmalfuss. Johann Ambrosius Barth, Leipzig. RM. 13.60. $9\frac{1}{4}$ x $6\frac{1}{4}$; xvi + 203; 1937 (paper).

This is a general treatise on the problems of biochemistry discussed from the biological viewpoint. In order to reach the attention of readers of various scientific disciplines and of various degrees of education, the author endeavors to write in a simple and generally intelligible style. To this end he shuns the use of technical scientific nomenclature, employing instead expressions of German coinage: e.g., *Stoffkunde* for *Chemie*, *Lebenskunde* for *Biologie*, *Erb* for *Gen*, *Ergänzungsnährstoff* for *Vitamin*, *Festwert* for *Konstante*, *Kerbtier* for

Insekt, *Regler* for *Katalysator*, *Wirkstoff* for *Hormon*, etc. etc. Thus the language may perhaps advance the *anschauliche Denken* for German readers, but for the foreign student it will make the perusal of the book more difficult. Fortunately the author furnishes a list of the employed *Verdeutschungen*. The work is annotated and supplied with illustrations, index of names and a topical index.

Traité de Chimie Organique. *Tome IV. Noyau Benzénique. Carbures Benzéniques. Dérivés Halogénés. Acides Sulféniques, Sulfiniques ou Séléniques. Acides Sulfoniques. Sulfones. Dérivés Nitrés. Nitrohalogénés. Nitrosés. Dérivés Nitrosulfiniques, Nitrosulfoniques, Nitrohalogénés. Groupe du Biphényle et Polyphényl Benzènes. Dérivés di-, tri- et Polyphénylés du Méthane et de l'Éthane. Polyaryl Propanes et Homologues Supérieurs. Carbures Polyaryliques non Saturés. Arylcyclohexanes Divers. Industrie des Chlorobenzènes et Chlorotoluènes. Pétroles. Industries Dérivées des Combustibles Solides.*

By P. Amagat, P. Baud, L. Bert, L. Boisselet, Cl. Duval, J. Doeuvre, R. Heilmann, G. Hugel, J. Lichtenberger, M. Louis, L. Piaux, T. Salomon, F. Swarts, E. Vellinger, H. Weiss. Published under the direction of V. Grignard and Paul Baud. Masson et Cie, Paris. 220 francs (paper); 240 francs (cloth); 10 x 6½; xxvii + 828; 1936.

This is the fourth volume of a comprehensive work on organic chemistry which when complete, will include fifteen volumes. The scope of this volume is as indicated in the subtitle. The preceding volumes have previously been noticed in these columns in Vol. 11: Volume I in Number 1, Volume II in Number 4, and Volume III in Number 2.

Out of The Test Tube. *Second Edition, Revised and Expanded.*

By Harry N. Holmes. Emerson Books, Inc., New York. $3.00. 9¼ x 6¼; 301; 1937.

Out of the Test Tube is a popular discussion of chemistry and of many of the chemical conquests which have profoundly influenced the life of the modern age and it quite definitely stands out as a leader among the books of popularized science. Dr. Holmes has not been content with merely calling attention to the many "wonders" which have been produced through the medium of this branch of science, but has given sufficient background so that the intelligent layman may gain an appreciation of the methods underlying chemical research and reasoning. The list of subjects given consideration in this new edition is a long one as is indicated by its four page index.

The book is recommended to all who have a keen interest in the affairs of life.

Chemie der Inkrete *und ihre wichtigsten Darstellungsmethoden. Zwanglose Abhandlungen aus dem Gebiete der Inneren Sekretion Band I.*

By Kurt Maurer. Johann Ambrosius Barth, Leipzig. RM. 7.20. 9¼ x 6¼; vi + 67; 1937 (paper).

This inaugural volume of a series of endocrinological works as related to medical practice and theory is a good, concise exposition of what is known of the chemistry of the internal secretions. The material includes descriptive material on the chemical structure and action, and directions for tests. It is annotated but not indexed.

Proceedings of the Society of Biological Chemists, (India). *Volume 1 (January 1936–November 1936).*

Society of Biological Chemists, Indian Institute of Science, Hebbal P. O., Bangalore. Re. 1 (postage extra). 8⅝ x 5½; ii + 45; 1936 (paper).

SEX

History of Modern Morals.

By Max Hodann. Translated by Stella Browne. William Heinemann (Medical Books), London. 12s. 6d. net. 8½ x 5½; xv + 338; 1937.

The title of this book is not a true indication of its contents. First of all, only sex taboos are considered, and secondly, it is essentially an apology for and a eulogy of sexology. In the first chapter, the author discusses the theory of evolution and the development of genetics, embryology and endocrinology. Here he notes that the public reaction against sexology is similar to that which occurred with respect to the theory of evolution. There follow chapters on homosexualism, prostitution and the campaign to eradicate syphilis, the development of sexology, birth control, legalized abortion, sex education, psychoanalysis, woman suffrage. For each subject, the author demonstrates the strength of the existing taboos and how biological reality is disregarded in order to enforce the mores.

It is a book shrewdly prepared to appeal to the emotions of intellectual liberals whose political ideas are slightly tinged with red. The battles of the sexologists, their objectives and the value of their work are overly dramatized and somewhat exaggerated. Each chapter is furnished with a fairly complete bibliography, and a list of the dates of occurrence of significant events for sexology. Notwithstanding its aim, which is that of propaganda, this publication is one of the best general discussions of the subject to appear in recent years.

Practical Birth Control by Nature's Method. *With an Appendix on How to Promote Conception.*

By George R. Scott. Cassell and Co., London. 3s. net. $7\frac{1}{2}$ x 5; 101; 1937.

The author relies chiefly on the work of Ogino and Knaus to fix the ovulation period between the twelfth to the sixteenth day, both inclusive, counting backwards in the menstrual cycle. After taking into account the probable duration of life of the sperm, and of the egg, tables are constructed indicating when intercourse must be avoided by women with regular menstrual cycles, the cycles ranging in the length from 21 to 44 days. Similar tables are included for women whose cycles are subject to irregularities. Blank charts for the recording of menstrual cycles have been added at the end. There is an index.

Woman's Prime of Life. *Making the Most of Maturity.*

By Isabel E. Hutton. Emerson Books, Inc., New York. $2.00. $7\frac{1}{2}$ x 5; 150; 1937.

This book treats the climacteric period of women in a sound way. Dr. Hutton has written especially for those women who have little general knowledge of the physiological processes of the female reproductive organs and who look with dread towards the trying period of the menopause. Much wise advice is given but the book is not intended to replace the guidance of the physician—rather to supplement it.

Wunder der Fortpflanzung. *Eine Einführung in das Wesen des Lebens für Jedermann.*

By Curt Thesing. Gustav Kiepenheuer, Berlin. RM. 6.00. $9\frac{1}{2}$ x $6\frac{1}{2}$; 371; 1936.

This is one of the better popular introductions to the processes of propagation and heredity. It is an interesting and clear presentation, based on the most authoritative recent investigations. The bibliography is short, including only the more general German works. The illustrations are well chosen and elucidative and there is an index.

BIOMETRY

Statistical Tables. *Their Structure and Use.*

By Helen M. Walker and Walter N. Durost. Bureau of Publications, Teachers College, Columbia University, New York. $1.60. 9 x $5\frac{3}{4}$; v + 76; 1936.

A First Course in Statistical Method. *Second Edition.*

By G. Irving Gavett. McGraw-Hill Book Co., New York. $3.50. 9 x 6; ix + 400; 1937.

These two books are intended for beginning students of the elements of statistics. The Walker-Durost treatise tells the reader

how to put together tables, with a degree of detail that passes the bounds of pedantry and wanders off into the precious. Most of what is sound and useful in the book is commonplace—taught in virtually all elementary courses, and embodied in most text books. When the book goes beyond this range the judgment of most veteran statisticians will differ from that of the authors. To take but a single example by way of illustration: on p. 66 the authors set up a table (XVII A), criticise it, and substitute in its place as preferable two tables (XVII B and C). To any normal person, gifted with the habits of simple, clear-cut, logical thinking table form A is enormously superior to forms B and C. B and C *look* more impressive because they obscure the facts with mathematical symbols. Furthermore the cost of setting forms B and C in type is greater than that for form A (and the cost difference would be still greater if the unnecessary vertical rules with which form A is cluttered were removed). Also the chances of errors in composition (with subsequent proof correction) are much greater in forms B and C than in A.

Prof. Gavett's excellent elementary text is improved in the second edition. The most annoying feature of it is the absence of bibliography, suggested supplementary reading, or acknowledgment to earlier workers who originally ploughed the same fields. The author's habit of appropriating ideas from other authors and dressing them up just enough differently so that a change of plagiarism technically will not lie, falls something short of that generosity that formerly existed in the field of scholarship but now seems rather demoded in a generally smarter but less honest world.

Mathematics for the Million.

By Lancelot Hogben. Illustrated by J. F. Horrabin. W. W. Norton and Co., New York. $3.75. $8\frac{1}{2} \times 5\frac{3}{4}$; 647; 1937.

Those of our readers who are interested in the history of elementary mathematics, and how its various branches got "started" will find a great deal of valuable information in this book. The answer to the question "what can you do with mathematics?" constitutes the principle theme. One is shown how to obtain the height of cliffs and the width of rivers, how to find the area of triangles, how to construct a star map, and so on—all unquestionably very useful information. The author, between examples, which are numerous and well illustrated, drags in, for no apparent reason, just about everything from the theory of organic evolution to a character out of the latest H. G. Wells novel.

Now let us consider the reader who has some intellectual interest in mathematics, and is more concerned in what it is "about" than in locating the position of a star, or how to find the width of a stream. The chapter "What the Calculus is About" will serve as an illustration. Here the author demonstrates that the first derivative of a plane curve is the slope of the tangent line, shows the relation of the second derivative to the first in terms of acceleration, gives the more simple rules of differentiation, explains the integral as the limit of a summation process, and then adds some elementary integrations. This takes 56 pages. Any introductory text in calculus covers the same ground, and requires considerably less space to do so. The long winded "simplifications" involving trains that run between London and Edinburgh do not, in our estimation, add anything in the way of clarity.

In reference to infinite series the term "convergence" is standard usage, and "d^2x" is always called the "second differential." But in the interests of clarity Professor Hogben thinks it well to introduce "choking off," and "the second dwarf x."

Short tables of sines, tangents, logarithms, and squares have been included. Exercises appear at the end of each chapter. There is no index.

Wie Statistische Zahlen Entstehen. *Die entscheidenden methodischen Vorgänge.*

By Franz Zizek. Hans Buske, Leipzig. RM. 4.50. $9\frac{1}{4} \times 6\frac{1}{4}$; iv + 151; 1937 (paper).

This is a rather general simple discussion of statistics for the student of statistics and also the layman who has to answer many questionnaires and often wonders what happens to his answers. The author considers his subject from three points of view for purposes of explanation. In the first place he deals with the individual phenomena or units that make up the statistical mass. In the second place he considers the purpose or goal for which the statistics are collected, and what conclusions can be drawn from the statistics. In the third place he discusses statistical methods. His methodology is limited to a practical discussion of the mean and standard deviation. No other constants are discussed, nor are any formulas given for derivation of these two constants. There is no bibliography or index.

Biologie Mathématique.

By V. A. Kostitzin. Preface by Vito Volterra. Armand Colin, Paris. 13 francs (paper); 15.50 francs (cloth). $6\frac{3}{4}$ x $4\frac{1}{2}$; 223; 1937.

This is a compact exposition of the methods used in the mathematical analysis of biologic phenomena. The well known methods proposed by Volterra, Lotka, Pearl, and Haldane for the study of the growth of organisms and populations under different environmental conditions are synthesized and welded into one coherent system. Well selected examples are presented to illustrate the application of analytic procedures to concrete problems.

Continuous Investigations into the Mortality of Assured Lives. *A 1924–29 Light and A 1924–29 Heavy. Mortality Functions and Monetary Tables.*

Published on behalf of The Institute of Actuaries and The Faculty of Actuaries in Scotland. Cambridge University Press, London. £2. 2s. $9\frac{3}{4}$ x 6; viii + 147; 1937.

Life tables, and auxiliary functions, of use to actuaries, based upon groups showing respectively low and high rates of mortality. The work has been done in the best traditions of the Institute of Actuaries in England. Earlier volumes in the series have been noted in these pages.

PSYCHOLOGY AND BEHAVIOR

Primitive Behavior. *An Introduction to the Social Sciences.*

By William I. Thomas. McGraw-Hill Book Co., New York. $5.00. $9\frac{1}{4}$ x 6; ix + 847; 1937.

This treatise presents a systematic survey of the patterns of human behavior as observed in primitive societies. The subject matter includes descriptions of characteristic attitudes and language forms, criterions of kinship and its related taboos, sexual behavior and ceremonies, forms of law and government. The three last chapters of the book are concerned with the author's ideas on culture patterns, illustrated by an outline of the development of Bantu culture, and with the perennial question of superior and inferior races.

In the exposition of the subject, the author ably illustrates his own viewpoint regarding the development of behavior patterns in human societies. These are that the theories which assume the existence of differences in mental and psychic characteristics among races or populations have not been proven. Instead, the author believes that diversities in behavior and culture patterns are due to different interpretations of experiences. Consequently, the theory of a uniform course of cultural and behavioral evolution cannot be accepted. Moreover, the author thinks that culture area rather than the physical environment should be emphasized.

> In their adjustive strivings territorially isolated groups develop, through their specific experiences, characteristic values and habits, some of them unique, and the circulation of these traits, their migrations from area to area, and the borrowing back and forth, represents a sort of social inheritance, and is perhaps the main basis of social change and of advance to the cultural level termed "civilization"

One of the outstanding characteristics of this book is that the interpretations of ob-

servations always demonstrate a realistic perspective of social phenomena. The literature has been thoroughly reviewed and a good portion of the text consists of excerpts from the sources cited. The bibliography covers 36 pages.

LABYRINTH UND UMWEG. *Ein Kapitel aus der Tierpsychologie.*
By J. A. Bierens de Haan. E. J. Brill, Leiden. 4. guilders. $9\frac{1}{4} \times 6\frac{1}{4}$; 231; 1937 (paper).

The aims of the author of this monograph are

(1) To introduce the reader by means of a historical and critical survey to the problems and the methods of maze and detour researches; (2) To give a summary of what has been attained with different animals in both fields, in which special attention has been given to the work done with the white rat in the maze; and (3) To try to combine the results of both the departments of research work and to find for them a common explanation, by conceiving them as a (primary or secondary) grasping of spatial relations with regard to a goal striven after by the animal.

The study opens with a discussion of Small's work and an elaborate description of different types of mazes. The detour work of Thorndike and later criticisms by Hobhouse are discussed. Then follows a survey of the work done on different animals including cats, dogs, non-domesticated carnivora, rats, the lower vertebrates, and invertebrates. Much of the work on the invertebrates seems dubious in the eyes of the author. The rest of the book is devoted to the learning of maze and detour by the white rat. The author sums this up and surveys it critically coming to the not very exciting conclusion that it will never be possible to formulate laws and rules that can predict accurately the behavior of any one rat in any particular maze. Only it can be said with certainty that finding the way in the maze is based primarily on a striving to reach a goal and is achieved through an acquired knowledge and idea of the apparatus and its structure and the path through it. The process seems to be similar to finding one's way about in a strange city. There is a detailed bibliography.

THE BIOLOGY OF HUMAN CONFLICT. *An Anatomy of Behavior, Individual and Social.*
By Trigant Burrow. The Macmillan Co., New York. \$3.50. $8\frac{1}{2} \times 5\frac{1}{2}$; xl + 435; 1937.

Neurosis, crime and insanity, the author believes, are the external signs of a universal "lack of coördination and peace that is primary and internal to man." This condition is perceptible within man when a non-mental (non-symbolic) method of observation is employed. It appears then that disorders of behavior arise from the conflict between "physiological tensions" due to the desire to maintain social security and integrity and those tensions caused by the necessity for social intercourse. In the realization of this fact lies the hope that human behavior disorders may be successfully treated and in the same manner as physical disorders. There is much to be said in favor of this theory which, however, is far from being completely original. But, a clear interpretation of the author's thoughts is not always possible due to his rather involved style and penchant for coining new words. It is also to be noted that in the preface and again in the concluding chapter the author states that this book is the outcome of an investigation of behavior problems conducted by and among the staff of the Lifwynn Foundation. In other places throughout the book this fact is mentioned, but nowhere is there a definite statement of the procedures employed in the investigations or of the actual results. Lacking these, the reader is unable to judge the significance of the author's conclusion.

A HISTORY OF PSYCHOLOGY IN AUTOBIOGRAPHY. *Volume III.*
By James R. Angell, Frederick C. Bartlett, Madison Bentley, Harvey A. Carr, Sante De Sanctis, Joseph Fröbes, O. Klemm, Karl Marbe, Charles S. Myers, E. W. Scripture, Edward L. Thorndike, John B. Watson, Wilhelm Wirth. Edited by Carl Murchison. Clark University Press, Worcester, Mass; Oxford University Press, London. \$5.00. 9×6; xvii + 327; 1936.

The purpose of this book is expressed in the introduction in the following manner:

> The author of a recent book of psychology found that it was impossible to get important facts concerning the scientific development of certain individuals except from those individuals themselves. Since a science separated from its history lacks direction and promises a future of uncertain importance, it is a matter of consequence to those who wish to understand psychology for those individuals who have greatly influenced contemporary psychology to put into print as much of their personal histories as bears on their professional careers.

The idea is a good one, and the volumes have a real and permanent value. But, as must surely have been evident to an editorial committee of psychologists different men do find widely varying degrees of difficulty in artistically stripping off their mental clothes, down to the nude, in a gold fish bowl placed in a show window on Main Street.

THE INTELLECTUAL FUNCTIONS OF THE FRONTAL LOBES. *A Study Based Upon Observation of a Man After Partial Bilateral Frontal Lobectomy.*

By Richard M. Brickner. The Macmillan Co., New York. \$3.50. $7\frac{3}{4} \times 5\frac{1}{4}$; xvi + 354; 1936.

In the course of two operations in August and September 1930, the patient, a man then 41 years of age, underwent a partial bilateral frontal lobectomy. Beginning in November 1931 efforts were made to re-educate him. Complete notes, and in many cases the patient's own words, were recorded by the various people participating in the re-education process. The author of the book arrives at no conclusions, but presents the material in such a way as to enable the reader to draw his own.

The book is divided into sections which include the patient's history prior to the operation, anatomical and surgical discussion of the operation, post-operative observations, symptoms in the intellectual sphere, psychological tests, and other similar useful information.

Since this is the only known case where the patient survived such an operation for several years and led "a more or less well adjusted life," this careful study should prove to be of great value to neurologists, psychiatrists, and psychologists. There is an extensive bibliography, an index of names, and an index of subjects.

A MIND MISLAID.

By Henry C. Brown. E. P. Dutton and Co., New York. \$2.00. $8 \times 5\frac{1}{2}$; 219; 1937.

Any man who regains his normal faculties after a period of mental disturbance and then writes a good honest report of his experience does a big service to medical science. Every physician, and particularly every physician who deals with the psychopathic and the insane, should read such a report with the greatest care because it can teach him much.

Mr. Brown's breakdown came late in life, perhaps partly as a result of overwork, but largely because of a tremendous disappointment and psychic shock. The book might have been even more helpful if the writer had given us a better picture of his mental processes during the period of agitated depression, but there are many things in the book which will interest not only the psychiatrist, but also the layman, be he normal or psychopathic. One of the last chapters of the book will be particularly helpful to the relatives of the insane whose feelings are often harrowed by the fear that their loved one is being brutally handled. Brown has only kind words to say about the way in which he was treated during his long stay in the asylum. The book is interesting and reads easily.

A PRIMER FOR CRITICS.

By George Boas. The Johns Hopkins Press, Baltimore. \$2.00. $8\frac{1}{4} \times 5\frac{1}{2}$; viii + 153; 1937.

A thoroughly intelligent discussion of the difficulties encountered, and the distinctions that must be made in analyzing one's reactions to a work of art, whether the critic is the spectator, or the artist himself. There are chapters on instrumental values in art, terminal values, schools of criticism, and the authority of criticism. There are no elaborate discussions of

aesthetics, nor is there any attempt to furnish the reader with any sort of a scale for evaluating emotional experiences. The reader need not be deceived by the word *primer* in the title, for most persons who are interested in the arts could derive some profit from reading the book. As supplementary reading *Practical Criticism*, by I. A. Richards, would serve very well.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

Atoms, Men and Stars. *A Survey of the Latest Developments of Physical Science and Their Relation to Life.*

By Rogers D. Rusk. Alfred A. Knopf, New York. \$3.00. $8\frac{3}{4}$ x $5\frac{3}{4}$; xviii + 289 + ix; 1937.

Within the present generation there have occurred three fundamental changes in scientific conceptions. The first of these was the theory of atomic structure associated chiefly with the names of Rutherford and Bohr. The second was Einstein's theory of relativity, and the third was the quantum theory of Planck.

The ramifications of these theories are legion, and it is perfectly possible to be a useful and altogether admirable citizen without being cognizant of all the implications that depend upon them, but it is not possible to consider as liberally educated anyone who does not know a little about what they mean and why the earlier theories which they have displaced had to give ground.

For this reason a great multiplicity of books has appeared within the past few years, by the reading of which the man in the street is supposed to acquire a knowledge of what it is all about. The present work is one of the best, if not the best, attempt to achieve this result that has as yet appeared. It is not in any sense a popularization or a simplification, but is a straightforward effort to present in lucid language the latest developments in physical science.

Modern scientific theory is essentially esoteric, and he who would become acquainted with it must reply to the question "Understandest thou what thou readest?" in the words of the Ethiopian eunuch of Queen Candace, "How can I, except some one shall guide me?" Just how efficient a guide the present work is may be demonstrated by considering some of the instances in which it is likely to prove helpful.

Everyone today concedes the existence of subatomic particles, commonly referred to as electrons. But the reader of this work will be informed that the electron is only one of five different types of particles, and that the existence of two others has been predicated upon reasonable evidence. Also everyone is familiar with Mendeléjeff's theorem that the chemical properties of the elements are periodic functions of their atomic weights, but that there is a group of elements called the "home for incurables" which have fractional atomic weights and which will not fit into the scheme. It now appears that such substances are isotopic, i.e., their atoms do not all have the same weight. A fractional weight indicates a mixture of isotopes; a pure isotope always has an integral atomic weight and fits into the periodic table just as Mendeléjeff thought it should.

The interested reader will also find an explanation of why the constancy of the velocity of light as observed by Michelson and Morley makes inevitable the conclusion that bodies in motion are shorter in the direction of their motion than when at rest, and that their mass is greater by virtue of their motion.

The quantum theory is merely the atomic theory applied to energy. The atom of energy is the quantum. Just as the elements differ in the value of their atomic weights, so do they also differ in the value of the quantum of the energy derived from their oxidization. The quantum is inversely proportional to the wave length of the light with which it is associated, and Planck's constant "h" is the product of the value of the quantum by its associated wave length. Like the atom, the quantum can be divided into subatomic particles, but these while they resemble energy in some ways and matter in others, cannot be considered to be either energy or matter. One is tempted to paraphrase Augustine of Hippo and call them the primordial substance from which

the universe of energy and matter has been created.

Finally, the closing chapter is a masterpiece of expression. It is philosophical without being metaphysical, and thus is a worthy culmination of the whole work.

The author is not infallible. Especially is his geography a trifle shaky, as when he locates the two hundred inch telescope on Mt. Wilson instead of an Palomar Mountain, or Boulder Dam in the state of Colorado instead of on the river of the same name. But trifles of this sort do not unmake great books.

GARDEN RUBBISH AND OTHER COUNTRY BUMPS.

By W. C. Sellar and R. J. Yeatman. Illustrated by S. Dowling, Footnotes by Will Cuppy. Farrar and Rinehart, New York. \$1.50. $7\frac{7}{8} \times 5\frac{1}{4}$; xvi + 146; 1937.

This is the type of book that seems destined to dissipate its saccharinity on the atmosphere of the uninhabited regions. In the first place it is a humorous book for gardners, a class in whom a sense of humor is conspicuously lacking, as is evidenced by the fact that they take themselves so seriously. Secondly, it is largely devoted to a discussion of beadles and cornucopias, both of which are somewhat mythical objects that have become practically extinct in the land of their origin, and are altogether unknown this side of the water. Finally, it is the output of English authors for American consumption, and between English and American humor there is fixed a great gulf.

But that is not half the trouble. The American can respect honest English humor even when he doesn't understand it. The present work exemplifies, not English humor but the English conception of American humor. As an example let us refer to the statement that Henry VIII's love for flowers was evidenced by the fact that one of his wives was named Anne Pollen. (This is, not so bad as it sounds. The English really do pronounce Boleyn as if it rhymed with pollen). The book abounds in similar instances, which constitute a sad commentary on the effect of our humorous authors on the Englishmen who have read them.

In the entire work perhaps the cleverest bit of humor, according to western standards, is a quotation heading one of the chapters:

> "A garden is a loathesome thing
> So what?"

The brilliance of this parody causes it to stand out like the shadow of a mighty rock within a weary land, or a shelter in the time of storm, or a wart on the nose, or what have you?

This peculiarly oligocene type of humor is likely to appeal not so much to the floriculturist as to the folkloriculturist, and not so much to the agriculturist as to the gagriculturist. If there are no such words as these there ought to be, else how could we adequately express the American conception of the English conception of American humor?

A chief merit of this work is that it has no index whatever.

ADVENTURES IN ERROR.

By Vilhjalmur Stefansson. Robert M. McBride and Co., New York. \$3.00. $8\frac{1}{2} \times 5\frac{3}{4}$; viii + 299; 1936.

The first two chapters are, with alterations from the author's book "The Standardization of Error," and the remainder is material from earlier magazine articles and public addresses. What differentiates Stefansson from practically all other debunkers is his expert knowledge (largely gained through personal experience) of the matters under discussion, and secondly, his notable talents for sardonic humor, exhibited at its best in Chapter 2, "The Pleasures of Buncombe."

The following is from the chapter on "Standardized Wolves:"

> *The Saturday Evening Post, Science Service*, and the Department of Agriculture of the Dominion of Canada, powerful and worthy of all respect and confidence, are in this relation spokesmen too for a popular and scientific multitude. The wolf-pack, then, is secure even as things stand. But conditions are bound to trend steadily in their favor. As living beasts, wolves are getting fewer with the colonization of the wilderness, but wolf-pack stories do not thereby get fewer . . . witness how they come again

and again from districts where wolves no longer exist. The fewer the living wolves the less the chance of their being so studied that evidence against the pack habit can be gathered. Finally there will be no wolves left, except in zoos. The belief in packs will have survived the means of refuting it. It will have become a truth.

H. L. Mencken's now famous article on the history of the bathtub in America is reprinted in full. There is no index.

ELEMENTARY PHOTOGRAPHY *for Club and Home Use.*

By C. B. Neblette, Frederick W. Brehm and Everett L. Priest. The Macmillan Co., New York. 72 cents. $7\frac{3}{4} \times 5\frac{1}{2}$; viii + 253; 1937.

Self-directed activity is the keynote to the plan of this book. The subject matter is arranged into five units which form a series of project studies whereby the student becomes acquainted with the fundamental principles of photography through simple illustrative experiments, and is initiated into the art of picture taking through channels in which he learns by doing. The subject matter covered in the first four units includes the making of a pinhole camera, picture taking with hand cameras and the making of negatives and prints. Unit five furnishes pointers for the selection and use of different types of film and filters and points out various factors to be considered for successful purposeful photography. Abundant photographs are used to illustrate points brought out in the text. The book is designed primarily for schools or clubs (of the junior or senior high school stage) but is sufficiently complete to make it suitable also for individual self-instruction.

THE MARCH OF SCIENCE. *A First Quinquennial Review* 1931–1935.

By Various Authors. Pitman Publishing Corp., New York; Sir Isaac Pitman and Sons, London. $1.25. $8\frac{1}{2} \times 5\frac{1}{2}$; viii + 215; 1937.

This report of the British Association for the Advancement of Science is the joint work of a dozen or so leading scientists of England to recapitulate the advances made in the various sciences during the past quinquennial period. The chapter on zoology is very comprehensive, but perhaps the best one is that on cosmic physics contributed by Sir James Jeans.

THE PRICES OF BIOLOGICAL BOOKS IN 1937

By RAYMOND PEARL and MAUD DeWITT PEARL

Department of Biology, School of Hygiene and Public Health, Johns Hopkins University

WHEN the QUARTERLY REVIEW OF BIOLOGY began publication in 1926 the custom was inaugurated of reporting at the end of each volume on the cost of the books that had been reviewed in its columns during the year. The present paper, therefore, is the twelfth of these reports on the cost of biological books. The prices of foreign books have been converted into dollars on the basis of the exchange at the time the books were received. Table 1 shows the findings for 1937, arranged in the customary manner.

TABLE 1

Prices of biological books, 1937

ORIGIN	TOTAL PAGES	TOTAL COST	PRICE PER PAGE
			cents
Germany	13,374	$260.91	1.95
British American	6,223	89.32	1.44
Great Britain	15,927	202.54	1.27
United States	86,398	805.93	0.93
Other Countries	3,588	30.68	0.86
France	9,087	77.09	0.85
British Government	1,277	4.32	0.34
U. S. Government	4,486	6.95	0.16
Totals and weighted average, 1937	140,360	1,477.74	1.053
Totals and weighted average, 1926–1936 incl.	1335,258	14,605.87	1.094

The total number of pages reviewed in 1937 is 140,360, an insignificant increase over 1936 but an increase of approximately 70 per cent over 1926, the year in which these tabulations began. In the twelve years of the QUARTERLY REVIEW's history the books reviewed in these columns have aggregated a total of 1,475,618 pages. To American buyers these cost in the aggregate a total of $16,083.61, leading to an average price per page for the total of 1.090 cents. The weighted average cost per page of 1.053 cents for all the books reviewed in 1937 is 3.7 per cent lower than that for all the books reviewed in our columns during the preceding eleven years 1926–36 inclusive, taken as a bulk total. It is lower than the corresponding average for 1936 of 1.147 cents per page by 8.2 per cent. The 1937 average price per page for all books reviewed is 4.0 per cent lower than the corresponding figure for 1926, which was 1.097 cents. The general picture presented by the 1937 summary is clearly of lowered prices. The American biologist buying books in 1937 got off relatively easily, as compared with other years, as judged by our review sample.

In 1937 Germany returned to her customary position at the head of Table 1, as the source of origin of highest prices for biological books. And, in general, all the sources of origin stood this year in about their usual order. It does not appear that the announced policy of the German publishers relative to book prices that has been discussed in these columns during the past two years has produced much of a realistic effect in lowering average prices, as yet.

Following the custom inaugurated last year Table 2 shows the price trends of books published in various countries during the decade from 1928 to 1937 and the absolute and relative changes in price from 1936 to 1937 and from 1928 to 1937.

The average prices per page of our samples of biological books from every origin except Germany and Great Britain were *lower* this year than in 1936, by amounts ranging from about 10 per cent for books commercially produced in the United States to 79 per cent for the British Government official publications. The 1937 average price per page for books commercially published in the United States was 18.4 per cent lower than that shown by our 1927 sample. The rise in the per page price of biological books published in France, which has been commented on in these notes in recent years, continued in 1937 and amounted to 19.0 per cent over 1936 prices and 88.8 per cent over those of a decade earlier. Our sample of biological books from British commercial publishers showed a 35.1 per cent advance in price over the 1936 sample. German books, as judged by our samples, increased in price in 1937 to American biologists, by 6.0 per cent over 1936, and by 31.8 per cent in the last decade.

Table 3 sums up the whole twelve years experience of the QUARTERLY REVIEW.

It is evident, from what has now grown to be a substantial sample, that during the past eleven years biological books from all over the world taken together have averaged to cost the American biologist very close to a cent a page, taking good, bad, and indifferent together. Furthermore it is plain that the sources of origin of these books fall into three fairly sharply defined groups relative to unit prices to the American buyer. In the first or rela-

TABLE 2

Comparison of the prices of biological books for the decade from 1928 to 1937

ORIGIN	AVERAGE PRICE PER PAGE										CHANGE + OR − FROM 1936 TO 1937		CHANGE + OR − FROM 1928 TO 1937	
	1928	1929	1930	1931	1932	1933	1934	1935	1936	1937	Absolute	Relative	Absolute	Relative
	cents	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*cents*	*per cent*	*cents*	*per cent*
British-American	1.46	1.90	1.91	2.27	1.48	1.29	1.45	1.53	1.81	1.44	−0.37	−20.4	−0.02	−1.3
Other countries	1.13*	1.68	0.97	1.53	1.02	0.85	0.86	1.20	2.26	0.86	−1.40	−61.9	−0.27	−23.9
Great Britain	1.09	1.29	1.13	1.19	0.89	0.66	0.96	0.84	0.94	1.27	+0.33	+35.1	+0.18	+16.5
United States	1.14	1.14	1.09	1.05	1.00	1.02	0.93	0.90	1.03	0.93	−0.10	−9.7	−0.21	−18.4
Germany	1.48	1.65	1.82	1.75	1.60	1.43	1.89	2.04	1.84	1.95	+0.11	+6.0	+0.47	+31.8
British Government	1.26	0.39	1.19	1.03	1.45	1.39	0.89	0.50	1.62	0.34	−1.28	−79.0	−0.92	−73.0
France	0.45	0.47	0.47	0.69	0.60	0.74	1.00	0.86	1.05	0.85	−0.20	−19.0	+0.40	+88.8
U. S. Government	0.21	0.23	0.30	0.28	0.36	0.17	0.18	0.11	0.21	0.16	−0.05	−23.8	−0.05	−23.8

* With two special treatises omitted as explained in Vol. 3, p. 601.

TABLE 3

Average biological book prices over the twelve year period, 1926–37 inclusive

ORIGIN	TOTAL PAGES	AVERAGE PRICE PER PAGE
		cents
British-American	102,415	1.606
Germany	164,943	1.587
Other Countries	49,761	1.327
United States	863,508	1.030
Great Britain	119,861	1.026
British Government	8,836	0.931
France	122,222	0.694
U. S. Government	44,072	0.235
Total and weighted average	1,475,618	1.090

tively high priced group fall books in the British-American, Germany, and "Other countries" categories of origin. The next or medium priced group includes the United States, British Government, and Great Britain (commercial publishers). The average per page cost of biological books has been very nearly the same for these origins. Finally the third or relatively low price group includes books published in France and by the U. S. Government.

The reader should bear in mind that these reports are based on small samples of books in general and, for some countries, on small samples of the biological books published. He should therefore be cautious in applying conclusions drawn from this material to the general domain of book prices.

INDEX

Date of Issue	Date of Issue	Date of Issue
5.8.57	22 SEP 1969	
21.3.61		
27.12.61		
27.6.61		
[illegible]		
17.2.62		
1.6.1962		
25.6.62		
18.7.62		
6/9/62		
3.4.63		
5.1.68		